THE SEARCH FOR ANCIENT DNA

IN THE MEDIA LIMELIGHT:

A CASE STUDY OF CELEBRITY SCIENCE

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DECLARATION

I, Elizabeth Jones, confirm that the work presented in this thesis is my own.
Where information has been derived from other sources,
I confirm that this has been indicated in the thesis.

DEDICATION

For My

Paleontology Professors
Mary Schweitzer and Gregory Erickson

Family
Dobsons and Joneses

Husband
Patrick Jones

In Memory Of

My Father-In-Law
Johnny Jones

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ABSTRACT

This is the first academic historical account of the search for DNA from ancient and extinct organisms and the first account of the celebrity science concept. The search for DNA from fossils surfaced from the interplay between paleontology, archeology, and molecular biology in the 1980s and has evolved from an emergent into a more established technoscience today. However, it has evolved under intense public interest and extreme media exposure, particularly as it coincided with and was catalyzed into the media spotlight by the book and movie *Jurassic Park* in the 1990s. Drawing on historical material and oral history interviews with over fifty scientists, I explore ancient DNA's disciplinary development and explain its relationship with the media, especially through examining its close connection to de-extinction, the idea of bringing back extinct species. As the discipline developed, researchers responded to its technoscientific challenges and status as a public-facing practice. Authentication of research results was a primary problem for scientists. Here, contamination concerns placed the practice's credibility on the line. However, celebrity was also a crucial component to ancient DNA's disciplinary development. While media mobilized the practice, it destabilized it, too. This thesis argues that the search for ancient DNA can be characterized as a history of a celebrity science. I argue that a celebrity science develops within a shared conceptual space of professional and popular interests. Media are crucial in the making of a celebrity science, pursuing the science and scientists for the news values. But researchers participate in this process, too, responding positively and negatively to the attention. Ultimately, a celebrity science is the outcome of prolonged publicity advanced by a relationship actively pursued and produced by both scientists and media members. Ancient DNA as a case study of celebrity science has implications for the process of science and science communication.

TABLE OF CONTENTS

TABLE OF FIGURES

INTRODUCTION

Science in the Spotlight

In 2015, researchers reported the investigation and identification of blood cells from 75-million-year-old dinosaur bones (Bertazzo et al. 2015). The paper was published in *Nature Communications* on June 9, 2015, the day that *Jurassic World* – the fourth film of the franchise – premiered in Hollywood ("Jurassic World (2015)" 2017). The film was released on June 12, 2015 to audiences across sixty countries, generating $550 million worldwide and taking the title of the fourth-highest-grossing-film in history ("Jurassic World (2015)" 2017). The timing of the recent research with its debut did not go unnoticed. *The Independent* heralded the headline: "Just in time for 'Jurassic World': scientists extract blood cells from 75-million-year-old dinosaur fossil" (Connor 2015). However, these scientists were not necessarily searching for original organics in dinosaur bones. Interestingly, the fossils did not indicate exceptional instances of preservation that might have made it possible, or would have at least made scientists think this level of preservation was possible. Instead, these fossils were fragments that had traveled from Canada to England nearly a century ago where they were tucked away at the Natural History Museum in London. They seemed to be little more than fossil fragments until practitioners from University College London and Imperial College London applied microscopic and molecular approaches to their investigations. There was no evidence for dinosaur DNA, the main motif driving Michael Crichton's and Steven Spielberg's *Jurassic Park*, but scientists saw what looked like evidence of protein preservation. They also saw what looked like the remnants of blood cells.

Two points are important. First, this study was not the first on the potential preservation of biomolecules or soft tissue structures in dinosaur fossils. There was earlier controversial evidence that pointed to this phenomenon, too. In 2005, Mary Schweitzer from North Carolina State University and coauthors published a paper in *Science* on evidence of what looked like blood vessels in fossil fragments from *Tyrannosaurus rex* (Schweitzer et al. 2005). In 2009, they published evidence of tissues, vessels, and proteins from *Brachylophosaurus canadensis* (Schweitzer et al. 2009). Schweitzer, as a doctoral student at Montana State University, had published similar studies as early as 1993 (Schweitzer 1993; Schweitzer, Cano, and Horner 1994; Schweitzer et al. 1997). These studies suggested that exceptional events had helped soft tissue structures persist over time. It seemed to be the exception rather than the rule. However, the 2015 study seemed to suggest that the best of the best fossils were not the only place to find original organics from the past. Rather, the preservation of molecules may be more common than conceived.

Second, this study was not the first to attract media attention or to be framed within the fame of a major blockbuster movie. This echoed an event reminiscent of the 1990s when scientists claimed to have extracted and sequenced DNA from a 120–135-million-year-old insect in amber. Their research was reported in *Nature* on June 10, 1993, the day after the first film in the *Jurassic Park* franchise premiered and the day before its release worldwide. In other words, the interplay between press and public interest in the search for biomolecules in fossils has followed this field closely. Since the 1950s, researchers have been promoting new perspectives in the area of molecular paleontology, and the study of biomolecules in fossils – which includes lipids, amino acids, proteins, and nucleic acids – has generated curiosity and controversy along the way. In this endeavor, the hunt for DNA from ancient and extinct organisms has served as the flagship of the fleet, eliciting extraordinary press and public attention, as well as cultivating scientific controversy in its attempt to answer old questions about evolutionary history through new technologies and techniques. Ancient DNA, since the 1980s, has captured public curiosity with its short but sensational history.[1] This thesis is about that history.

Research Questions

This is the first academic historical account of the search for DNA from ancient and extinct organisms as a technoscience.[2] It is also the first account of the celebrity science concept.[3] In this PhD project, I have asked and answered three research questions. My first research question asked: "How has ancient DNA research evolved from an emergent into an established technoscientific practice?" The primary point of this project was to trace the development of a discipline from its emergence in the 1980s to its status as a more or less established practice today. However, tracing this disciplinary development involved researching the relationship between the search for DNA from fossils and the intense press

[1] Ancient DNA refers to the process of extracting, sequencing, and analyzing degraded or damaged DNA from ancient and extinct organisms including plants, animals, humans, and bacteria. Ancient DNA can be preserved in skins, tissues, and bone if the bone is not a fully mineralized fossil. I use the terms "ancient DNA research," "the search for DNA from ancient and extinct organisms," or the "search for DNA from fossils" interchangeably to refer to the investigation of DNA from many different materials including museum skins, paleontological or archeological remains, and amber fossils.

[2] The term "technoscience" refers to the systematic relationship between scientific and technological practices that come together to form a research system that science scholars call a technoscience. In a technoscience, science and technology are intimately intertwined. See Brown, Rapport, and Webster (2000).

[3] The celebrity science concept is a new concept described in this introduction, throughout this thesis, and discussed in detail in the fourth and final chapter.

and public interest that has followed the field throughout its history. My second research question asked: "What is the extent of the interplay between the technoscience and the mass media, and has this interplay influenced the development of the discipline?"[4] I was specifically interested in the connections between the search for DNA from fossils and de-extinction, the idea of using DNA to bring back extinct species.[5] My third research question asked: "Why is ancient DNA research so closely connected to the rhetoric of resurrection?" Here, my objective was to discover when, how, and why a relationship between the two exists, and to determine in what ways it does or does not reflect research reality.

Thesis Argument

This is a new narrative of the search for DNA from ancient and extinct organisms. I recover information necessary – ideas, instruments, and individuals – for an integrated history of ancient DNA research, which, until now, is information neglected in standard reports and reviews. I establish a history of ancient DNA research that divides its development into three periods according to chronology and types of technology. As a result, three generations of researchers emerge, then evolve throughout the narrative. There are a number of histories that trace the disciplinary development of the sciences, but this history is different.

The search for DNA from ancient and extinct organisms surfaced from the interplay between paleontology, archeology, and molecular biology in the 1980s, and over the decades it developed into a discipline. However, I argue it developed under intense public interest and extreme media exposure, especially as it coincided with and was catalyzed by the book and movie *Jurassic Park* in the 1990s. Ancient DNA, since its start, occupied a special space between professional and public expectations. As the discipline developed in the media limelight, researchers responded to both its technoscientific challenges and its status as a public-facing practice. Authentication of research results was a primary

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[4] The term "mass media" refers to any medium used to communicate to a large number of individuals and institutions. Mediums include newspapers, magazines, books, radio, television, and the Internet. In discussing science and the mass media, the interactions between researchers, reporters, and the public play a predominant part in creating, communicating, and responding to new news in science and technology. I use the terms "mass media," "media," "popular press," and "press" interchangeably to refer to the process of reporting research to the public and its interpretation or reinterpretation across audiences. See Friedman, Dunwoody, and Rogers (1986) and Friedman, Dunwoody, and Rogers (1999).

[5] The term "de-extinction" refers to the process of recreating an organism that is a member of, or resembles a member of, an extinct species through back-breeding, cloning, genetic engineering, or reverse genetic engineering. I use the terms "de-extinction," "resurrection," and the idea of "bringing back extinct organisms" interchangeably. See Shapiro (2015).

problem for scientists. Here, contamination concerns placed the practice's credibility on the line. However, celebrity was also a crucial component to ancient DNA's disciplinary development. While media mobilized the practice, it destabilized it, too. Indeed, some scientists felt that media interest or influence was a second source of contamination that affected the credibility of the technoscience. Both celebrity and contamination concerns played a part in driving, even defining, the search for DNA from fossils as technoscientific practice. Overall, in writing this history, I have produced the first oral and material archive on the history of this technoscience.

The argument in this thesis is that the history of ancient DNA research can be characterized as a history of celebrity science. Here, I argue that a celebrity science is a subject of science, or in this instance a technoscience, that evolves within a shared conceptual space of professional, press, and public expectations that contribute to the shaping of the science. The mass media are critical in the making of a celebrity science because they seek the science and its scientists for its news values and potential to attract public attention. But press and public interest are not enough. Researchers participate in the process, too. The mass media are so influential that researchers respond, positively and negatively, to the attention and even reinvent the reputation of their technoscience accordingly. Ultimately, a celebrity science is the outcome of prolonged publicity advanced by a relationship that is actively pursued and produced by both scientists and members of the media. It is an active process. It is a dialectical process. It is a product of the ultimate integration of science, press, and public interests in a world of modern media, celebrity culture, and at a time when expectations in science and science communication were evolving. I suggest this concept as a model for other scholars interested in studying other sciences in the media limelight.

Research Methods

My research methods included traditional historical and archival research methods, but I also have the added approach of oral history interviews. I conducted, transcribed, and analyzed fifty-five interviews with scientists, as well as doctoral and postdoctoral researchers, involved in ancient DNA activity. These interviewees include researchers from disparate disciplines within evolutionary biology and can be characterized within the following categories: paleontology, archeology, anthropology, botany, epidemiology, evolutionary genetics, population genetics, molecular biology, microbiology, and computational biology. These interviewees work within the following countries: United

States, Canada, England, Ireland, Australia, Germany, Denmark, Sweden, Norway, France, Spain, and Israel. This selection is a sample of the population. Interviewees were not selected at random but by the professional and popular literature and via "snowball sampling." Fifty interviews were individual interviews; five interviews were group interviews with doctoral and postdoctoral researchers. Of the fifty individual interviews, ten were female and forty were male. Of the five group interviews, which included a total of seventeen doctoral and postdoctoral researchers, nine were female and eight were male. Of all fifty-five interviews, forty-one were conducted in person and fourteen online via Skype audio and video. These interviews were semi-structured in style and on average two-hours in length. The quotations were anonymized. Anonymity allowed for the candid stories and memories detailed in this history and for the professional or personal protection of individuals in the community. See appendix for a detailed methods discussion.[6]

Further, it is necessary to note that this history is based only on interviews from a practitioner perspective. Initially, I planned to interview thirty to forty individuals, half being scientists and half being members of the media who have played a part in reporting ancient DNA activity over the decades. Ancient DNA's history, however, was a history not previously written so in order to present a clearer and more comprehensive account of its disciplinary development within evolutionary biology, I decided to include interviews with as many of the scientists as possible. For this thesis, I decided to only interview the scientists and to use their stories in the writing of this history. It became apparent, based on information from these interviews, that the media played a larger role in this history than previously presumed. Consequently, this thesis has discussed the role of the media and scientists' relationship with the media from their perspective of it. It argues that the history of ancient DNA research can be characterized as a history of a celebrity science. It argues that a celebrity science is a process, the result of a relationship pursued by both scientists and members of the media. It should be noted this is then an asymmetrical account because of its intentional and methodological focus on the practitioners to the exclusion of interviews with the media.

Ancient DNA Considerations

The search for DNA from ancient and extinct organisms is an interdisciplinary practice and its history can be told from various disciplinary viewpoints. In this research

[6] See appendix for methods, interviewee questionnaire form, interviewee consent form, and ethics.

community, there are research sub-specialties. For example, a history of ancient DNA research can be told from a paleontological perspective or from an archeological or anthropological approach, highlighting the unique contributions, challenges, and controversies within each tradition. However, this thesis has approached this history from the broad background of evolutionary biology with the goal of finding overarching themes that affect the research community as a whole regardless of its sub-specialties. This decision to provide a broad backdrop of the technoscience presented two challenges. First, the ancient DNA community reflects a remarkable range of professional influences and interests. Therefore, this thesis is not an extensive or exhaustive account of all the major movements, citations, or conclusions that some scientists might be inclined to include. This was not the objective, and it was also not possible considering other constraints on the project's scale and scope. Instead, I tried to capture an overall picture of the technoscience's disciplinary development. Second, the ancient DNA community is a dynamic and diverse one in terms of personalities that have contributed to its colorful, even complicated, history. I have tried to represent the community's various viewpoints, including their disagreements. In light of these two challenges, I suggest that a serious strength of this thesis is that despite practitioner's sometimes steep professional or personal differences, there is one common theme running through accounts of their history. Interviewees all agree, though to differing degrees, on the role of the media as a direct or indirect influence on the development of the discipline. This is not the only theme, of course, but it is the theme that I explore in this thesis.

Celebrity Science Considerations

This empirical evidence has suggested an interesting theoretical idea that I call the celebrity science concept. Ancient DNA is a case study of celebrity science but is by no means an exclusive example of it. Three points are important. First, in my mind, the celebrity science concept is a positive phenomenon. However, I am aware that celebrity is a term that carries historical and sociological baggage. It denotes fame and fortune but can also carry negative connotations of superficiality. Indeed, the term can be used as a pejorative term. That is not how I use it or how I hope others will use it. In this history, celebrity is a story of the tension between the positive and negative consequences of life in the spotlight. In a celebrity science, researchers can choose to learn how to balance between the worlds of science and media with careful consideration of the expectations they embody, making a virtue out of this tension between science and the spotlight. Second, I am aware that the celebrity science concept runs the risk of being misinterpreted or reinterpreted across different audiences, and I am aware it may alienate some scientists for which this history was written. I hope it will not. Chapter Four outlines the celebrity science concept with attention to its definition, how and why it works, and its implications for understanding the process of science and science communication. Finally, I am aware that as a historian of science writing about celebrity science makes me a participant in the making of celebrity, too. In telling a narrative of ancient DNA research as a case study of celebrity science, I am reinforcing the celebrity spotlight which will likely affect the researchers working in or around this practice. Further, writing about celebrity science also makes me a product of it. There will be consequences from situating my work within a celebrity science context, and I appreciate the need to be reflexive regarding this issue.

Thesis Plan

This thesis has four substantial chapters. Chapters One, Two, and Three are arranged according to chronology and technology. Chronologically, each chapter is roughly divided by decades (1980s, 1990s, and 2000s). Technologically, each chapter is roughly represented by a type of technology (Sanger sequencing, polymerase chain reaction, and next-generation sequencing). In organizing this work in this way, for pragmatic reasons of outlining a narrative, three generations of researchers emerge and their relationships between each other evolve over the decades. Chapter Four is an analysis of the relationship between the search for DNA from fossils and de-extinction, and an argument for the celebrity science concept with implications for contemporary science communication. There is no traditional literature review in this thesis. Rather, literature in the form of ancient DNA reviews written by researchers and reporters are interspersed throughout the thesis. Literature in the history and sociology of science, as well as evidence for my argument that ancient DNA research can be characterized as a celebrity science, is embedded throughout the thesis and brought together in the fourth and final chapter.

Chapter One: This chapter focuses on the search for DNA from ancient and extinct organisms from the late 1970s to late 1980s and uncovers the origination and exploration of ideas that contributed to the construction of the new technoscience. It reveals that these ideas were pursued by different people in different places around the same time. It also reveals that from its beginning, the search for DNA from fossils was closely connected to the idea of resurrecting extinct species. My synthesis shows that the beginning of ancient DNA research is a story of the interplay between science, speculation, and spectacle. My analysis argues that these features were central to the emergence of ancient DNA research into a technoscience and a celebrity science.

Chapter Two: This chapter covers developments from the late 1980s to the late 1990s. It presents the testing and defining of limits as scientists searched for the first or the oldest DNA from paleontological, archeological, and botanical specimens with the new technology of the polymerase chain reaction. Technology accelerated this era of exploration, but professional and popular interest also coincided with, then was catalyzed by, the book and movie *Jurassic Park*. As the search for ancient DNA evolved, it did so under intense public interest and extreme media exposure. The press created opportunities for publicity which scientists could, and often did, take advantage of for the pragmatic purposes of obtaining further funding for research. Scientists fashioned their own opportunities for publicity, too. However, publicity could be problematic, especially in light of contamination concerns which challenged the credibility of technoscience within evolutionary biology. Here, some scientists viewed popular interest and influence was a second source of contamination. Overall, media both helped ancient DNA's disciplinary development, but according to practitioners it simultaneously hindered its acceptance as an authentic activity within evolutionary biology. I argue that this interaction between science and media contributed towards the co-construction of ancient DNA research into a technoscience and a celebrity science.

Chapter Three: This chapter covers the technoscience's disciplinary development from the turn of the century to today. It describes how a handful of practitioners produced a strict set of scientific standards for how to properly practice the search for DNA from fossils at a time when the technoscience's credibility was contested. In light of skepticism regarding ancient DNA authenticity, a group of researchers tried to standardize the practice via criteria of authenticity. Replication became a measure of experimental expertise and credibility. While initially intended to reduce controversy regarding authenticity, criteria effectively engendered controversy within the community. This chapter also details how these standards, designed around the technology of the polymerase chain reaction, were challenged by the innovation of a new technology called next-generation sequencing. Ultimately, criteria of authenticity were a response to contamination concerns, but it was also a response to celebrity. Scientists, in response to these concerns, engaged in boundar-work to try to demarcate credible from less credible research. This boundary-work is evident through researchers' activities, as well as their memories of their history. I argue that boundary-work, in response to contamination concerns and celebrity, was a crucial component to disciplinary development, influencing how scientists tried to make sense of the future of the field.

Chapter Four: This chapter outlines researchers' initial reactions to the idea of de-extinction to try to understand why, and in what ways, the search for DNA from fossils is linked to resurrection research. I argue that the link between the two is not necessarily a link made in the technoscience itself, but a link made through the media and legacy of *Jurassic Park*. However, scientists play a part in making this connection, too. They engage with de-extinction, or at least entertain press and public interest in it, not necessarily because it represents their research, but because they acknowledge the advantages of news values when communicating to public and political audiences for support. This chapter also outlines the celebrity science concept. I argue that a celebrity science is the outcome of prolonged publicity advanced by a relationship that is actively pursued and produced by both scientists and members of the media. This chapter argues that the search for ancient DNA in the media limelight is a case study of celebrity science with implications for contemporary science communication.

CHAPTER ONE

BEGINNING OF A CELEBRITY SCIENCE: SPECULATION AND SPECTACLE IN ANCIENT DNA

1.1 INTRODUCTION

This chapter is about the history of ancient DNA research from the late 1970s to late 1980s. It covers the origination and exploration of ideas that contributed to the emerging technoscience. Although this is the first academic historical account of ancient DNA's disciplinary development, there are other reviews and reports that outline its history.[7] Most cite a paper published in *Nature* in 1984, where researchers reported the discovery of DNA from an ancient and extinct quagga, as the beginning of ancient DNA's history. Most also cite Michael Crichton's 1990 book and Steven Spielberg's 1993 movie, *Jurassic Park,* as the beginning of its popularity. I tell a different story. In this history, I reveal the relationship between the search for DNA from fossils and the rhetoric of resurrection as it relates to the idea of bringing back extinct creatures like dinosaurs and mammoths. Stories about these ideas and the individuals who championed them have been told in one way or another, but they have never been told together in a way that captures their contribution to the disciplinary development of ancient DNA research. My synthesis of these stories is evidence that the beginning of ancient DNA research is a history of the interplay between science, speculation, and spectacle. I argue that this interplay was central to the emergence of ancient DNA research and its eventual evolution into a technoscience and celebrity science.

1.2 ORIGINATION

1.2.1 Introduction

This section is about the early history of ancient DNA research from the late 1970s to early 1980s. The prospect of procuring DNA from fossils was initially inspired by the

[7] Most narratives of ancient DNA's history have been written by journalists in the media or scientists who have documented their discipline in textbooks or books as well as professional and popular articles. See Jeffreys (1984), Cherfas (1991), Browne (1991), Brown and Brown (1992), Begley (1993), Herrmann and Hummel (1994), Lister (1994), Stoneking (1995), Schweitzer and Staedter (1997), DeSalle and Lindley (1997), Wayne et al. (1999), Hofreiter et al. (2001), Jones (2001), Hummel (2003), Pääbo et al. (2004), Willerslev and Cooper (2005), Shapiro and Hofreiter (2012), Hagelberg et al. (2015), Pääbo (2014), and Shapiro (2015).

preservation of insects in ancient amber.[8] This idea was formative in the foundation of a novel, and at the time controversial, technoscience. First, I argue that three ideas contributed to the emergence of the technoscience; one) the theoretical preservation of DNA in ancient and extinct organisms, two) the potential extraction of DNA from ancient and extinct organisms, and three) the hypothetical resurrection of extinct life from DNA in ancient material. Second, I argue that this search for DNA from fossils arose independently among four different groups of people from four distinct perspectives, which impacted the way they imagined and investigated these ideas. Third, I argue that speculation, especially about dinosaur resurrection, played a part in the emergence of ancient DNA research.

1.2.2 "Dinosaur Capsule"

In 1985, Charles Pellegrino, polymath scientist and futurist, published an article called "Dinosaur Capsule" with a recipe for dinosaur resurrection. In this article, Pellegrino speculated about an ancient past to one day be unlocked by paleontologists and technologies of the future. For Pellegrino, amber specimens – often whole organisms first trapped in sticky fluid tree resin and later encased in hardened amber fossil shells – were the ultimate means to rediscover and recreate the past. He imagined a time when someone, somewhere, would discover insects pristinely preserved in amber; insects that once lived during the days of the dinosaurs. Pellegrino proposed that "ancient bacteria on and in the flies may still be capable of reproducing themselves" and that in the "stomachs may be some undigested bits of their last meals, meals that came from animals, including dinosaurs that roamed the earth millions of years ago" (Pellegrino 1985a, 40). He speculated about the "genetic codes of creatures known only from bones and footprints" (Pellegrino 1985a, 40). Here, he suggested that if parts of the "genetic code" were "missing" that one could write the "lost 'paragraphs'" using genetic information from "currently living animals." He said that "everything that goes into building a dinosaur could be published in the form of chromosomes" to be inserted "into a cell nucleus" with "a yolk and an eggshell" to "hatch our own dinosaur" (Pellegrino 1985a, 40 and 114). For

[8] Ancient DNA, as mentioned in the first footnote in the introduction, can be preserved in skins, tissues, and bone if the bone is not a fully mineralized fossil. To be clear, a fully mineralized fossil is unlikely to preserve DNA, whereas a subfossil, a partially mineralized part of an organism, may retain remains of its cellular or molecular components. Generally, insects preserved in ancient amber, a type of fossilization, are often referred to as fossils although the organism trapped inside is not itself mineralized. Specifically, an organism's status as a fossil or subfossil, or whether it exists as a piece of skin or tissue, matters when considering whether cellular or molecular components may be preserved. Nonetheless, I use the terms "ancient DNA research," "the search for DNA from ancient and extinct organisms," or the "search for DNA from fossils" interchangeably throughout this thesis.

Pellegrino, "[a]ncient genes and bacteria embedded in amber may one day be used to re-create prehistoric animals" (Pellegrino 1985a, 38). It was a fantastical but at the time potentially practical recipe for bringing back a dinosaur.

Pellegrino's ideas in "Dinosaur Capsule" were inspired by two specific events. In 1977, fossil hunter Gerard Case introduced Pellegrino to a treasure trove of ancient amber, a site in the state of New Jersey dating to the Cretaceous. According to Pellegrino, two digs and two years later, they had recovered two 95-million-year-old flies preserved in amber. Shortly following his search for amber in the field, Pellegrino, then at the Smithsonian Institution in Washington, D. C., and entomologist Paul Wygodzinski at the American Museum of Natural History in New York, recalled finding something strange in the lab. Under the microscope, they found "mummified" insects in amber, "whose muscles and internal organs appeared to be preserved in microscopic detail" just "as if they have been alive the day before" (Pellegrino 1985a, 40). These events prompted Pellegrino to imagine the potential of amber for studying ancient and extinct organisms like dinosaurs (Pellegrino 1985a; Pellegrino 1995). Pellegrino speculated that if cellular structures could stand the test of time, then perhaps molecular components like DNA could, too. If so, it was a way of perhaps "bringing back dinosaurs" to "study them face to face" (Pellegrino 1985a, 114). Inspired by amber-embedded insects in the field and in the lab, Pellegrino actively advocated for these ideas about the theoretical preservation, potential extraction, and hypothetical resurrection of extinct life.

In the late 1970s and early 1980s, however, Pellegrino and his ideas faced criticism. Before the publication of "Dinosaur Capsule," Pellegrino discussed his ideas with colleagues, only to discover they thought them to be "too speculative," "totally bizarre," and even "downright crazy" (Pellegrino 1995, 69–70; Pellegrino 2014). While he said he experienced difficulty convincing his professional colleagues of his ideas, Pellegrino encountered even more difficulty publishing his ideas in professional journals. Pellegrino had corresponded with *Smithsonian Magazine* since 1981 about his amber article, but editors and reviewers alike thought the article was too much speculation with too little science. Editor John Wiley explained that even "legitimate speculation" would be difficult to defend because the magazine tends to be "conservative" and confronts enough challenges publishing the "conventional" (Wiley 1986). Pellegrino was disappointed by professional/popular journals, too. He recalled, "In the technical literature, in the late 1970s, the idea that even amber was preserving cellular material was considered 'too speculative.' Even the semi-technical/semi-popular journal 'Earth Science' […]

considered that […] 'too speculative.'" (Pellegrino 2014). Consequently, his ideas encountered repeated resistance from these communities. They were too speculative, too fantastical.

There were two reasons for this resistance. The first was theoretical. Theoretically, it was assumed that organic components like amino acids, proteins, and nucleic acids (RNA and DNA) did not preserve in the fossil record. Instead, when an organism died, it was assumed that organic components decayed over time due to chemical and environmental processes (Raup and Stanley 1971). Internal processes like autolysis contributed to the self-digestion or self-destruction of an organism's cellular and molecular features. Taphonomy also influenced degradation. For example, external factors like contact with water and high or low temperatures also contributed to the degradation of cells and molecules. With these processes combined, often little was left of an organism except its skeleton. However, some studies in the 1950s to 1970s challenged this assumption with evidence for the retention of amino acids and proteins in fossils (Abelson 1954; Abelson 1956; Erdman, Marlett, and Hanson 1956; Ho 1965; de Jong et al. 1974; Westbroek et al. 1979; Weiner 1980; Armstrong et al. 1983). Despite these studies that were promoting new perspectives in the area of molecular paleontology, there was still no evidence for DNA in fossils. This presented a problem for Pellegrino. Nonetheless, he was confident that amber, perhaps an exceptional environment for preservation, could protect an organism from chemical and environmental processes of degradation, preserving its DNA despite the test of time.

Second, Pellegrino and his ideas faced resistance for technical reasons. Even if DNA could be preserved under exceptional environments, the technology for extracting and sequencing what would likely be degraded DNA was insufficient. In the 1970s, new technologies and techniques in molecular biology offered new opportunities for researchers in the fields of evolutionary biology and those interested in biomedical and biotechnological research. For example, molecular cloning methods of the early 1970s allowed the isolation, replication, and manipulation of genes in ways not previously possible (Jackson, Symons, and Berg 1972; Cohen et al. 1973; Lobban and Kaiser 1973). But in the mid-to-late 1970s, the first method of DNA sequencing was developed (Sanger et al. 1977). This method, named Sanger sequencing after one of its developers, Frederick Sanger, became the main sequencing method for the next three decades. Sanger was awarded the Nobel Prize in Chemistry in 1980 for its invention (Nobel Media 2014). However, this method, while powerful, was largely limited to modern material where DNA occurs in larger amounts; not ancient material where DNA, if preserved, occurs in smaller, degraded, and damaged amounts. Consequently, the search and study of ancient DNA depended on technical improvements.

In 1985, technology for ancient DNA extraction, genome construction, and resurrection of extinct organisms was non-existent, and Pellegrino knew this. He said, “Three more decades of technological advance and we may be able to extract and read DNA from the flies’ stomachs, where, if we are lucky, we will find the blood and skin of dinosaurs” (Pellegrino 1985a, 40 and 114). Pellegrino imagined a future “on the verge of redefining the word *extinct*” so as a “precaution” and to “prevent deterioration,” he packed away the two 95-million-year-old flies in a refrigerator where they would “remain sealed for 20 years and perhaps more, while we wait for technology to catch up with them” (Pellegrino 1985a, 40 and 114). Pellegrino was not naïve to the necessity of technology, but he also did not let its absence obstruct his vision for what might be possible in the future. Nonetheless, professionals resisted his speculations. As one researcher remarked, “Let’s face it. In 1982-83, that was considered crazy. It was more of a career *breaker*, than a career *maker*” (19-01:30:30). Finding difficulty convincing his colleagues and publishing professionally, Pellegrino approached *Omni*, a futurist magazine. *Omni* welcomed his article on amber, and in 1985, “Dinosaur Capsule” was finally put into press and published.[9] However, the article received no response. Reflecting on this, one researcher recalled, “[…] [I]t was just silence out there” (19-01:23:00). For whatever reason, “Dinosaur Capsule” fell flat.

1.2.3 “A Brief History of the Extinct DNA Study Group”

Pellegrino was not the only one thinking about these ideas. In the summer of 1980, John Tkach, dermatologist and paleontology enthusiast, was riding his bicycle to work, pondering the extinction of the dinosaurs (Tkach 1993). Tkach was interested in immunology, fascinated by dinosaurs, and he, as well as others, wondered why dinosaurs, apart from the line leading to birds, went extinct nearly 65 million years ago. It was a life-long question in paleontology and while there were hypotheses about dinosaur evolution and extinction, Tkach found them unsatisfying. Indeed, among professionals there was a serious and sustained controversy over the causes of this mass extinction event (Glen 1994). As he rode to work, he recalled that “[w]ithin two blocks” he “had the answer.” For

[9] Pellegrino also published his recipe for dinosaur resurrection in two books. See Pellegrino (1985b) and Pellegrino and Stoff (1986). Science reporter and writer Roger Lewin credits Pellegrino as the first with the idea. See Lewin (1996).

Tkach, dinosaurs likely lacked a "bursa of Fabricius" – a vital organ in immune systems of modern birds. Tkach speculated that dinosaurs, lacking this organ, "could not cope with infections" and that "new pathogens must have come along that they could not defend against, and over millions of years, they died out" (Tkach 1993, 4). He had a hypothesis but no evidence. So, in the autumn of 1980, Tkach was "pondering the problem of where to look for dinosaur genes" and, again, during his ride to work, he had an idea. First, "[i]f a mosquito had fed on a dinosaur, it might have a dinosaur white blood cell with a diploid set of chromosomes in its stomach." Further, "[i]f such an insect had been preserved in amber, it might be possible to recover the chromosomes from that white blood cell, put them into an enucleated amphibian egg, and grow a dinosaur. […]" (Tkach 1993, 4–5). Tkach thought it "[u]nlikely, but theoretically possible" (Tkach 1993, 5).

Tkach shared this speculation with scientists in hopes of enlisting their efforts in the search for the preservation and extraction of DNA from ancient amber. In the process, he encountered a mix of pessimism and optimism. According to Tkach, he was passed along from scientist to scientist until he was directed to Allan Wilson at the University of California, Berkeley. According to Tkach, Wilson was enthusiastic but doubtful in their correspondence. No collaboration was formed on this occasion. Nonetheless, Tkach submitted his speculations to *Evolution*, where they were rejected because he did not provide evidence in support of his hypothesis. His research came to a close until the spring of 1982 when he read a paper published in *Science* by George Poinar and Roberta Hess announcing the exceptional preservation of cells in a 40-million-year-old insect in amber. As Tkach recalled, "The article by Poinar and Hess was pivotal. It suggests that the ideas I had been developing with the help of many other scientists were practical" (Tkach 1993, 10). He said, "In short, it was damned exciting" (Tkach 1983a, 3). According to Tkach, Poinar and Hess's work on cellular preservation in ancient amber, and principally its publication in a high-profile and prestigious journal like *Science*, was the empirical evidence he needed.[10] Tkach wrote Poinar in December 1982, and in January 1983 he received a reply. Poinar suggested they start a research group.

[10] The formation of the American Association for the Advancement of Science (AAAS) in the US in the mid 1800s has its origins as an academic association situated between professional and popular interests in science. In the late 1800s, AAAS adopted *Science* as its official outlet for publishing research. *Science*'s identity as a serious and successful journal struggled between pressure to popularize research while also meeting the scientific standards of professionalism from a growing group of researchers. After World War II, AAAS actively advocated for presenting scientific and technological research to the public, especially with regards to social or political issues. *Science*, as well as the UK's *Nature*, one of the most prestigious journals today, continues to cater to scientific standards while trying to reach the public through high-interest and high-impact popular science pieces. See Kohlstedt (1976) and Kohlstedt, Sokal, and Lewenstein (1999).

In 1983, the Extinct DNA Study Group was formed. By February, Tkach had written and distributed the first "Extinct DNA Newsletter" announcing that the goal of the group was the "recovery of genes from extinct organisms" and their "transcription" and "translation" through "recombinant DNA (plasmid) technology." The group was also concerned with the "study of the evolution of proteins based on amino-acid sequences of proteins recovered from extinct DNA," as well as the role of "bacteria," "fungi," "parasites," and "pathogens" in the "extinction of species." Yet the Extinct DNA Study Group was also interested in "culturing tissues from extinct life forms" and "[c]loning extinct life forms by the recovery of haploid or diploid sets of chromosomes" (Tkach 1983a, 1). The first newsletter was distributed to Poinar, Hess, and Wilson, and to several other scientists across the country. Overall, Tkach described the Extinct DNA Study Group as a group of "mature scientists with various extensive educations, travels, and a hard core molecular biology outlook" (Tkach 1993, 1–2). For Tkach and this group, the answers to paleontology and evolutionary biology would be found using molecular biology techniques and technologies.

Following the first newsletter came the first meeting of the Extinct DNA Study Group. The meeting was held on March 9, 1983, and hosted by Tkach at his home in Montana. The conversations centered on issues of contamination and authenticity. They also talked about terminology like "paleobiology," "paleogenetics," "paleoDNA," "paleogenes," "paleogenomes," and "amberization" (Tkach 1983b, 4).[11] During the discussion, Tkach discussed his idea for dinosaur resurrection and detailed it in the second newsletter. He suggested that someday someone might find "a mosquito that fed on a dinosaur" that had died and been "preserved in amber." He suggested, "If one could recover a white blood cell of a dinosaur from the stomach of a mosquito, he might be able to transplant it into an enucleated egg and grow dinosaur tissue culture or ultimately a dinosaur" (Tkach 1983b, 8–9). The group was "interested" but "felt it unlikely that DNA would survive sufficiently intact" for such a study (Tkach 1993, 13). As Poinar and Hess remembered, Tkach "corresponded with quite a few people about his idea of extracting dinosaur DNA from blood cells found in the stomachs of bloodsucking insects embedded in amber" (Poinar Jr

[11] The Extinct DNA Study Group used "paleobiology" as new terminology in the 1980s. However, by the mid-to-late 1970s, a new sub-discipline called paleobiology had actually already been established through efforts of several different scientists. From the 1950s to 1970s, a handful of paleontologists sought to reevaluate and reinvent the discipline of paleontology and its relation to geology, biology, and the modern evolutionary synthesis. Part of the process was a shift in methodology to computational and statistical studies of fossils as a way of rereading the fossil record. These new paleobiologists sought to enhance paleontology's scientific status by contributing to understandings of evolutionary patterns and processes. See Sepkoski and Ruse (2009), Turner (2011), and Sepkoski (2012).

and Poinar 1994, 91). As Poinar recalled, "Although his idea aroused interest, I'm not sure anyone believed it possible until he wrote to me" (Poinar Jr and Poinar 1994, 91). From such speculation, the Extinct DNA Study Group was the first forum to investigate ideas of the preservation and extraction of DNA in fossils and the resurrection of the prehistoric past.

Despite their enthusiasm, however, the group knew their ideas would elicit criticism. In 1983, Tkach began recruiting researchers to join the Extinct DNA Study Group but several scientists were skeptical to join. As Poinar and Hess recounted, "[F]ew were willing to jeopardize their careers or suffer the derision of colleagues, so the actual membership was limited to a courageous handful" (Poinar Jr and Poinar 1994, 92). However, even the "courageous" were cautious. Tkach recalled that "[a]ll members of the group feared for their reputations" because "[t]hey were concerned that premature release of information without adequate proof of authenticity" would "damage their reputations" (Tkach 1993, 14). The group "did not want to make any claims that would later be proven false" (Tkach 1993, 14). Tkach said, "We agreed to be careful about our claims until we felt we had done as much as we could to prove authenticity" (Tkach 1993, 14). Therefore, they actively avoided the press. According to the first newsletter, Tkach noted, "[…] Dr. Poinar has asked that we not discuss this work with the press […]. Adverse publicity could have a crushing effect on his work" (Tkach 1983a, 4). From a scientist's standpoint, speculation was a healthy part of the process of science. However, too much speculation with too little evidence, coupled with premature press, could damage the research and reputations of those involved. In other words, scientists saw hype as having positive or negative consequences. Here, timing and evidence was everything. But even with evidence, criticism was sure to ensue. Clair Folsome, one of the members of the group, said, "No matter what we do, it won't be beyond criticism" (Tkach 1983b, 7). Regardless, the Extinct DNA Study Group moved forward, marking the formal formation of a group interested in investigating DNA from fossils.

1.2.4 "The Quest for Life in Amber"

In 1980, entomologist George Poinar and electron microscopist Roberta Hess at University of California, Berkeley went to work for what they thought would be another average day in the lab (Poinar Jr and Poinar 1994). However, this day turned out to be different. Under the microscope, they said they were shocked to see the insides of a 40-million-year-old insect preserved in amber: "When we looked up at each other, the same thought was written across our faces – surely this fly's cell structure must be intact too! From then on, that mycetophilid fly became the center of our research project" (Poinar Jr and Poinar 1994, 64–65). Poinar, with a life-long love for amber, and Hess, with a set of specialized skills in microscopy, began to investigate this strange situation. First, they photographed the fossil for documentation, then sliced the specimen in two – one for the current study, another for future research. Next, they undertook the tedious process of sectioning the specimen for examination. Results were ready after weeks of work and the "routine cycle of ups and downs" (Poinar Jr and Poinar 1994, 68). In the end, Poinar and Hess found exceptional evidence for the organic preservation of a 40-million-year-old fly in Baltic amber. They saw "nuclei and organelle" and "entire muscle bands with easily identifiable components such as fibrils and mitochondria." They even saw that the "[t]racheoles, the breathing apparatus of insects, had intact linings, recognizable tubercles, and even possibly remnants of the plasma membrane" (Poinar Jr and Poinar 1994, 65). According to Poinar and Hess, it was a curious case of preservation from the prehistoric past.

In 1982, Poinar and Hess's paper was published in *Science*. With the aid of the electron microscope, they observed and described what they found to be "an extreme case of mummification" (Poinar and Hess 1982, 1241). For Poinar and Hess, it was evidence that resin was a resource for protecting the soft tissue structures of life even after death, but they wondered whether these results were replicable. Poinar wrote to a colleague, requesting a specific amber sample with the idea that sampling different sources could inform whether this form of mummification was a repeatable or rare occurrence. A week later, they received a 70–80-million-year-old wasp in Canadian amber which they subjected to the same procedures as the 40-million-year-old fly in Baltic amber. To their surprise and pleasure, this wasp in ancient amber also revealed evidence of its cellular structure. Poinar and Hess were confident that amber and its properties could be a reliable resource for preserving soft tissues structures of ancient and extinct organisms (Poinar Jr and Poinar 1994, 68). They wondered if amber might preserve DNA, too.

Poinar and Hess's realization that amber was a special source of cellular preservation caused Poinar and Hess to wonder if it could be a source of molecular preservation, too. According to Poinar and Hess, their paper "represented a pivotal point" in their "research plans." They thought, "If tissues could be discovered so well preserved in amber-embedded insects 40 million years old, what else could be found?" (Poinar Jr and Poinar 1994, 68). They thought, "What about nucleic acids?" (Poinar Jr and Poinar 1994, 69). The amber fly and wasp studies convinced them that amber was a special substance, and that if DNA preservation was possible, it would be so through the preservational properties of amber. Although they did not publicly suggest the theoretical preservation or potential extraction of DNA from amber, they did privately speculate about it. They knew that theoretical and technical constraints would potentially prohibit further investigation of these unconventional ideas. According to Poinar and Hess, they came to this conclusion before they came in contact with Tkach. In 1982, they assumed they were alone in the search for cells and molecules in ancient amber. However, their paper, showcased in *Science,* a well-respected and well-circulated journal, placed it in a position to attract attention. Indeed, it did. Their intellectual isolation transformed into a collaboration when Tkach contacted them and organized the Extinct DNA Study Group (Poinar Jr and Poinar 1994, 69). As Poinar and Hess recalled, "It's amazing when you consider it, how people can arrive at the same idea (extracting DNA from amber insects) from different perspectives" (Poinar Jr and Poinar 1994, 91). But their paper also attracted attention from other individuals. Perhaps one of the most interesting and influential was someone named Michael Crichton.

In the early 1980s, Michael Crichton, doctor turned science fiction writer, was writing a script for a screenplay about a genetically engineered dinosaur. But shortly after starting he "put the project aside." According to Crichton, there was an "enormous mania about dinosaurs" and he "did not want the book to appear to ride a current fashion." Indeed, starting in the 1970s and moving into the 1980s, there was a rapid rise in professional and popular interest in dinosaurs, how they lived, and how they died. This "Dinosaur Renaissance" was stimulated by research that questioned traditional views of dinosaur anatomy, physiology, evolution, and extinction.[12] However, Crichton realized that this renaissance was far from fleeting. According to Crichton, "the fashion never went away." Rather, "the fascination with dinosaurs was permanent" (The Official Site of Michael Crichton 2015; Shay and Duncan, 1993, 3). So, Crichton decided to continue the project. Poinar and Hess's publication in *Science* and Tkach's formation of the Extinct DNA Study Group conveniently coincided with the development of Crichton's science fiction story

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[12] Beginning in the 1970s, there were a series of scientists who proposed different and dramatic hypotheses about dinosaur evolution and extinction. Some, for example, proposed that dinosaurs were hot, not cold blooded, creatures. See Bakker (1975), Desmond (1975), and Bakker (1986). Others argued that birds were the direct descendants of one lineage of non-avian dinosaurs. See Ostrom (1974). There were also new fossil finds that exhibited evidence that parents practiced brooding behavior. See Horner and Makela (1979). Finally, in the 1980s, a novel and controversial theory about dinosaurian extinction was proposed. See Alvarez et al. (1980). This revival of interest among the press and public can also be attributed to a long historical line of fascination with and popularization of paleontology. See Rudwick (1985), Rainger (1991), Bowler (2009), and Brinkman (2010).

about dinosaurs brought back from the past. According to a recent media report, George Poinar told Bryan Curtis, a writer and blogger, that Crichton visited Berkeley in 1983. Poinar said that during their discussion, Crichton asked about amber but never revealed dinosaur resurrection as an interest (Curtis 2011; Curtis 2013).[13] Afterwards, Poinar and Hess continued their collaboration with the Extinct DNA Study Group, not thinking of Crichton again until nearly ten years later.

According to Poinar, in the early 1990s, he received a phone call from Universal Studios. The caller informed him that he, his now wife Roberta, and the Extinct DNA Study Group were acknowledged in the back of a new book called *Jurassic Park* (Poinar Jr and Poinar 1994, 153). Written by Crichton, *Jurassic Park* was a science fiction story about the resurrection of dinosaurs from DNA preserved in and extracted from insects in ancient amber, and the book was now a major movie in the making under the direction of Steven Spielberg (Shay and Duncan 1993, 6–8). In the book, Crichton acknowledged the ideas and individuals that inspired his story with Poinar, Hess, and the Extinct DNA Study Group as clear contributors (Crichton 1991a). However, it is not clear what role was played by Pellegrino's "Dinosaur Capsule" (Crichton 1991b). In one version of the acknowledgements, Crichton explained that "[c]ertain ideas" about "paleo-DNA" were "first articulated by George O. Poinar, Jr., and Roberta Hess, who formed the Extinct DNA Study Group at Berkeley" (Crichton 1991a). However, in another paperback version published the same year, the acknowledgements read slightly differently, explaining that "[c]ertain ideas" were "first articulated by Charles Pellegrino" and "based on the research by George O. Poinar, Jr., and Roberta Hess, who formed the Extinct DNA Study Group at Berkeley" (Crichton 1991b). It is unclear based on documentation if Crichton was aware of Pellegrino's or Tkach's specific speculations about dinosaur resurrection as publicly published in "Dinosaur Capsule" or privately discussed in the "Extinct DNA Newsletter." If he was, it seems he was only aware of them, particularly Pellegrino's idea, after he had published *Jurassic Park*. This realization after publication could explain why Crichton's acknowledgments from one book version to another differ, at first excluding then including Pellegrino. What is clear, however, is that ideas about the preservation and extraction of DNA from fossils, and the hypothetical resurrection of extinct organisms,

[13] Curtis interviewed Poinar and reported it in a blog. See (Curtis 2011). Curtis also obtained and published a letter from Crichton to Poinar acknowledging his research but not necessarily the idea of dinosaur resurrection from insects in amber as an inspiration for his book *Jurassic Park*. See (Curtis 2013).

arose among different people in different places around the same time.[14]

1.2.5 Conclusion

In this section, I argued that in the late 1970s and early 1980s there were three ideas that inspired the emergence of the technoscience; one) the theoretical preservation of DNA from fossils, two) the potential extraction of DNA from fossils, and three) the hypothetical resurrection of extinct life from DNA in ancient material. I also argued that these ideas arose independently among four distinct groups of people from four distinct perspectives. Pellegrino, a scientist and futurist, approached these ideas as a visionary. Tkach, a science enthusiast, was similarly speculative but successful in enlisting experts to investigate his hypothesis. Poinar and Hess arrived at the idea through more conventional means in the lab and publication of research results in the high-profile and highly-publicized journal *Science*. Pellegrino's "Dinosaur Capsule," Tkach's organization of the Extinct DNA Study Group, and Poinar and Hess's publication in *Science* were events connected to the beginning of ancient DNA research. Crichton's imaginative work-in-progress was also a part of this process, but one that would play more prominently in the next decade and next chapter. Finally, I demonstrated that these ideas, while speculative, were influential in the emergence of ancient DNA research. Pellegrino's ideas were incredibly imaginative. Indeed, they were so speculative that they were rejected by the professional practitioners. Tkach, like Pellegrino, entertained imaginative ideas about DNA from fossils, too. Interestingly, unlike Pellegrino, his attempts to attract scientists like Poinar and Hess were well received and resulted in the Extinct DNA Study Group. This was critical because it brought in researchers and resources to explore the existence of DNA in insects preserved in ancient amber which could potentially provide evidence for the hypothesis. In other words, speculation generated interest and guided activity in an innovative but seemingly impossible endeavor.

[14] In the 1990s, Pellegrino and Poinar engaged in a lengthy legal battle following Crichton's publication (1990) and Spielberg's production (1993) of *Jurassic Park*. Controversy ensued between the two over who had priority to the recipe for dinosaur resurrection as the idea that inspired *Jurassic Park*. Pellegrino and Poinar were contemporary colleagues, but their relationship was complicated. In the early 1980s, Pellegrino and Poinar were aware of each other. While Pellegrino discusses Poinar in his memoirs, Poinar actively avoids mentioning Pellegrino in his own. See Pellegrino (1985a) and Pellegrino (1995), as well as Poinar and Poinar (1994). Further, a letter from the editor of the *Smithsonian Magazine* to Pellegrino discloses Poinar's rejection of Pellegrino's ideas of dinosaur resurrection. See Wiley (1986). However, Poinar later took credit for the idea and claimed it inspired *Jurassic Park*. See Browne (1991).

1.3 EXPLORATION

1.3.1 Introduction

This section is about the exploration of ancient DNA research from the early to late 1980s. The emerging practice of ancient DNA research, initially inspired by the microstructural preservation of insects in ancient amber, eventually evolved into the investigation of DNA from museum specimens and human remains. First, I argue that ideas about DNA from ancient and extinct organisms continued to surface among different people in different places, but that it was scientists' ability to turn these ideas into experiments and evidence for that was crucial to the technoscience's disciplinary development. Second, I argue that spectacle, the prospect of DNA in fossils, played a part in the emergence of ancient DNA research as a series of studies tested, then affirmed the anomaly of DNA in material hundreds to thousands of years old. Third, I argue that spectacle encouraged speculation, particularly among the press, about the resurrection of extinct creatures such as dinosaurs and mammoths.

1.3.2 "Blueprint of a Lost Animal"

Poinar and Hess's publication in *Science* attracted attention from many, including Allan Wilson, a molecular and evolutionary biologist at the University of California, Berkeley. Although their study did not provide evidence of molecular structures, it did provide empirical evidence for the preservation of cellular structures in ancient amber. The ability to access and analyze the evolutionary history at the cellular and molecular level could be profound for paleontology, archeology, and paleobotany. This appealed to scientists as a way to introduce experimentation into traditionally observation and description based disciplines.[15] Whether Wilson contacted Poinar, or whether Poinar contacted Wilson, is uncertain (Taylor 1980; Poinar and Poinar 1994, 72). What is certain, however, is that in

[15] In the 1900s, the rise of experimental biology changed the way some scientists approached the study of the life sciences. In a way, it introduced a tension within the life sciences as a younger generation of biologists turned to the physical sciences with an interest to incorporate arguably more accurate methods, like experimentation, into the observation and description based discipline of biology. See Allen (1975) and Maienschein (1991). Disciplines like paleontology were part of this observation and description based tradition within the life sciences. Consequently, as the life sciences turned experimental, paleontologists faced pressures to remain relevant, especially in their contributions to understanding evolutionary patterns and processes. See Rainger (1991). The rise of paleobiology in the 1950s to 1970s was a response to this tension. See Briggs and Crowther (1990), Sepkoski and Ruse (2009), Turner (2011), and Sepkoski (2012). The rise of molecular biology methods in the mid 1900s offered new opportunities for studying the past on a molecular level. See Runnegar (1986). Specifically, the recovery of molecules in fossils, like DNA, could add an experimental element to paleontological practices. Here, molecular evidence, in combination with morphological evidence, could allow scientists to directly determine evolution in action. See Dobson (2012). Also see Dietrich (1998), Hagen (1999), and Suárez-Díaz and Anaya-Muñoz (2008).

1983 Wilson's lab and Poinar's lab, both at the University of California, Berkeley, established a collaboration (National Science Foundation, 1984).

In 1983, Poinar and Hess, together with Russell Higuchi, specialist in molecular biology and postdoctoral researcher working with Wilson, embarked on the first experiment to test ideas about the preservation and extraction of DNA from insects in ancient amber. The challenge was apparent as preparation alone was a tedious task. First, Poinar began by selecting specimens that would potentially offer optimal preservation of DNA. Next, all the equipment used in the experiment was sterilized to try to avoid contamination. For Higuchi, it was incredibly important to be clean. Even as early as the 1980s, he realized the risk of contamination with modern DNA; a problem that would plague the search for ancient DNA as it evolved over the years. Therefore, measures were taken to minimize contamination and maximize the amount of authentic ancient DNA that could be recovered. Meanwhile, Hess started the tedious task of sectioning samples for extraction, and removing the tiny tissues from the insects in amber. Finally, they performed a template assay test on the tissues; an experiment that would make a radioactive copy and give a radioactive signal in the presence of even the most miniscule amounts of DNA. If DNA was present it would be copied and a signal would be emitted, but if no DNA was present, then no signal would be produced. In the end, only two out of seven specimens – a moth and a fly – revealed alleged DNA ("Molecular Paleontology: Search for Fossil DNA" 1984, 12–13; Poinar Jr, Poinar, and Cano 1994, 93–95). One issue seemed solved – how to get DNA – but another issue emerged, namely, how to determine whose DNA it was. Was it authentic ancient DNA from the insect or a contaminant? As one researcher recounted, "[…] [I]n fact, we got a little bit of what we call incorporation, and it was a sign that there was DNA there, but it could not tell you whose DNA it was" (26-00:32:10). At the time, no extra experiments were done to determine the authenticity of the DNA, and the research was never publicly published ("Molecular Paleontology: Search for Fossil DNA" 1984, 12–13; Poinar Jr, Poinar, and Cano 1994, 93–94; Poinar Jr and Poinar 1994, 73–75). Instead, Higuchi and Wilson redirected their research.

Higuchi and Wilson turned their thoughts from insects in amber to the quagga, thinking that extracting and sequencing DNA from material hundreds of years old may prove more promising than material millions of years old. However, one researcher remembered that even then it was "clearly a gamble" (35-00:25:00). *Equus quagga* was a curious creature and a preferred pick for several reasons. The front half of its body, along its head and neck, was decorated with brown and white stripes, but beyond its neck the back of its body was solid brown, making it look like a cross between a zebra and horse. In the 1800s, humans hunted it to extinction, leaving scientists with questions about its evolutionary history and leaving others with a strong sense of guilt about the role humans played in its demise as a species. Taxidermist Reinhold Rau, in particular, felt this guilt and turned it into a moral mission to bring back the quagga ("The Quagga Project" 2016). In the 1980s, Rau was responsible for obtaining bits and pieces of a 140-year-old quagga from the Natural History Museum in Mainz, Germany for biochemical research. The samples were delivered to Oliver Ryder at the San Diego Zoo, then shared with Wilson at Berkeley (*New Scientist* 1984, 21). One researcher recalled, "Allan […] asked for some of the samples from Oli and they showed up in the lab in a plastic bag and they looked kind of like potato chips, and that's what we had to work with" (26-00:39:00). While the quagga was a sentimental specimen, it also presented an interesting phylogenetic problem that could be answered with molecular data. At the time, paleontologists who used morphological data were in disagreement over its evolutionary history. Some argued the quagga was more closely related to the zebra while others argued it was more related to the horse (Higuchi et al. 1984, 284). Although sentimental and conceptual arguments for choosing the quagga were important, it was, for Wilson and his lab, a technical task that could be a stepping stone to travel back in time. As one scientist said, "[…] [I]t was something to try to apply the technology to and to just prove the principle that cloning […] was efficient enough to recover the amount of sequences you need to answer this particular question" (26-00:39:00). Researchers hoped the quagga would be a step in this direction, and in retrospect it was. In the spring of 1984, they recovered short but informative sequences of mitochondrial DNA (mtDNA) that they confirmed as authentic to the quagga. It was the first recovery of DNA from an ancient and extinct organism, and it was a serious source of curiosity for both researchers and reporters.

In the autumn of 1984, Wilson and his lab submitted the first formal proposal to search for DNA in ancient and extinct organisms. The proposal, submitted to the National Science Foundation (NSF) and titled "Molecular Paleontology: Search for Fossil DNA," requested $330,000 to be distributed over a two-year-period to search for DNA in the quagga, bison, moa, mammoth, and insects in ancient amber. Research from the quagga, shared that summer at a conference, formed the foundation for the grant. It was both the preliminary and primary evidence on which the proposal stood. For the lab, the "successful recovery, cloning, and sequencing of DNA" from "the quagga" was the reason and rationale for "exploring the feasibility of recovering and cloning DNA from bones and teeth" and "the possibility of extracting and cloning insect DNA from specimens preserved in amber" ("Molecular Paleontology: Search for Fossil DNA" 1984, 2). There was professional interest in the project, and Wilson cited evidence that he had a team ready to engage in the endeavor. Researchers included Higuchi, Poinar, Hess, electron microscopist Alice Taylor, and graduate student Barbara Bowman. Wilson also cited evidence for popular interest in the search for DNA from fossils. In June 1984, after the presentation but before official publication of the quagga study, *Newsweek* published a piece speculating about scientists' ability to use the quagga DNA to not just reveal its evolutionary history, but to recreate it, too, by bringing the quagga back to life. However, Wilson was careful to balance this speculation with skepticism. Journalists reported, "Unfortunately, the scientists are a long way from reconstructing the animals themselves. 'That will be generations – or more – from now,' says Wilson" (Begley and Katz 1984, 64). With evidence, as well as professional and popular interest on their side, Wilson hoped for the future of molecular paleontology. In the proposal, they wrote, "This is the first proposal to study the possible utility of DNA to paleontology. If clonable DNA is present in many fossil bones and teeth and in insects included in amber, a new field, molecular paleontology, can arise" ("Molecular Paleontology: Search for Fossil DNA" 1984, 4). But the proposal was in the hands of other scientists who would accept or reject it.

Reviewers were enthusiastic, but caution also colored their feedback. One reviewer referred to the proposal as "interesting, significant, even exciting" ("Molecular Paleontology: Search for Fossil DNA" 1984, 151). A second said it was a "pioneering effort" that "represents penetration of a barrier interfacing molecular systematics and paleontology" ("Molecular Paleontology: Search for Fossil DNA" 1984, 152). Another reviewer recognized potential pitfalls but said, "[…] I refuse to gaze into a crystal-ball and reject the possibility <u>a priori</u>. It is clear that <u>looking</u> for fossil DNA is worth the trials and tribulations, particularly if so distinguished [a] researcher as Wilson wishes to undergo the trauma" ("Molecular Paleontology: Search for Fossil DNA" 1984, 155). Another made a similar statement: "I am not convinced that selection of these organisms will demonstrate the universal applicability of recombinant DNA technology to systematic evolutionary studies. […]. However, at one time it was common knowledge that the earth was flat and the moon was made of green cheese" ("Molecular Paleontology: Search for Fossil DNA" 1984, 152). However, one scientist was suspicious, claiming that it was "interesting" and "technically difficult" but "not clear" on "how this approach will broaden our perspective on any major evolutionary problems" ("Molecular Paleontology: Search for Fossil DNA"

1984, 154). Indeed, the proposers themselves perceived their application as "exotic" and "speculative." But one referee reported, "If it is possible to do, they can do it" ("Molecular Paleontology: Search for Fossil DNA" 1984, 150). Overall, the referees gave every indication that this was an exciting research endeavor.

However, in the end, they rejected the proposal. The panel explained its decision based on their perception that "the proposal is not designed to develop a new technology which when developed would be broadly applicable to a wide range of specimens." The panel further explained, "At most the project will provide that some fossil remains contain clonable DNA." Further, "If clonable DNA is obtained, its usefulness for phylogenetic studies remains to be shown, given the likelihood of the occurrence of unquantifiable diagenetic change and the presence of contaminating DNA" ("Molecular Paleontology: Search for Fossil DNA" 1984, 158). They concluded, "The Panel does not consider that obtaining clonable DNA from a 140 year old museum specimen, however interesting, provides sufficient preliminary evidence that DNA from 10,000 or 26 million year old specimens is likely to yield valuable information" ("Molecular Paleontology: Search for Fossil DNA" 1984, 158). In retrospect, researchers recognize the quagga study as a breakthrough. But in 1984, the panel disagreed. The search for DNA from fossils seemed at a standstill.

1.3.3 "Molecules and Mummies"

While Berkeley was experimenting with amber fossils and museum skins, similar ideas, once again, were gaining ground elsewhere. Svante Pääbo, graduate student at University of Uppsala, Sweden, was on his own in search for ancient DNA. In 1981, he began exploring the idea of DNA in ancient material, with the hope of recovering DNA from ancient Egyptian mummies. Pääbo was aware of Wilson's work from the 1960s and 1970s, and like Wilson was attracted to applications of molecular biology in evolutionary biology, especially for studying the origin and evolution of humans.[16] Pääbo was also aware of Alec Jeffreys, molecular biologist at University of Leicester in the UK, and his work on the genetic evolution of humans and apes.[17] Their works prompted Pääbo to speculate on how molecular methods might apply to not only life today but life in the past. However, before

[16] Allan Wilson was a leader of a major movement to integrate molecular biology with the study of evolutionary history, specifically regarding the relationship between humans and primates. See Sarich and Wilson (1967) and Cann, Stoneking, and Wilson (1987).
[17] In the 1980s, developments in the world of forensic science, like DNA fingerprinting and DNA profiling, emerged in parallel with ancient DNA research, revolutionizing the forensic studies. See Jeffreys, Wilson, and Thein (1985).

exploring these ideas, Pääbo examined the literature. He discovered no reports of the preservation and extraction of DNA in ancient material, but the literature on ancient proteins and ancient mummies looked promising. Encouraged, he decided to search for DNA in ancient Egyptian mummies (Pääbo 2014a, 23–26).

In the summer of 1981, Pääbo went to the supermarket to buy a small sample of liver. He was interested in the theoretical preservation of DNA in ancient Egyptian mummies, and he knew that mummification involved dehydration. Pääbo assumed that the process of mummification would remove water, deterring DNA degradation. So, he decided to mimic mummification by heating and dehydrating the liver in an oven, thus transforming it into a shriveled, hardened, and mummified substance. Pääbo then tried to extract and sequence DNA from it. In short, Pääbo recalled that the "secret" experiment – secret to avoid reprimands from his supervisor or humiliation if his speculative study failed and was found out by his lab – was surprisingly successful, revealing small but significant amounts of DNA. However, the preservation and extraction of DNA hundreds and thousands of years old remained to be tested. To satisfy his curiosity, Pääbo approached the curator of a small museum with a request to sample and search mummy material for DNA.[18] Pääbo was allowed to sample previously detached or already damaged skin and tissue from three mummies, but to his disappointment, he did not discover any DNA (Pääbo 2014a, 26–28).

Despite this unsuccessful search for DNA in ancient Egyptian mummies, Pääbo remained insistent in his investigation. In the summer of 1983 – the same year Poinar, Hess, Higuchi, and Wilson embarked on their search for DNA from insects in ancient amber – Pääbo approached a different museum, the Berlin State Museums in Germany, and asked for mummy material. The curators and collection proved helpful, and after two weeks he returned to Sweden with more than thirty samples. He hoped that at least a few samples would show signs of DNA. Indeed, under the microscope, the left leg of a mummy child showed evidence of cellular preservation, and when stained, it also showed evidence of DNA preservation (Pääbo 2014a, 26–30). For Pääbo, this was a sure sign of authentic DNA and not a case of contamination. He recalled, "Since this DNA was in the cell nuclei, where the cellular DNA is stored, it could not possibly be from bacteria or fungi because

[18] In its early days, and still today through to different degrees, ancient DNA activity and specimen sampling is destructive. Museums value their collections for their rarity, and their main mission is to conserve past and present specimens for future generations to study or enjoy. While molecular methods offer new opportunities for curators to make new uses of old collections, taking samples of skin, tissue, or bone can damage specimens. This presents a challenge to researchers and curators to find a compromise between their motives. See Graves and Braun (1992).

such DNA would appear at random in the tissue where the bacteria or fungi were growing." Therefore, he reasoned, "This was unambiguous evidence that DNA from the child herself was preserved" (Pääbo 2014a, 30). Two other samples looked equally encouraging, so Pääbo attempted to recover DNA if indeed it was there. His search appeared successful. The DNA was damaged, but it was present and for Pääbo that was what mattered. He remembered feeling exhilarated, and started writing up his research, methods, and results for potential publication. As he reported his research, he "speculated wildly about what might be possible if DNA from ancient Egyptian mummies could be systematically studied." In his review of these results, he stated, "'Work over the next few years will show if these expectations will be fulfilled'" (Pääbo 2014a, 32). Pääbo had high hopes for this introduction of molecular biology into the study of history and archeology.

To acknowledge East German curators who allowed him to sample the specimens, Pääbo chose to publish the first recovery of DNA from 2,000-year-old humans in an East German journal called *Das Altertum* (Pääbo 1984; Pääbo 2014a, 31–32). It was published in 1984, but its reception was anticlimactic. According to Pääbo, this research, which he considered groundbreaking, received no attention at all. The first formal report on the preservation and extraction of DNA from ancient Egyptian mummies received no response – no letter, no question, not even a reprint request. Pääbo, disappointed and discouraged, reasoned that perhaps the absence of attention was because it was published in an East German journal that was not widely read. After much anticipation of this publication, Pääbo recalled, "I was excited, but no one else seemed to be" (Pääbo 2014a, 32). In an attempt to reach a wider audience and attention, he sent a second manuscript for review to the *Journal of Archaeological Science*. The journal received the manuscript in October 1984, and although the article was accepted, the review process was slow and the paper was not published until 1985 (Pääbo 1985b; Pääbo 2014a, 32–33). Between low readership and slow publication, Pääbo said he questioned if anyone cared about the prospect of procuring DNA from ancient or extinct material, and its implications for paleontology, archeology, and evolutionary biology (Pääbo 2014a, 32–34).

1.3.4 "Raising the Dead and Buried"

In 1984, the year Pääbo started studying DNA preservation in ancient Egyptian mummies, an article on the recovery of DNA from the quagga was published in *Nature* (Higuchi et al. 1984). Higuchi and Wilson, along with Barbara Bowman, Mary Freiberg, and Oliver Ryder, had succeeded in the "first demonstration" that "DNA" could be "recovered from the remains of an extinct species" (Higuchi et al. 1984, 284). This was important for three reasons. First, this study outlined the theoretical and technical details for identifying, extracting, amplifying, and sequencing DNA from ancient and extinct material. Although many methods used were standard to the extraction and purification of DNA from modern material, the process was complicated by the fact that the quagga DNA was degraded, making technicalities more challenging. Second, the study demonstrated that despite the odds, DNA could stand the test of time, at least in the case of the 140-year-old quagga. Third, the team demonstrated that the DNA could be successfully analyzed and applied to phylogenetic problems like the extinct quagga's evolution in relation to extant relatives like the zebra and horse. At this time, the quagga's exact relation to other relatives in the genus of *Equus* – a group that includes horses, donkeys, and zebras – was uncertain based on morphology alone. However, Higuchi and colleagues demonstrated that the molecular evidence could clarify this and even suggest a date of divergence between the two (Higuchi et al. 1984; Zuckerland and Pauling 1970).[19] Overall, the study suggested that the search for ancient DNA was a way to travel back in time to study evolution in action.

According to Pääbo, he was shocked at a successful study so similar to his own, and was surprised at its publication in an esteemed journal like *Nature*. However, he also appreciated this work because it validated his own. Pääbo recalled, "If Allan Wilson was studying ancient DNA, and if *Nature* considered an article about 120-year-old DNA interesting enough to publish, then surely what I was doing was neither crazy nor uninteresting" (Pääbo 2014a, 34). Wilson, an established experimentalist who was respected for his molecular work in evolutionary biology, created credibility around the prospect of procuring DNA from fossils. Inspired, Pääbo wrote a third manuscript on DNA from ancient Egyptian mummies and submitted it to *Nature*. It was reviewed and published in April 1985 (Pääbo 1985a). For Pääbo, DNA could provide genetic answers to historical and archeological questions about Egyptian culture, evolution, population, and disease (Pääbo 1985a, 645). However, like the 1983 amber study and 1984 quagga study, DNA was only preserved in some samples but not others. In this case, only one of twenty-three

[19] By comparing ancient and modern sequences, researchers confirmed that the DNA was not a contaminant but authentic to the quagga. They also used the comparison of sequences to construct a phylogenetic tree from the molecular data, which provided evidence that the quagga and zebra were most closely related but diverged from each other approximately 3–4 million years ago. From this, they estimated that the common ancestor for odd-toed and even-toed ungulates – a group of hooved mammals including horses, zebra, giraffes, and camels – occurred 55–60 million years ago. This evidence of evolution supported the molecular clock hypothesis; a relatively recent idea, at the time, that molecules and their mutation rates could be used to date divergence of one species from another in their evolutionary history. See Zuckerkandl and Pauling (1962).

mummies exhibited evidence of DNA. If paleontological and archeological specimens were to be relevant resources of data for molecular evolution studies, then its survival in specimens would need to be a repeatable, not rare, occurrence. Ancient DNA would need to be more than an anomaly. Pääbo hoped this publication in *Nature*, would finally attract the attention he originally anticipated, and encourage further exploration.

As Pääbo recalled, he was excited to see another scientist, especially a scientist like Wilson, researching the preservation and extraction of DNA from fossils. As Pääbo recollected, "I thought about how to approach Allan Wilson – a demigod, in my view – to ask if I might work with him at Berkeley after my PhD defense" (Pääbo 2014a, 35). Yet insecure about how to introduce himself and his work, Pääbo simply sent Wilson a print of the manuscript to be published, thinking he would like to see it before it was put into press. Wilson had no knowledge of Pääbo as a researcher, but the manuscript made a considerable impression. Pääbo remembered Wilson's reaction: "I received a response from Allan Wilson, who addressed me as 'Professor Pääbo' – this was before both the Internet and Google, so there was no obvious way for him to find out who I was. The rest of the letter was even more amazing. He asked if he could spend his upcoming sabbatical year in 'my' laboratory!" (Pääbo 2014a, 35). For Pääbo and his lab, it was a "humorous misunderstanding." Pääbo remembered, "I joked with my lab mates that I would have Allan Wilson, perhaps the most famous molecular evolutionist of the time, wash gel plates for me" (Pääbo 2014a, 35). After this invitation, Pääbo replied to Wilson, explained the situation, and hoped for an invitation for a postdoctoral position with Wilson in Berkeley.

In the mid 1980s, the search for ancient DNA, mainly a private and professional affair, turned public. In a commentary to complement the quagga study, Jeffreys speculated on the scientific significance of this novel but controversial research (Jeffreys 1984). In "Raising the dead and buried," Jeffreys remarked, "Is the quagga as dead as a dodo? Not entirely, and nor indeed might be the dodo, if the remarkable findings of Russell Higuchi, Allan Wilson and co-workers […] are anything to go by." Speculating on its practical potential, Jeffreys wrote, "Any hopes that molecular biology and palaeontology can be fused into a grand evolutionary synthesis by studying fossil DNA, still look like nothing more than a glorious dream. However, it is far too early to give up, and it might just be possible that DNA has survived in some fossilized material" (Jeffreys 1984, 198). DNA from fossils was an anomaly to researchers. While the quagga study offered credibility behind the idea, it was also a serious source of curiosity. For researchers, it introduced more questions than answers. There was a sense of spectacle around the science, which was beneficial in stimulating other studies to search for DNA from ancient and extinct organisms. In 1986, for example, subsequent studies claimed to recover DNA from a 2,500-year-old bog body in the UK and 8,000-year-old brains in a swampy cemetery in the US (Hughes and Jones 1986; Doran et al. 1986). Wilson hoped that this new line of research might be applied to studies of ancient or extinct humans, with the goal of determining the evolutionary relationship between Neanderthals and modern humans (Schmeck Jr. 1985, 22). While researchers at the time recognized the quagga study as a conceptual, empirical, and technical breakthrough, they also saw it in this light because it was published in *Nature*. Publication in *Nature* sent a statement of authority and legitimacy to the scientific community, but as a journal with a history of trying to attract attention from both professional and popular audiences, it sent a statement of interest to the public, too. [20]

For Wilson, the quagga study was his first study to exhibit evidence of DNA from an ancient and extinct organism, but he and his lab were no strangers to the study of ancient molecules, like amino acids or proteins, and nor were they strangers to the press and public attention associated with it. In the summer of 1977, a 40,000-year-old baby mammoth was found preserved in permafrost. Named Dima, the baby mammoth, was an exceptional fossil find for two reasons. First, it was the most complete mammoth discovered since the 1800s. Second, it was the only complete mammoth to be excavated, then refrigerated in a lab. Wilson was especially interested in this specimen for the latter reason. In the spring of 1978, bits and pieces of the carcass were selected, sampled, packed in dry ice, and shipped from the USSR to the US ("Tissue of Baby Mammoth at Berkeley" 1978). At the time, Wilson was one of the first and few researchers to use molecular data to reconstruct patterns and processes in evolutionary history. Since the 1960s, he had demonstrated the power of molecular evidence from modern organisms to understand evolutionary change (Sarich and Wilson 1967; King and Wilson 1975; Cann, Stoneking, and Wilson 1987; Dietrich 1998). Wilson and his lab thought to extend their work to ancient organisms, too. Subjected to cold conditions, Dima offered this opportunity for research in the unexplored areas of immunological, chemical, and molecular research of fossils.

[20] Founded in London in 1869, *Nature* began as a periodical dedicated to disseminating popular science pieces for laymen and scientists. Indeed, it tried to attract contributions from leading men of science for laymen interested in science to read. However, *Nature*, like *Science*, found achieving this sort of balance between professional and popular interests difficult to do. In the twentieth century, particularly during the interwar and postwar years, *Nature* took the stage as an exciting and elite scientific journal with a worldwide readership. Today, *Nature*, along with *Science*, is one of the most sought after scientific journals, attracting attention from scientists and non-scientists alike. See Baldwin (2015).

From the beginning, the recovery of molecules from mammoths was of interest to scientists as well as members of the press and public. Dima's discovery and delivery to Berkeley hit headlines ("Dima: A mammoth undertaking" 1978; "Russia's gift: A well-aged mammoth" 1978; "Siberian baby mammoth" 1977; "Tissue of baby mammoth at Berkeley" 1978; "UC to Test Slice of Mammoth" 1978).[21] Reporter Walter Sullivan for *The New York Times* explained that the primary point of the study was to investigate the preservation of proteins, as well as nucleic acids, with the hope it would help elucidate the relationship between the extinct mammoth and extant elephant. Yet Sullivan also entertained the idea of bringing back the mammoth. He stated that while the possibility to "clone" a mammoth seems "improbable" it is not "impossible" (Sullivan 1978). In 1980, Ellen Prager, postdoctoral researcher with Wilson, and others reported the recovery of proteins, but not DNA, from the baby mammoth, and they published a paper with the results in *Science* (Prager et al. 1980). Reporter John Wilford, also for *The New York Times*, showcased the study as an "exploratory tool in the emerging science of fossil genetics" but also speculated on its use as a tool for bringing mammoths back to life: "If they could find intact strands of DNA […], the raw material of heredity, they could conceivably reconstruct the long-extinct species through cloning, though the chances of doing this are considered quite remote" (Wilford 1980). Later, Wilson and Higuchi tried to extract DNA from the mammoth, but had difficulty authenticating and replicating the DNA (Higuchi and Wilson 1984; Schmeck Jr. 1985; Biomedical Research Support Grant 1985). Nonetheless, early evidence of molecules in ancient and extinct creatures like the mammoth encouraged speculation about their resurrection.

However, four years later, the fantasy of bringing back the mammoth looked like a reality, at least according to the media. In April 1984, *MIT Technology Review* published a story declaring that the mammoth, extinct for the past tens of thousands of years, had been brought back from the dead (Ben-Aaron 1984). Dr. Sverbighooze Nikhiphorovich Yasmilov, University of Irkutsk, and Dr. James Creak, Massachusetts Institute of Technology, were the masterminds behind this achievement. According to the article, Yasmilov had recovered a frozen mammoth egg from a frozen mammoth carcass in Siberia, and sent the sample to Creak. Creak recovered DNA from the frozen mammoth egg, then combined the sequences from the extinct mammoth with sequences from the sperm of an extant Asian elephant. The resulting product was implanted into the wombs

[21] Like dinosaurs, mammoths have fascinated humans across the world and over the centuries as we have tried to make sense of prehistory and our own human history. See Cohen (2002).

of Indian elephants who served as surrogates for the elephant-mammoth hybrids. Although several surrogates miscarried, two gave birth to the first elephant-mammoth hybrids. Scientists called these babies a new species, *Elephas pseudotherias*. Long story short, the news went viral as the story was publicized by the *Chicago Tribune*, then sensationalized across 350 newspapers across the US (Salsberg 2000; Matill 1984). However, the story was a parody, written by an undergraduate student, Diane Ben-Aaron, for an undergraduate course. It was published on April 1, 1984 (April Fool's Day). Nonetheless, the piece produced a dramatic response. Lawyer Corey Salsberg, writing on ethics and science, said, "Hoax or no hoax, the very idea of bringing the mammoth back from its icy grave – even a hybrid containing only a half-complement of mammoth genes—struck a nerve and raised issues sufficiently sensitive to command international headlines" (Salsberg 2000, 1–2). The idea of resurrection was of immediate and international interest.

Enthusiasm for the spectacle of DNA from fossils, and subsequent speculation that it could be used to bring back extinct organisms, resulted in reports across newspapers. *The National Examiner*, for example, wrote a tabloid titled, "Mad scientists are cloning dinosaurs as weapons of the future," with a hybrid story highlighting the recent mammoth resurrection hoax and real current research at Berkeley (Clifton 1984). The leading story line said, "Egghead scientists are secretly cloning dinosaurs – and terrified humans may soon be fleeing for their lives from gigantic monsters belonging to the prehistoric past" (Clifton 1984, 31). But there were also soberer accounts of resurrection. Reporter Harold Schmeck for *The New York Times*, for example, speculated on whether scientists could "recreate" extinct animals but confessed that the "revival of species" was "remote" (Schmeck Jr. 1984). In another article, he recorded Wilson and Higuchi saying mammoth cloning was "far from practical reality as to be hardly worth discussing" (Schmeck Jr. 1985, 22). *New Scientist* ran a report, "The resurrection of the quagga," suggesting that "[s]tories that the quagga, the dodo, and the mammoth might be about to rise and stalk the Earth once more are somewhat exaggerated" but admitted that "resurrecting the quagga" may one day "indeed be possible" ("The resurrection of the quagga" 1984, 21). However, the media was not alone in reporting on resurrection.

Indeed, some scientists entertained the idea of bringing ancient and extinct creatures back. In "Cry of the quagga," Jerold Lowenstein, evolutionary biologist and collaborator with Wilson, noted the public interest and media exposure that the pursuit of DNA from fossils had attracted (Lowenstein 1985). He noted countless media conjectures that claimed "that this was the first step toward bringing extinct species back to life" (Lowenstein 1985, 42).

He also commented on a piece published by a scientist in *New Scientist* – “To clone a dinosaur” – that added fuel to the fire (Lowenstein 1985, 42). Paleontologist Michael Benton, in this article, entertained dinosaur resurrection if only to say it was a distant dream. However, he did suggest a scenario in which the quagga could be brought back by inserting its DNA into the embryo of a mountain zebra (Benton 1985, 43). By the mid 1980s, the spectacle of DNA from fossils was an increasingly interesting topic among professional and popular audiences. It spurred further speculation about the possibility of using DNA to bring back dinosaurs, mammoth, the quagga, and other extinct organisms.

1.3.5 Conclusion

This section showed that the prospect of procuring DNA from fossils, initially inspired by insects in ancient amber, grew to include the investigation of DNA from museum specimens and human remains. First, I argued that ideas about DNA from fossils continued to surface among different people in different places, but activity to turn these ideas into experiments with evidence for such a hypothesis was crucial for the development of this novel and controversial area of research. Here, the lab – specifically Wilson’s lab in Berkeley and Pääbo’s lab in Uppsala – became a center of activity that suggested the search for DNA from ancient and extinct organisms was a credible, but still curious, phenomenon. The first proposal to investigate DNA from fossils, as well as the first publications of DNA from the extinct quagga and ancient Egyptian mummies, were critical components in transforming ideas into experiments with evidence in support of a hypothesis. Although the proposal was rejected, the two papers published in *Nature* made a serious statement about the feasibility of recovering DNA from fossils. These papers placed the nascent technoscience on the map and encouraged its exploration. Second, I argued that there was a sense of spectacle around the anomaly of ancient DNA. It was this sense of spectacle that encouraged the emergence of a technoscience as several independent studies explored for the presence of DNA in ancient and extinct organisms. For scientists, DNA’s preservation in certain circumstances presented more questions than answers. Its unusualness caused further curiosity about whether such studies were a rare or repeatable occurrence. Finally, I argued that with evidence for DNA from fossils also came speculation about resurrecting the dead and buried. The idea of molecules in fossils attracted media attention as early as the 1970s, specifically with Dima’s discovery and delivery to Berkeley where researchers recovered, then reported proteins from the baby mammoth. In the 1980s, journalists continued to wonder whether bringing back extinct creatures was possible, while some scientists dismissed cloning dinosaurs but speculated more soberly about bringing back the quagga. Together, spectacle and speculation contributed to the emergence of a technoscience but one closely connected to the idea of resurrection.

1.4 SPECULATION AND SPECTACLE IN ANCIENT DNA

1.4.1 Introduction

Ancient DNA, since its start, attracted professional and popular attention. It was a curiosity to scientists, as well as the press and public, and the spectacle of DNA from ancient and extinct organisms fed speculation about bringing them back to life. The growing hype for the search for DNA from fossils was both indicative of and performative in the starting stages of the technoscience. But the nascent technoscience needed more than publicity. In the 1980s, researchers recognized there were theoretical and technical challenges they needed to address if their ideas were to develop into a discipline in its own right. Overall, the spectacle of DNA from fossils placed the practice in the media limelight, helping to create a public profile for the emerging technoscience. However, its public profile was also a simultaneous source of tension for researchers who wanted to separate the science from speculations about bringing back extinct creatures such as dinosaurs or mammoths. It was this sense of spectacle that would simultaneously help and hinder the emergence of ancient DNA research as scientists tried to transform it into an established technoscience.

1.4.2 "Almost guaranteed that there would be a media response"

As historians of science have argued, disciplinary development is often messy.[22] In reality there are a range of reasons for the emergence of new ideas and innovations, but there is a tendency to reduce history's complexity by pinpointing a single and solidifying event as the cause of scientific or technological change. As sociologists of science have argued, "breakthrough" is a distinct discourse usually used to try to capture a moment or event responsible for a technoscientific transformation. Nik Brown and Mike Michael said, "Breakthrough is probably our most constant and pervasive discursive method for organising narratives about science, and yet it is also probably our most contested" (Brown

[22] Scholars have explored and tried to explain the process of disciplinary development in the sciences with regards to differences between discipline formation and professionalization. See Kohler (1982), Nyhart (1995), Farber (1997), and Barrow (1998). Also see Everett Mendelsohn's "The Emergence of Science as a Profession in Seventeenth-Century England" (1964) and Nathan Reingold's "Definitions and Speculations: The Professionalization of Science in America in the Nineteenth Century" (1976).

and Michael 2003, 5–6). For Brown, the metaphor of breakthrough is constitutive, having rhetorical, historical, and material properties that help to mobilize interest and investment (Brown 2000, 89). However, Brown and Michael argued that this discourse of breakthrough can distort the reality of scientific, technological, and methodological developments which often occur through a prolonged process of trial and error (Brown and Michael 2003, 5–6). They argued that breakthrough offers structure to otherwise ill-structured episodes of emergence, but neglects the complex process of science. Indeed, breakthrough, as a rhetorical strategy, draws attention to a moment in science, not the process of science. For example, researchers retrospectively refer to the 1984 quagga study as a breakthrough and the beginning of ancient DNA research. As one scientist argued, "The '84 study – Russell's '84 study – was the actual beginning of the field because he was on the front cover of *Nature*. […] That was the beginning of the field because people […], for the first time, thought, 'What happens to DNA […] when an organism dies?' […]." This scientist said, "Russell's paper was not the first demonstration necessarily of DNA in a dead thing, but it was the first one which stimulated people to think that ancient DNA existed" (4-01:39:15).[23] However, in the midst of activity, breakthrough is not always apparent. Another researcher involved in the quagga study remarked, "When one has been part of what looks like a breakthrough, you would think one might want to continue. I had the opposite reaction. I didn't think it was a breakthrough. Again, it tells you how wrong a scientist can be. I thought it was a one-off" (26-01:10:45). This history is evidence that emergence is not simply a story of discovery or breakthrough. Instead, it demonstrates that developments in ancient DNA research were part of an extended, contingent, and contentious process. Expectations are part of this process.

For some scholars, the presence of exaggerated expectations is evidence of an emerging science or technology.[24] Brown argued that in the starting stages of a technoscience, expectations are intense, ambitious, and even exaggerated. He argued that the intensity in

[23] In China in 1980, Hunan Medical College researchers exhibited evidence that DNA could be preserved and extracted from ancient bodies. See Hunan Medical College (1980). A second study in 1982 demonstrated the discovery of short tandem repeats (STR), which later became the basis for DNA fingerprinting and profiling. See Hamada, Petrino, and Kakunaga (1982). These precursors of ancient DNA activity, like Pääbo's paper published in *Das Altertum* (1984), were not widely read or recognized at the time, nor are they frequently featured in reviews of ancient DNA research, with the exception of Susanne Hummel's *Ancient DNA Typing: Methods, Strategies and Applications* (2003). The paper published in 1984 in *Nature* on DNA from the quagga is typically credited as the first demonstration that DNA could be preserved and extracted from an ancient or extinct organism. This is likely because the paper was published in a high-profile and highly-publicized journal such as *Nature*.

[24] Scholars noted further features of emerging sciences and technologies, but this thesis focuses on the role of hype in the emerging technoscience and celebrity science of ancient DNA research. See Brown, Rapport, and Webster (2000).

which expectations are expressed is indicative of emergence. In other words, hype, or prolific publicity, is indicative of emergence. Brown said, "Hype corresponds to a particular phase in the career of innovations. The whole language of novelty, newness and revolutionary potential is actually part and parcel of the hyperbolic discourse surrounding the early or opening moments of resource and agenda building […]" (Brown 2003, 11). Brown and Michael also argued that hype is especially expressed through discourses of improvement and replacement as new technologies and techniques come to substitute old ones (Brown 2003; Brown and Michael 2003). Here, scholars suggested that a crucial component in producing hype is articulating a vision in order to mobilize the financial and material investments required to bring that vision into being (Brown 2003; Brown and Michael 2003). For example, in the case of the quagga, ancient DNA research was portrayed by both researchers and reporters as a new tool to study evolutionary biology. It represented a way to refine or rewrite hypotheses by combining morphological evidence with molecular evidence. Yet the press also hyped the technoscience by speculating on it as a way to bring back extinct organisms This was a hypothetical and fantastical vision, but one that nonetheless contributed to the emergence of expectations during this decade.

Hype is not only indicative of emergence, but it is also performative in the starting stages of innovations (Michael 2000; Brown 2003; Borup et al. 2006). In the beginning, the utility and reliability of a new technology or technique is not a given but must be demonstrated. Brown said, "That is, the newer or more unfamiliar a research agenda is, the greater will be the need to use hype as a means of defining roles, responsibilities and duties" (Brown 2003, 13). In other words, hype can be used to generate interest and guide activity that might lead to the experimentation of novel ideas or innovations in order to establish evidence for their value. Wilson's proposal, as well as NSF's rejection of it, supports this suggestion. Although the proposers were slightly skeptical about the preservation and extraction of DNA from fossils, they hyped the idea in terms of its potential payoff for evolutionary biology in hopes of enlisting further funding to generate more evidence for or against the idea. Interest, including popular interest, was also valuable in raising the professional and public profile of the novel and controversial technoscience. Wilson's grant cited a recent media report on their discovery of DNA from the extinct quagga as evidence of its research relevance for a professional and public audience ("Molecular Paleontology: Search for Fossil DNA" 1984; Begley and Katz 1984). In an emergent technoscience, any publicity appeared to be good publicity.

However, it is important to note that expectations have a temporal and spatial dimension

(Brown 2003). In other words, expectations change over time, adapting to changing circumstances as research realities unfold, and they vary from one individual or group to another. These early expectations of DNA from fossils were not isolated imaginings of a single group. Rather, both researchers and reporters engaged in expectations, and sometimes had different visions for the future of ancient DNA research. Brown put the point this way: "Futures are never simply homogenous singular representations but are differently interpreted and engaged across constituencies […]" (Brown 2003, 13). However, it is also possible, and likely, for an individual or group to embody multiple expectations simultaneously. Brown explained, "This then is part of the difficult and challenging business of uncovering the complex chains of agency that together contrive to produce various representations of the future, thus guiding action and building agendas" (Brown 2003, 13). As scholars in sociology of science have argued, it is important to realize that various individuals or groups, other than those scientists involved in the innovation or application of the new technoscience, may share a stake in it (Borup et al. 2006, 286). These different individuals or groups may contribute to the construction of expectations. Indeed, this chapter is evidence of this. In this history, researchers, reporters, and the public were interested in the search for ancient DNA for one reason or another.

Scholars have highlighted that no single group is solely responsible for generating hype, nor can necessarily be blamed for hype when expectations crash and burn. Brown, as well as science communication scholar Dorothy Nelkin, noted the conventional press release as an illustrative instance of where science and media values coalesce, then coevolve. For Brown and Nelkin, the press release exceeds publication expectations of both groups in exchange for a publicly accessible piece that sits between traditional boundaries of science, press, and public relations (Brown 2003, 14; Nelkin 1995). The discovery of DNA in the quagga is an example of this. In reference to the press release, one interviewee recalled, "[…] The University of California likes to publicize papers that are in a journal like *Nature*. […] [We] probably wrote it, but it was edited at least to make it sound sexier, I think, than it really was." This interviewee presented this perspective: "Juxtapositioning terms like 'clone' and 'extinct' and 'DNA' – even though the clone was a recombinant DNA clone, not a clone of the animal – almost guaranteed that there would be a media response" (26-01:24:00).[25] These different groups participate in productions of hype, capitalizing on publicity potential in order to bring awareness to an emerging and evolving

[25] I tried to search for the press release via internet and archive searches, including the Allan Wilson Papers at the Bancroft Library at the University of California, Berkeley. I also asked scientists who might have a personal record. Unfortunately, my efforts and others' efforts to locate the press release were unsuccessful.

practice. In the starting stages, hype is performative and can benefit researchers and reporters alike. Indeed, it is often pragmatic to engage in hype to attract public attention and further funding. As scholars stated, "In a sense, expectations are both the cause and consequence of material scientific and technological activity" (Borup et al. 2006, 286). This early history of ancient DNA's disciplinary development showcases that some scientists and media members contributed to the construction of expectations that helped form its foundation as an emerging technoscience that appealed to both a professional and public audience.

In this history, speculation that contributes to expectations takes on different degrees and various forms. In this history, specifically, speculation takes on two forms. First, speculation existed as a part of the regular scientific routine. For example, scientists – like Poinar, Wilson, and Pääbo – speculated about the preservation and extraction of DNA from fossils. This degree of speculation was a normal, even necessary, function of generating and testing hypotheses. Second, speculation extended beyond immediate research practices and potential. For example, some scientists – like Pellegrino and Poinar – speculated about using DNA from fossils to not only study evolutionary history but to maybe one day bring back extinct organisms. However, it is important to note that other individuals, like members of the media, engaged and encouraged speculation, too. Here, journalists speculated about the future application or implications of particular scientific projects when reporting research to the public. This is a frequent feature of science journalism (Nelkin 1995). Further, speculation plays off of spectacle. Generally, a scientific spectacle typically refers to visual or audible phenomenon (Schaffer 1983; Golinski 1989; Werrett 2011). However, science – its production and presentation – can take on various forms of spectacle (Agar 1998). Ancient DNA, for example, is far from a sensory phenomenon, but the idea of extracting and sequencing DNA from fossils was a source of wonder to both professionals and the public as it defied previous presumptions about molecular preservation. Here, different people had different ways of interpreting ancient DNA's sense of spectacle. For some it was a way to directly study evolutionary history. For others, it was a way to bring back extinct species.

1.4.3 "For better or for worse, in ancient DNA there is media"

While ideas about the theoretical preservation and potential extraction of DNA from fossils arose independently among different people in different places, the search for ancient DNA became localized, then specialized at Berkeley. It was here that activity increased, initially influencing the beginning of a way to study the evolutionary history of ancient and extinct organisms. Wilson had the authority, security, and institutional infrastructure to test these speculative ideas. In fact, it was an extension of his expertise and visionary tendency. One scientist said, “Allan wanted to be able to expand the reach of the molecular clock to extinct species as well as existing species” (26-00:27:00). It was risky research, but Wilson encouraged it. As another researcher remembered, “That lab was incredibly creative. […]. Allan – [laughs] – was pretty out there and he just let his postdocs do pretty much anything they wanted to – encouraged it actually, encouraged you. The crazier you were the more he encouraged you” (18-00:03:00). Today, researchers regard the Wilson lab, along with the 1984 quagga study, as the birthplace of the technoscience. As this researcher remarked, “Allan’s lab is the birthplace […]. […] It’s the birthplace of ancient DNA […]. […] [H]e would always take it and say, ‘Let’s put a molecular clock on it. Let’s look at the past’ […]” (18-00:06:00). Working on the boundaries of paleontology, molecular biology, and evolutionary biology, the Wilson lab pushed perimeters during this era of exploration. However, the more they explored the anomaly of ancient DNA, the more challenges they encountered. Even in this exceptional environment, researchers faced resistance.

Scientists faced resistance for theoretical and technical reasons. By the mid-to-late 1980s, several studies had demonstrated that DNA, although chemically altered, survived in fossils hundreds to thousands of years old. The 1984 quagga and 1985 mummy study elicited excitement at the notion of DNA in paleontological and archeological specimens, but the results flew in the face of assumptions regarding the chemical composition of DNA. Even researchers responsible for producing these results found them surprising. One scientist said, “Most of the resistance was […] a preconceived notion […] – the same notion that [we] had – that DNA shouldn’t last that long […]” (26-01:07:00). Although studies suggested that DNA could survive in ancient tissue, they also suggested that DNA degradation processes and patterns were far from understood. For example, in the mummy study, Pääbo noted that the majority of mummies exhibited no evidence of DNA. For Pääbo, this suggested that preservation potential was fickle. Therefore, a better understanding of how chemical, environmental, and taphonomic processes interacted were needed to better guide sample selection of specimens likely to yield DNA (Section 1.3.3). Second, technologies and techniques during this decade were insufficient to process degraded DNA, or were at least insufficient to turn the search for DNA from fossils into routine research. While molecular cloning methods facilitated the foundation of ideas behind the technoscience, they were only optimal for larger quantities of DNA. Ancient DNA, often occurring in smaller quantities, was difficult if not impossible to accurately amplify without errors. Scientists who tested the technology with ancient and extinct specimens quickly realized these challenges. For example, the quagga study was only accomplished through a "painstaking process" of trial and error. As one researcher recounted, "I remember watching him [Russell Higuchi] moving racks from water bath to water bath, and I didn't know what it was because it was '83–'84 […], and it was PCR. He was doing PCR and he was doing this on the quagga. The technology needed to be integrated with the question […]" (18-00:08:30).[26] Even Pellegrino, the most imaginative individual of this group, exercised a sense of skepticism when it came to technology (Section 1.2.2). Overall, theoretical and technical advancements were required to demonstrate whether DNA preservation and extraction from fossils was a rare or repeatable phenomenon.

However, even if theoretical and technical challenges were overcome, there were still two issues researchers needed to address; contamination and replication. Could scientists control for contamination, and if so, could they replicate the results? Tkach, Poinar, and the Extinct DNA Study Group, as early as the 1980s, conceded these challenges (Sections 1.2.3 and 1.2.4). Criteria of authenticity were crucial to the credibility of the emerging technoscience. Disregard for this could harm the search for DNA from fossils before it began. Researchers involved in the quagga and mummy studies also demonstrated an awareness of this by testing for ancient DNA authenticity via phylogenetic comparison. These publications marked the first seemingly successful efforts to demonstrate the preservation and extraction of DNA in ancient and extinct organisms (Section 1.3.4). However, the results were anomalous. The NSF Panel was skeptical of supporting Wilson's proposal for this reason (Section 1.3.2). Further work would need to tackle these theoretical and technical tasks, as well as address issues of contamination and replication. Together, these points posed a challenge to the future of ancient DNA research if it were to evolve from an emergent into an established practice.

From the beginning, the search for ancient DNA was a topic that found itself in the media limelight. In the mid-to-late 1980s, DNA from fossils was an anomaly, and as such it

[26] This process was an early example of a technique called the polymerase chain reaction, PCR, that was developed in the mid-to-late 1980s. PCR, after its development and implementation worldwide, came to replace the previously manual process described in this quotation. PCR become a fundamental technique in molecular biology for the sequencing of both modern and ancient DNA.

interested researchers from paleontologists and archeologists to molecular biologists and evolutionary biologists. One scientist said, "[…] [I]t just gives you access to what you might intuitively think is unreachable, unknown, and mysterious. […] It is like a key to a mysterious room or a looking glass or something like that" (1-00:52:25). But for the press and public, and some scientists, ancient DNA research was more than a way to rediscover the past. It was a way to resurrect it, too. This speculation interested the public, and the media capitalized on this interest by repeatedly reporting the search for DNA from fossils in relation to ideas of resurrection. Looking back on the technoscience's past and present, a second scientist said, "The media love ancient DNA. *They love it.* They *absolutely* love it. Usually, the key question is about cloning. They just can't get enough of it" (3-01:13:00). Reflecting on its history and the role of the media in ancient DNA research, another researcher remarked, "[…] I think they're inextricably linked. They've certainly been front and center with this field for many, many years because of the general interest and the fascination. […]. They are tightly, tightly, tightly linked. So, for better or for worse, in ancient DNA there is media and I don't think you can disentangle them" (33-P2-00:01:30). Situated in between professional and popular interests, these researchers faced a special situation.

The spectacle of DNA from fossils situated the technoscience in both a professional and popular context. It was this sense of spectacle that would simultaneously help and hinder the emergence of ancient DNA research as scientists tried to transform its study into an established technoscience. This was an unusual but not unique situation for an emerging and public-facing practice. Jan Golinski, historian of science, provided a study on the history of chemistry at the Royal Society in seventeenth-century England that revealed a strikingly similar situation among natural philosophers at this time (Golinski 1989). In tracing the history of chemistry, he focused on the phenomenon of phosphorescence; a strange sensation of light without heat. Golinski argued that natural philosophers used phosphorescence to appeal to the public at a time of social, political, and cultural change in England. It was a source of public entertainment, and natural philosophers exploited its wonder to their advantage. Golinski claimed that phosphorescent phenomena "served to captivate spectators" and "encouraged attempts to extend the appeal of natural philosophy into society at large." Yet this advantage also had a disadvantage. Golinski explained that "phosphorus" and "its use in these contexts also put at risk the public image of the natural philosopher, who became liable to be confused with a conjurer, showman, or wonder monger." This tension had important implications for the rise of experimental philosophy.

As Golinski argued, “The spectacle of phosphorus, though apparently phenomenologically simple, was thus sociologically quite complex, functioning in a number of ways in attempts to construct a public culture for science” (Golinski 1989, 24). Golinski argued that “phosphorescence” as a “‘wonder’” was a “vital resource” used to “boost the prestige of the new philosophy” but simultaneously “a target from critics by those who opposed the public status of the science” (Golinski 1989, 11). Ancient DNA faced a similar situation. The spectacle and speculation surrounding it attracted public attention. However, by placing the search for ancient DNA on a public platform, it also put the practice and its practitioners in a position to be confused as a research area more focused on sensation than science.

In some situations, the anomaly of ancient DNA helped build bridges between scientists from disparate disciplines who were interested in testing hypotheses about the preservation and extraction of DNA from fossils. The 1984 quagga study is an example (Section 1.3.2). First, researchers’ decision to turn to the quagga was based on a technical argument; the idea that extracting and sequencing DNA from 100-year-old material may prove more productive than 100-million-year-old material. For Higuchi, this technical task was the main motivation. However, the specimen was acquired and available for study based on a sentimental argument; namely, Rau’s remorse that humans were a contributing, if not ultimate, cause of the quagga’s extinction. For Rau, there was a moral responsibility to bring it back to life. Moreover, there was also a conceptual argument underlying the specimen’s selection. The quagga was a convincing candidate because it represented a phylogenetic problem that could not be addressed through morphological data alone. For Wilson, the quagga was a perfect pick because results would be relevant to a range of researchers concerned with these kinds of phylogenetic problems. But the quagga, a crossroad of professional interests, was also a crossroad of public interests. The idea of studying evolutionary history directly through DNA enticed the press and public to wonder if DNA could be used to recreate extinct creatures, like the quagga.

Ancient DNA elicited enthusiasm from across different audiences, but it also ran the risk of drawing criticism. Scientists feared that too much speculation and spectacle with too little evidence could harm the technoscience in terms of credibility (Sections 1.2 and 1.3). For example, Tkach, Poinar, Hess, and the Extinct DNA Study Group acknowledged reputational risk associated with speculation if not established in evidence. Even Wilson, known for his revolutionary research, confessed the search for DNA from fossils was exotic or speculative. Higuchi was concerned with contamination that would affect authenticity. Pääbo conducted his study in secret for fear of failure or ridicule. Pellegrino, arguably the most imaginative individual, sought speculation to the extent that his ideas were overlooked by professional practitioners. This tension between science, speculation, and spectacle – most notably scientists' awareness and ability to engage or disengage as necessary – was critical. Enthusiasm for what seemed to be revolutionary research was not always unbridled. While these individuals flirted with a science of the future, they tamed these visions with scientific skepticism. For example, Wilson and Higuchi engaged press and public interest by answering questions about cloning the mammoth only to say it was far from possible or practical (Section 1.3.4). In the NSF proposal, they steered clear of this sort of speculation, emphasizing scientific evidence for their project instead (Section 1.3.2). Scientists downplayed spectacle and speculation in certain contexts, prioritizing one element over another depending on what was at stake. This activity was an equally important element that helped shape the emergence of the technoscience.

The spectacle of DNA from fossils encouraged its exploration, but as an anomaly it was also a serious source of tension for practitioners who hoped to transform the idea into a credible practice. Here, some scientists experienced a tension as they tried to appeal to the press and public for legitimacy, but simultaneously distanced themselves in an attempt to maintain authority over their practice. Indeed, researchers involved in ancient DNA activity faced a similar situation to the natural philosophers of the seventeenth century as they tried to boost the status of their new experimental philosophy through the spectacle of phosphorus. As Golinski argued, "Wonder could be the parent of philosophy, but only if spectators passed beyond simple admiration and began to exercise their reason to judge the significance of a phenomenon" (Golinski 1989, 24–25). Tkach, for example, speculated about bringing dinosaurs back to life, and while scientists were skeptical about this idea, they were indeed interested in speculation about the preservation and extraction of DNA from insects in ancient amber. The result was the foundation of the Extinct DNA Study Group and a collaboration between Poinar and Wilson at Berkeley. Here, speculation increased interest in, and eventually evidence for, the novel but controversial practice. However, the wonder of ancient DNA research was also a source of uncertainty. NSF's rejection of Wilson's grant illustrates this. Ancient DNA's rarity, the feature that made it stand out among the press and public, was the very feature that made some scientists think twice about investing in a curious but potentially profitless idea. For the proposers, DNA from the quagga was the preliminary and primary evidence on which the proposal stood, but for the panel, this was not evidence enough.

This tension between ancient DNA research as a simultaneous source of science and spectacle would intensify as researchers tried to transform this anomaly from an emergent into an established phenomenon (Chapter Two and Chapter Three). It would also intensify as individuals with multifarious motivations gravitated towards the practice for the press and public interest it elicited (Chapter Two and Chapter Three). Overall, spectacle and speculation – while fundamental features in the starting stages of this technoscience – could only take it so far in its journey from emergence to establishment. At some point, novelty would need to be replaced with evidence of reproducibility (Golinski 1989, 31; Collins 1985; Shapin and Schaffer 1985; Pinch 1986; Collins and Pinch 1993). Indeed, these researchers recognized this, but issues associated with contamination and replication posed serious problems.

1.4.4 Conclusion

This discussion argued that the search for ancient DNA in the late 1970s to late 1980s was about the origination and exploration of ideas that led to a new technoscience. I argued that hype was both indicative of and performative in the starting stages of this technoscience. Researchers suggested it could be a way to study evolutionary history directly through DNA, while reporters, and some scientists, wondered if DNA from fossils could be a way to bring back extinct organisms. The increasing evidence for the preservation and extraction of DNA from fossils led to increasing speculation about the resurrection of ancient and extinct creatures. Both parties, though to differing degrees, contributed to the emerging expectations about the technoscience that propelled the practice into an era of exploration. I also argued that this interplay between science, speculation, and spectacle was imperative to the creation of expectations that contributed to the emergence of ancient DNA research as a technoscience. In this era of experimentation, practitioners faced theoretical and technical challenges that were further complicated by the need to control for contamination and replicate results. They also faced publicity. Ancient DNA, from its beginning, attracted professional and public interest. Researchers realized that if ancient DNA research were to evolve into a credible practice, then its novelty would have to be replaced by routine and reliable research. It was this sense of spectacle that would simultaneously help and hinder its emergence as scientists tried to transform it into an established technoscience.

1.5 CONCLUSION

This chapter outlined ideas regarding the preservation and extraction of DNA from ancient and extinct organisms that arose independently among different people in different places, but that ultimately converged under the direction of Wilson at Berkeley in the 1980s. It also outlined ideas about the hypothetical resurrection of extinct organisms from DNA in ancient material. The search for ancient DNA emerged from the interface of disparate disciplines from paleontology, archeology, and entomology to geology, molecular biology, and chemistry. Individuals, though to differing degrees, were interested in testing hypotheses about DNA preservation and extraction from insects in ancient amber to museum specimens and human remains. Pellegrino and Tkach ventured to imagine instances that would allow scientists to use DNA to bring back dinosaurs. So did Poinar and Hess, but they employed evidence for the microstructural and perhaps cellular and molecular preservation of insects in ancient amber as support for their speculation. Wilson and Higuchi, as well as Pääbo, tested hypotheses of DNA from fossils, and their publications put ancient DNA activity in a professional spotlight. However, the technoscience also attracted media attention. This chapter argued that from its beginning, the search for DNA from fossils elicited press and public enthusiasm. From the beginning, the rhetoric of resurrection was a part of the emerging expectations for the novel, yet controversial, technoscience. Both speculation and spectacle played a part in the emergence of ancient DNA research as a technoscience. Yet as a growing group of scientists navigated needs to control for contamination and replicate results, they also had to learn to navigate the public interest and media exposure that would follow the field and come to characterize the search for DNA from fossils throughout the 1990s.

CHAPTER TWO

FORMING A CELEBRITY SCIENCE: ANCIENT DNA AS A SHARED CONCEPTUAL SPACE

2.1 INTRODUCTION

This chapter is about the history of ancient DNA research from the late 1980s to late 1990s. It outlines the testing and imposing of limits as scientists searched for DNA from paleontological, archeological, and botanical specimens with the new technology of the polymerase chain reaction (PCR). It also outlines the search for ancient DNA as it coincided with the release of Michael Crichton's book and Steven Spielberg's movie *Jurassic Park.* Both the technology of PCR and celebrity of *Jurassic Park* were sources of inspiration for professional and popular audiences interested in this evolving technoscience.

In this chapter, I argue that in the 1990s ancient DNA research developed into a discipline under intense public interest and extreme media exposure. It is during this decade that the interplay between science and media is most evident, ultimately prompting both creative and conservative movements as practitioners sought to transform the technoscience from an evolving to an established practice. In the process, the press created opportunities for publicity which scientists could, and often did, take advantage of for the pragmatic purposes of obtaining further funding for research. Researchers also actively crafted their own opportunities for visibility, while some argued the importance of distancing themselves and their research from speculations about bringing back extinct creatures. Here, celebrity was a crucial component to ancient DNA's disciplinary development, but while media mobilized the practice, it destabilized it, too. Indeed, some scientists felt that media interest or influence was a second source of contamination that affected the credibility of the technoscience. In reaction, some scientists stressed the importance of standardization and replication within the practice. I argue that this interaction between science and media was a critical contributor towards the construction of ancient DNA research into a technoscience and a celebrity science.

2.2 TESTING LIMITS

2.2.1 Introduction

This section is about the influence of PCR, as well as the book and movie *Jurassic Park*, on the search for DNA from fossils in the late 1980s to mid 1990s. PCR was first developed in the US, but it was in the UK that ancient DNA research was first supported on a substantial scale. In the 1990s, some scientists tested the limits of DNA preservation and extraction from fossils. PCR produced a feeling of progress, and the search for the oldest DNA was an artifact of its widespread appeal and application. First, I argue that the innovation, then adoption of PCR made ancient DNA research more routine. Second, I argue that the technoscience evolved under the media spotlight as a series of studies, published in journals such as *Nature* and *Science,* reported the recovery of multi-million-year-old DNA. *Jurassic Park* coincided with these events, and its popularity placed the technoscience and its scientists in the spotlight. Finally, I highlight that ideas that characterized the emergence of ancient DNA research in the 1980s, like the preservation and extraction of DNA from ancient material, and even the resurrection of extinct organisms like dinosaurs, continued to contribute to its disciplinary development into the 1990s.

2.2.2 "A tool of unbelievable power"

By the mid-to-late 1980s, ancient DNA research was attracting professional and popular attention, but its potential was conditional on technology that could amplify decayed and damaged DNA characteristic of old specimens. PCR changed this (Saikia et al. 1985; Mullis and Faloona 1987). PCR was developed in the 1980s by biochemist Kary Mullis and colleagues at Cetus Corporation, a biotechnology company in Berkeley, California.[27] Following several publications and a presentation of its application, it became the most widely used technology in molecular and cellular biology, transforming these fields, along with the related disciplines of systematics, forensics, and medicine. PCR was first presented by Mullis at the Cold Harbor Symposium in 1986; the same meeting where Svante Pääbo first presented his results on DNA from ancient Egyptian mummies, and where researchers first discussed the beginning blueprint for the Human Genome Project ("Symposium Participants" 1986). PCR proved so powerful that Mullis was awarded the

[27] The conceptual, technological, and financial development of PCR is a complex history of interactions between scientists, researchers, and entrepreneurs. See Rabinow (1996).

1993 Nobel Prize for its invention. The advantage of PCR was its automatic amplification of DNA. This took the mental and physical strain out of the previously manual process of cloning via plasmid vectors. Overall, PCR could create billions of copies of DNA sequences from only a few strands, or even just one strand, of DNA. [28] Ancient DNA often occurs in short sequences, and PCR was specifically well suited for amplifying these damaged and degraded fragments. Furthermore, PCR was quick and inexpensive. *New Scientist* called it "a tool of unbelievable power" (Cherfas 1990). Researchers saw it as an opportunity to transform the technoscience.

The application of PCR to ancient and extinct organisms was first attempted at the University of California, Berkeley, where Allan Wilson had close connections with Cetus Corporation. In the 1980s, Wilson sent Russell Higuchi, a postdoctoral researcher in the lab, to Cetus Corporation to learn the method and bring it back to Berkeley. PCR proved promising as Pääbo, a new postdoctoral researcher in the lab, tested its utility and reliability on a series of samples from various ages and environments (Pääbo 1989). The study was both theoretical and technical in its objective, involving a 4-year-old piece of pork, some skin from a Tasmanian tiger, a 13,000-year-old ground sloth, and bits and pieces of mummy material. First, Pääbo was interested in testing the theoretical limits of DNA preservation. He was concerned with the chemical composition of DNA, specifically the modifications that occur through the desiccation of tissues as a result of hydrolytic and oxidative processes. He wanted to understand the properties of and processes that contribute to DNA degradation in hopes of finding observable or generalizable patterns. Interestingly, one feature he found was that the age of the sample did not necessarily correlate to the amount of DNA preserved or the degree to which it was damaged (Pääbo 1989, 1942). Second, Pääbo was interested in testing PCR's technical advantages and disadvantages. Specifically, there were two disadvantages, namely its sensitivity and tendency towards contamination. In order to try to control for contamination, Pääbo suggested "rigorous precautions" when preparing and handling samples, solutions, and materials in the lab. He also recommended taking "multiple extracts" from "different tissues" of the "same individual" to "control for contamination" (Pääbo 1989, 1943). This paper provided a conceptual and methodological foundation for ancient DNA research.

[28] PCR uses repeated cycles of heating and cooling to copy the DNA. First, heat is used to separate double-stranded DNA into single-stranded DNA. The single-stranded DNA is then exposed to primers. The primers attach themselves to the appropriate sites of desired DNA to be amplified. A copy of the targeted DNA is produced. This process continues as a chain reaction with the targeted DNA being exponentially amplified creating millions to billions of copies. See Mullis and Faloona (1987).

Reflecting on the history of the technoscience, an interviewee argued, “What I consider the best paper ever written in ancient DNA is surprisingly written and published in 1989 by Svante Pääbo […]. [T]he field from then, for like potentially twelve to fifteen years, lived on that – not necessarily on *that* paper but on the knowledge and the concepts related to that paper” (8-00:21:00). As early as 1989, Pääbo had suggested a short but succinct list of criteria to control contamination in an attempt to set the standards for ancient DNA research.

PCR transformed the idea of extracting, sequencing, and analyzing DNA from fossils into a potential research program. In the summer of 1989, Pääbo, Higuchi, and Wilson published a piece that tried to situate this development within the broader backdrop of molecular evolutionary biology (Pääbo, Higuchi, and Wilson 1989). They noted the long-lasting “frustration” of “molecular evolution” in “trying to reconstruct this historic process” and that without genetic data from the past it was challenging, if not impossible, to understand evolutionary history over time. They stated, “Until recently, there has been no hope of escaping this ‘time trap.’” (Pääbo, Higuchi, and Wilson 1989, 9709). They said, “The recently achieved ability to study DNA from museum specimens and archaeological finds via PCR opens up the possibility of studying molecular evolution by actually going back in time and directly approaching DNA sequences that are ancestral to their present-day counterparts” (Pääbo, Higuchi, and Wilson 1989, 9712). The paper’s point was to showcase PCR’s power to test hypotheses in evolutionary biology and to establish the emergence of a new field that they called “molecular archaeology” (Pääbo, Higuchi, and Wilson 1989, 9709).[29] Yet the evolving field was accompanied by evolving standards. Another principal point of this paper was to create serious standards for molecular research on paleontological and archeological material. They issued a criteria of authenticity from “control extracts” and “independent extracts” to a “strong inverse correlation between amplification efficiency and size of the amplification product” (Pääbo, Higuchi, and Wilson 1989, 9711). In other words, they suggested that DNA from ancient and extinct specimens should yield shorter sequences, approximately 150-500 base pairs, rather than longer sequences. Sequences longer than this might be a strong signal of contamination from modern material. They argued that contamination could be controlled, and consequently the authenticity and reliability of ancient DNA by PCR maintained (Pääbo, Higuchi, and Wilson 1989, 9712). Overall, with criteria considerations, PCR could be used

[29] The history of molecular archeology, or the search for DNA from ancient humans or sites and sources of ancient human activity, is an eventful history that will not be described in detail in this thesis. See (Jones 2001) and (Pääbo 2014a) for personal perspectives from scientists on this history.

to investigate the evolutionary relationships of extinct and extant species, and the evolution of populations in terms of variations, migrations, and selection.

One of the first charismatic creatures to test the benefits of ancient DNA research and PCR was the Tasmanian tiger. The last Tasmanian tiger died at the Beaumaris Zoo of Australia in 1936. At the time of its death, no one seemed to care that this creature was the last of the species *Thylacinus cynocephalus*. In fact, over five months passed before death of the last thylacine and extinction of the species was announced (Paddle 2000). The thylacine was an unusual animal; wolf-like in face and body, marsupial in anatomy and physiology, carnivorous in appetite, and nocturnal in behavior. It possessed a kangaroo-like pouch and resembled a tiger with a yellow-brown coat and dark stripes across its back. Like the quagga, the thylacine conveyed sentimental charm, but it was also an obvious object of study for its interesting evolutionary history. First, systematists had long argued over its phylogenetic placement. Some argued a closer relatedness to an extinct group of South American marsupials, while others considered the thylacine to be more related to Australian marsupials. The debates rested on fossil evidence but there were distinct differences in interpretation. For example, the thylacine and South American borhyaenids shared similar dental and pelvic traits, while the thylacine and Australian dasyurids exhibited similar hind limbs (Thomas et al. 1989, 465–467). It was one of the first extinct creatures that researchers used to test the power of PCR, and it would also be one of the first extinct candidates that scientists would try to bring back to life.

In the autumn of 1989, biochemist Richard Thomas at University of California, Berkeley, and molecular biologist Walter Schaffner from University of Zurich, Switzerland, along with Wilson and Pääbo, tried to recover DNA from the thylacine (Thomas et al. 1989). For one researcher it was his "first exposure to […] PCR" and although it was an "exciting time" the work was "very intense" with "very long days" (24-00:17:30). Of several samples, only one exhibited evidence of DNA from the thylacine. But with a short mitochondrial DNA (mtDNA) sequence of 219 base pairs, the team could compare it to the mtDNA of six other marsupials. After analysis, they found that the Tasmanian tiger was most related to Australian marsupials, a group called dasyurids that includes the Tasmanian devil. This suggested that the Tasmanian tiger was native to Australia, not South America as some supposed (Thomas et al. 1989, 465). The question seemed solved. However, the morphological data remained inconsistent. Within evolutionary biology, morphological and molecular data provide important but different kinds of information. While both are considered when researchers reconstruct evolutionary history, the two and their traditions are sometimes in tension. For example, while molecular data indicated an Australian origin, morphological data was more consistent with a South American one. However, the team concluded that the thylacine, based on ancient DNA and proteins, originated in Australia (Lowenstein, Sarich, and Richardson 1981). To reconcile these inconsistencies, they explained the similarities between the Australian thylacine and South American marsupials as an example of convergent evolution, where two species evolve similar features independently of one another (Thomas et al. 1989). Overall, researchers were confident in the authenticity of the DNA and the resulting phylogeny of the thylacine. The thylacine study, like the quagga study, helped confirm the significance of ancient DNA research as applied to museum specimens, opening an unexplored, even unimagined, area of research.

PCR led to innovative initiatives in the search for ancient DNA. While the extraction, amplification, and sequencing of DNA from fossils was first explored in the US, it was in the UK that ancient DNA research was first supported on a substantial scale. In November 1988, the Natural Environment Research Council (NERC) funded the Special Topic in Biomolecular Palaeontology, a £600,000 grant to be distributed across four years. The initiative, chaired by chemist Geoffrey Eglinton and organized by geologist Peter Westbroek, included a steering group of biochemists and geochemists (Eglinton 1994). Although centered on the investigation of amino acids, proteins, and other organics, the Biomolecular Palaeontology Special Topic proved invaluable in the conceptual, organizational, and financial development of ancient DNA research as an evolving technoscience. Some took it as an opportunity to test the limits of DNA preservation. One researcher remembered one proposal in particular: "I ended up sitting in [X's] office one day and [X] said, 'What do you think of this grant application?' […] I had a look at it and it was *the most stupid idea*. It was this young team […] and they wanted to get DNA from fossil bones. […]." The response was far from optimistic: "I said, 'Well, DNA is much less stable than proteins. There's no way you could get DNA to survive in fossil bones.'" (9-00:09:00). The idea seemed fantastic and unrealistic. However, the young team had empirical evidence. As this researcher recollected, "[…] I looked at [X] and said, 'This isn't going to work.' And then [X] said, 'Look! […] We've actually got a gel.' And it showed the band [on] the gel and then I said, 'Oh! Well, if they got the band from the gel we should give them funding!'" In the end, "We gave them a positive review and that funded Erika Hagelberg. And the band [on] the gel was the band [on] the gel that then appeared in *Nature* as the first record for DNA recovery from old bones" (9-00:09:00). In

November of 1989, *Nature* reported results by Erika Hagelberg, Bryan Sykes, and Robert Hedges on the "successful extraction and amplification of DNA from human bones between 300 and 5,500 years of age" (Hagelberg, Sykes, and Hedges 1989, 485). One scientist portrayed this paper as a "watershed" moment, as a conceptual contribution to the infant idea that DNA could, and did, survive in paleontological and archeological material including bones, not just skins and tissues (11-00:06:00). This same scientist said, "Twenty-five years ago, people had no idea whether DNA survived in bone, and if it did what to do with it or how to get it out in the first place" (11-10:15:00). This study elicited excitement. It also elicited skepticism.

In 1990, controversy was raised when researchers convened for the Biomolecular Palaeontology Community Meeting at University of Glasgow ("Natural Environment Research Council Special Topic in Biomolecular Palaeontology Community Meeting Programme" 1990). Early practitioners were present, including Pääbo and Hagelberg ("Natural Environment Research Council Special Topic in Biomolecular Palaeontology Community Meeting Accommodation" 1990). One researcher remembered the occasion: "Svante Pääbo, very famously at the meeting, stood up and said, 'Of course you can't get DNA from bone!' – just before Erika Hagelberg stood up and said, 'Here's my results on DNA from bone.'" (9-00:10:15). Another researcher recalled a similar situation: "Svante had […] some very public fights with her in conferences […] saying it was all shit. […]. Svante stood up and said, 'This is shit! It's full of shit. Where are your controls? You haven't got any. And the sequences you have are rubbish!' […] Anyway, it was a big shouting match" (32-00:15:00). One interviewee said "she [Hagelberg] felt very much as if he [Pääbo] was trying to undermine her work at the time" (9-00:10:15). Another said she "didn't take very kindly to that" (32-00:15:00). The disagreement over DNA from bone came down to a disagreement over contamination. One interviewee explained it this way: "[…] [G]iven that you've got human contamination out of everything that had been handled, what do you expect from an ancient Anglo-Saxon bone but an Anglo-Saxon sequence – which is what she had" (32- 00:15:00). A younger researcher recalled not the event itself but the retelling of it: […] "[F]or a long time ancient DNA was about, '[…]. What is possible?' […] [I]s it possible – in the very early days – to get DNA from bone?" This researcher remarked, "I wasn't there at the time, but I heard the story that at one of the first ancient DNA meetings Svante Pääbo said you will never be able to get ancient DNA from bone […]" (15-00:44:00). The sharing of stories about controversy in the early days was part of a process of establishing, then enforcing a narrative about the technoscience (Thompson 2000; Shopes 2002; Summerfield 2004; Thomson 2007). These disagreements over ancient DNA from bone were far from superficial. Rather, the disagreement was indicative of two themes – contamination and competition – that would define, even drive, the development of ancient DNA research in years to come.

A new wave of enthusiasm, then skepticism, confronted the community when Edward Golenberg and colleagues reported the recovery of the oldest DNA to date; 17–20-million-year-old DNA from a fossil *Magnolia* leaf (Golenberg et al. 1990). *The New York Times* ran a report, "Genetic Code Found in 17-Million-Year-Old Leaf," quoting a scientist who said the study was a "'fantastic breakthrough'" and citing the National Science Foundation (NSF) which claimed it was an "'unprecedented achievement'" (*The New York Times* 1990). *The Washington Post* wrote, "Scientists for the first time have read the genetic code of an organism that died between 17 million to 20 million years ago, achieving a record-breaking glimpse into the past based on new techniques that could soon be used on other ancient plants and animals […]" (Booth 1990). *New Scientist* printed six pages on "The Oldest DNA in the World" and wrote, "The discovery of genetic material that may be 16 million years old has left molecular palaeontologists with more questions than answers" (Johnson 1990, 43). But some scientists were skeptical. Pääbo and Wilson reflected enthusiastically but cautiously on the study saying that the conclusion "seems to surpass our wildest dreams" (Pääbo and Wilson 1991, 45). First, they challenged the claim arguing that the sequence, 790 base pairs, was too long. It surpassed Pääbo's suggestion of 150-500 base pairs. Further, the results could not be replicated. Although researchers recovered DNA, they discovered that it was not plant DNA but eubacterial in origin (Pääbo and Wilson 1991; Sidow, Wilson, and Pääbo 1991). In 1991, Pääbo and Golenberg came face to face at the Biomolecular Palaeontology Discussion Meeting at the Royal Society ("Biomolecular Palaeontology Discussion Meeting" 1991). Archeologist Martin Jones said in his account of events, "Up until the *Magnolia* publication, the front runner in the race for ancient DNA was emerging as Svante Pääbo […]" (Jones 2001, 24). However, his role subtly shifted. Jones recounted, "He was no longer simply the bright young star of the field, but was getting used to a new role as traffic policeman in a convoy moving with rather too much momentum for its own safety" (Jones 2001, 25). The technoscience was proving popular to researchers from disparate disciplines, and it was gaining ground with the public, too.

2.2.3 "Glimpses of past worlds"

In July 1991, the University of Nottingham in England hosted a conference called "Ancient DNA: The Recovery and Analysis of DNA Sequences from Archaeological Material and Museum Specimens" ("Ancient DNA: The Recovery and Analysis of DNA Sequences from Archaeological Material and Museum Specimens" 1991). Richard Thomas, formerly at the University of California, Berkeley, and recently relocated as Director of the DNA Laboratory at the British Museum of Natural History in London (now the Natural History Museum), organized the occasion with intentions of bringing the increasing interest in ancient DNA research into a shared space. One researcher remembered the inquisitiveness from an interdisciplinary and international audience of archeologists, paleontologists, and molecular biologists to forensic scientists, the UK Metropolitan Police, and the US Armed Forces (24-00:42:15). As another researcher reminisced, "[E]veryone was really excited. It was a completely unmapped field – getting DNA from dead things. No one had ever done it before" (4-45:35:00). The gathering's goal was exploratory as scientists shared, then compared their research, discussing protocols and problems: "We very consciously did not want to produce a book because the field was not remotely mature enough to do something like that" (24-00:42:15). The breadth of research was broad as scientists discussed both the prospects and problems of the nascent technoscience. Some used ancient DNA to trace the evolution and domestication of plants, while others tested hypotheses about the evolutionary relationships of extinct and endangered animals. A selection of studies focused on human evolutionary history and the sexing of skeletons for kinship. The meeting involved senior and junior researchers alike ("Ancient DNA: The Recovery and Analysis of DNA Sequences from Archaeological Material and Museum Specimens" 1991). Yet enthusiasm was countered by a realism that not every specimen was going to contain DNA and if it did, it would be degraded, damaged, and difficult to determine its authenticity. One scientist said there was "a lot of really ambitious speculation" but also "a lot of realism about what could be done and what couldn't be done" (4-45:35:00). But what began as a modest meeting soon turned into a media frenzy.

In June 1991, *The New York Times* published a piece announcing the meeting but advertising it alongside a recipe for bringing dinosaurs back to life (Browne 1991). The report – "Scientists Study Ancient DNA for Glimpses of the Past" – read: "Will it one day become possible to breed a living dinosaur from genes preserved in fossils? Although most scientists regard such an idea as unrealistic, a few have begun to conclude that it can no longer be dismissed out of hand" (Browne 1991). There was one practitioner in particular who was optimistic and inclined to imagine the impossible. Malcolm Browne quoted George Poinar: "'Obviously, we couldn't reconstruct an extinct animal today, even if we had all its DNA,' he said in an interview. 'However, my belief is that there are dinosaur cells inside biting flies trapped in amber of Cretaceous age and older. It's just a matter of finding the dinosaur DNA and getting it out.'" (Browne 1991). In a series of steps, Browne outlined a "'Recipe for a Dinosaur'" which he credited to George Poinar. Browne admitted that assumptions behind the recipe for resurrection were unrealistic given current technoscientific considerations, but claimed it captured the high hopes that scientists had for the evolving practice: "Analysis of DNA from fossils may open new windows in archeology and paleontology" (Browne 1991). Yet some scientists were circumspect. Browne quoted Pääbo: "It's really impossible to do things like that,' he said. 'It's theoretically possible to isolate the gene for a certain character, and introduce it into another species, if you thought that was worthwhile, which I do not.'" Pääbo continued, "'You could find the gene for the typical quagga color pattern, for example, and introduce it into a zebra. You would end up with something that looked like a quagga, but in reality it would just be a zebra that looked like a quagga.'" (Browne 1991). Despite Pääbo's skepticism, Poinar's speculation that dinosaur DNA may one day be found, as well as a recent report claiming to have found dinosaur proteins, fueled press and public interest in dinosaur resurrection (Browne 1991; Gurley et al. 1991).

In November 1990, Michael Crichton published *Jurassic Park*, a novel in the making since the 1980s (Crichton 1990). Within one month, it became a bestseller, taking the title for three months and receiving international readership after being translated into various languages from Chinese and Japanese to Hungarian (*Jurassic Park* 2017; Shay and Duncan 1993). *Jurassic Park* imagined a science fiction scenario where scientists brought dinosaurs back to life from DNA preserved in the gut of a mosquito trapped in ancient amber. What began as an ethically questionable experiment for an amusement park full of dinosaurs swiftly turned to chaos. Crichton, purposely playing on the recent dinosaur renaissance and the promise and perils of genetic engineering, captivated the professional and popular consciousness. In May 1990, Crichton sent the story to Alfred A. Knopf Publishing and within a week Hollywood was jockeying for the rights to make *Jurassic Park* into a major blockbuster movie (Shay and Duncan 1993). However, before the manuscript had even been proofed, much less published, Crichton had privately offered Steven Spielberg the rights to make the movie for free. Nonetheless, tense bidding ensued and top contenders emerged from Twentieth Century Fox and Warner Brothers to Universal Studios. The latter won the bid, and the rights went to Universal and Spielberg (Shay and Duncan 1993). The popularity of *Jurassic Park* was in part its technoscientific plausibility (Begley 1993). Crichton utilized DNA technology plus studies on DNA from fossils to make his science fiction story convincing: "Genetic material had already been extracted from Egyptian mummies, and from the hide of a quagga, a zebra-like African animal that had become extinct in the 1880s. By 1985, it seemed possible that quagga DNA might be reconstituted, and a new animal grown." Further, "If so, it would be the first creature brought back from extinction solely by reconstruction of its DNA. If that was possible, what else was possible? The mastodon? The saber-toothed tiger? The dodo? Or even a dinosaur? (Crichton 1991a, 68). *Jurassic Park*, coupled with renewed professional and popular interest in dinosaurs, brought the idea of extracting DNA from fossils and using it to bring back extinct species into the popular consciousness (Bakker 1975; Desmond 1975). The combination of science and science fiction placed the pursuit of ancient DNA in the spotlight. Scientists related to the science fiction story had to respond.

While the first meeting put the practice on the map, *Jurassic Park* and *The New York Times* announcement of the meeting alongside a recipe for resurrection put the technoscience in the media spotlight. *Science* ran a report of the conference following its finish, highlighting the fact that the meeting attracted more attention than anticipated. Jeremy Cherfas explained that organizers hoped for a "quiet" and "technical" workshop "[b]ut that was before the science section of *The New York Times* published a fanciful 'recipe' for recreating a dinosaur from ancient DNA" (Cherfas 1991, 1345). Cherfas quoted Thomas: "'We were inundated by people,' says Thomas. 'We were stunned and amazed by the reaction from the press. We had to spend a fair amount of our time telling them, 'No, we are not going to reconstruct the dinosaur.'" (Cherfas 1991, 1345). Cherfas further explained, "However much scientists may protest that it cannot be done, the public and the popular press clearly expect ancient DNA to create Jurassic Park for real" (Cherfas 1991, 1356). With the idea of bringing dinosaurs back consigned to fantasy, Cherfas opted for the next most charismatic creature, the woolly mammoth. However, even shifting expectations from a less ancient but equally engaging organism, scientists were still doubtful. Higuchi told Cherfas: "'The amount of mammoth DNA is enough that in theory a dedicated graduate student could reassemble the entire mitochondrial genome. So we could have elephants walking around carrying mammoth mitochondrial DNA sequences.'" However, there was a practical problem. "'It would make absolutely no difference,' he says. 'They'd still be elephants.'" (Cherfas 1991, 1356). Even the optimism for the hypothetical resurrection of extinct life, as showcased in *The New York Times*, was balanced by a strong sense of skepticism. Nonetheless, some scientists saw advantages in adopting the rhetoric of resurrection to communicate their real research. One interviewee presented this perspective: "But the 'clone-me-a-dinosaur' faction, people are obviously going to get excited about that […], and there were a few of us, I think, at the time that were happy to piggyback off that interest to get funding and so on" (24-00:49:30). The fascination with dinosaur resurrection was a special space where both researchers and reporters met to discuss or debate the expectations of the new technoscience.

While the press and public may have been disappointed that dinosaurs were less than likely to make a comeback, scientists found the conference rewarding. The conference was an interdisciplinary and international enterprise bringing together scientists from disparate disciplines such as archeology and anthropology to botany, paleontology, molecular biology, and forensic science. They were unified in their study of the preservation and extraction of DNA from ancient and extinct organisms. They were also interested in its applications to evolutionary history, systematics, and phylogenetics. Overall, the meeting conveyed that the pursuit for ancient DNA was a worthy and exciting endeavor. As Cherfas announced, "They found that molecular biology may be on the brink of revolutionizing archeology and paleontology, just as it had earlier revolutionized population genetics and evolutionary biology" (Cherfas 1991, 1354). Although theoretical and technical hurdles were high, particularly as practitioners tried to control contamination and replicate results, they continued to test the limits in order to transform the practice from an evolving to an established technoscience. Cherfas presented this perspective: "Despite the remaining technical problems, ancient DNA is no longer just a curiosity but an area where systematic studies can produce insights unavailable by any other technique." According to Cherfas, this had immediate implications: "For archeologists, anthropologists, and paleontologists the message is clear – the time has come to ensure that textbooks on the polymerase chain reaction and gene cloning are on the bedside table" (Cherfas 1991, 1356). Cherfas captured the conclusion of the meeting, claiming that in the end "they found they had created a new field" (Cherfas 1991, 1354). But its novelty was accompanied by celebrity as some searched for the oldest DNA. Golenberg, despite the fact that he appeared to hold the record for the oldest DNA, advised against making this the goal. Cherfas quoted Golenberg: "The object is not necessarily to see who can get the oldest DNA," Golenberg insists, "but actually to start working up research projects that can make sense" (Cherfas 1991, 1356). PCR had facilitated the founding of a field, and with the backing of organizational and financial initiatives, this growing group found themselves treading on unexplored territory in evolutionary biology.

By 1991, a community had consolidated around the name of "Ancient DNA" and started to communicate professionally, but informally, about the expectations of the developing discipline. Their first thought was to create a journal, but researchers reasoned that the work might be too premature to properly support its content and continuation. The second thought was to generate a newsletter (24-00:42:15; 32-00:52:30; Wayne 1991; Wayne and Cooper 1992a; Wayne and Cooper 1992b; Wayne and Cooper 1994). Robert Wayne, evolutionary biologist at the Zoological Society of London, and Alan Cooper, a graduate student at University of Wellington, New Zealand working at Berkeley with Wilson and Pääbo, accepted "the dubious honor of being the first editors" (Wayne 1991, 1). In a letter to the community, they introduced the "Ancient DNA Newsletter" as an informal and professional place for ideas, procedures, and problems related to research on DNA from ancient and extinct material. They intended on issuing the newsletter biannually, and the style of the newsletter was scientific but fun and free from peer-review pressures. One section – "Research news" – contained research results with the idea of creating collaborations between labs. "Protocols" highlighted new practical and analytical techniques in the lab, while "Articles" outlined detailed research descriptions. Other sections were more for amusement. For example, the "Editorial" section featured "advice" and "restaurant reviews" and the "Personals" highlighted "general gossip" and short research statements with the "intent of building bridges between laboratories with common interests" (Wayne and Cooper 1992a, 2). There was even a special space – "Dr Russ' problem corner" – where scientists could submit questions about their technical troubles and receive a response from Higuchi in the next newsletter (Wayne and Cooper 1992a, 6–8). This newsletter was a space for scientists to construct a culture of professional and philosophical values about the technoscience. As an interviewee argued, "[…] [I]t was a way of really standardizing the techniques and information and methods that were going on in the field. And it was pretty important at the time in terms of solidifying the field as an entity rather than people just using ancient DNA for quite different things" (32-00:52:30). Overall, 1991 was a significant year with major movements towards the discipline's development. While a significant time, it was also a somber time with the premature passing of one of its founding fathers (Sanders 1991). Wilson, who had been living with leukemia, passed two weeks after the first ancient DNA meeting. Nonetheless, scientists continued the search for ancient DNA to continue the discipline he helped to start.

2.2.4 "Here come the DNAsaurs"

In the 1990s, scientists once again turned to the task of recovering DNA from insects in ancient amber. Entomologist David Grimaldi had dedicated his life to studying amber, and during his days at the American Museum of Natural History (AMNH) in New York, he spent his time building a comprehensive collection. Grimaldi, like Charles Pellegrino and George Poinar, argued that amber could be a time-capsule for cellular and molecular preservation (Pellegrino 1985; Poinar Jr and Poinar 1994; Grimaldi 1996). Grimaldi teamed up with molecular biologist Rob DeSalle, also at the AMNH, to embark on an experiment. One researcher involved remembered the partnership this way: "All I know is that around 1993, [X] walked into my office one day and said, 'Have you read *Jurassic Park*?' […] [H]e said, 'We should try it. Let's crack open some insects and see if we can get DNA out of it.' And that was the only thing I knew. That's when our conversation started. […]" (17-00:36:40). Another practitioner recalled a slightly different situation: "[Y] […] came to us one day and said, 'Look. [There's] this fossil, and [there's] twenty others […] – and I'm sure you can get DNA out of it.' […] That's what stimulated it" (18-P2-00:13:30). How the collaboration convened is difficult to determine due to differences in memories, but it is important to note that *Jurassic Park* played a part in these accounts (Thompson 2000; Shopes 2002; Summerfield 2004; Thomson 2007). The narrative of *Jurassic Park*, whether real at the time or a result of retrospection, played a role in the research itself, or at least the retelling of it. Yet the study was more than an attempt to recover DNA from ancient amber. The study also centered on an evolutionary enigma. Like the quagga and thylacine, mastotermes, a type of termite, had puzzled researchers about its relatedness to other insects. One interviewee said, "We wanted […] something […] of […] phylogenetic significance, not just 'Is there DNA?' […] And that's one of the reasons why we decided on mastotermes, because at the time there was a controversy and discussion about 'Are termites roaches or are they closely related to roaches or are they outside?' […]" (17-00:58:45). With a pristinely preserved termite in ancient amber, scientists went to work.

In the autumn of 1992, Grimaldi and DeSalle, along with John Gatesy and Ward Wheeler, claimed to have extracted and sequenced DNA from a 25–30-million-year-old *Mastotermes electrodominicus* (DeSalle et al. 1992, 1936). *Science* published the paper.

Contamination was an issue, and the team took prescribed precautions such as negative controls, extraction blanks, and phylogenetic comparison to affirm DNA authenticity (DeSalle et al. 1992, 1936). They announced the achievement as “the oldest DNA extracted from a fossil” and it caused a remarkable media response (DeSalle et al. 1992, 1934; Browne 1992; Rensberger 1992; Hoppe 1992; Morell 1992). The topic of amber, particularly DNA from insects in ancient amber, captivated the press and public because of *Jurassic Park*, and a study that seemed to confirm the science fiction story caused further curiosity. One researcher remembered “an enormous amount of media requests” for “writing” and “filming” on the topic of “ancient DNA” (17-00:43:35). According to this researcher, scientists and scientific institutions benefited from the hype: “[T]he AMNH had built our first molecular lab […], and it kind of gave the museum a lot of mileage […][.] ‘Wow! See what our molecular lab has just done!’ It was maybe a few years old and these results were coming out of the molecular lab. So, the museum got a lot mileage out of it” (17-01:02:15). This institution also optimized the opportunity for publicity through an extraordinary traveling exhibition titled “Amber: Window to the Past” (“Amber: Window to the Past” 1996). Individuals like Grimaldi took part too, publishing a book and writing a feature for *Scientific American* (Grimaldi 1996a; Grimaldi 1996b). As a researcher argued, “There is *no question* that it rode on the heels of *Jurassic Park*. Without a question. The museum played it up. Everyone did. There was a lot of promotion. […]” (17-00:52:00). The search for ancient DNA continued with the help of public interest, but it also depended on scientists’ and scientific institutions’ decisions to capitalize on opportunities that would raise its public profile (discussed in detail in Section 2.4.2).

From New York to California, research resumed as George Poinar stepped back into the spotlight in pursuit of DNA from ancient amber. But this time he returned with his son Hendrik Poinar, a student at California Polytechnic State University, and Raul Cano, a microbial ecologist also at California Polytechnic State University. One researcher called the collaboration “serendipitous.” As this researcher recalled, “*Jurassic Park*, the book, had just came out and we were sitting in a laboratory and I was doing something and [X] was doing something else, and the topic of *Jurassic Park* came up. And [X] said, ‘My name is [X]. Would you be willing to help me test the *Jurassic Park* concept and extract DNA from amber?’ […].” This researcher replied, “I’m not really one to back out from a challenge so I said, ‘Sure. Let’s do it.’” (31-00:14:20). With a 25–40-million-year-old amberized bee (Apidae: Hymenoptera), the team took on the challenge. One scientist said, “I think the first challenge was really getting the DNA out and without presumed environmental contamination, and the second, and most important thing was, trying to convince ourselves that what we were getting was actually real DNA.” However, “The most exciting thing was seeing the first faint band in the gel after a PCR” (31-00:16:40). *Medical Science Research* reported the research (Cano, Poinar, and Poinar Jr 1992). In the media, journalists stressed the close connection to *Jurassic Park. The Washington Post*, for example, called it “a case of science imitating art imitating science” (Rensberger 1992). *The Washington Post* also framed the recent research in terms of a rivalry between two teams, Cano’s group in California and DeSalle’s group in New York, in a race for the oldest DNA: “Rival research teams have found that fossil insects embedded in amber for as long as 30 million years still contained DNA fragments […]. The DNA samples, found in extinct species of termites and bees, are said to be the oldest yet discovered” (Rensberger 1992). With race rhetoric came continued speculation about DNA from dinosaurs. In *The New York Times*, Browne repeatedly raised this possibility: “But paleobiologists and science fiction buffs dream of obtaining DNA still older than that recovered from ancient termites and bees – perhaps even DNA from dinosaurs” (Browne 1992). Browne specifically spotlighted *Jurassic Park* and scientists like George Poinar who entertained this resurrection rhetoric: “‘Sooner or later,’ Dr. Poinar said, ‘we’re going to find amber containing some biting insect that filled its stomach with blood from a dinosaur before getting trapped in the resin that eventually turned into amber. The blood may contain actual dinosaur DNA. That will be an exciting discovery.’” (Browne 1992). While Crichton’s *Jurassic Park* influenced the technoscience in terms of motivating, then disseminating research, Spielberg’s movie in the making boosted its public profile to a new level.

Following their first publication, Cano and colleagues broke their rival’s record for the most ancient DNA with a second study claiming to have extracted and sequenced DNA from a 125–135-million-year-old amber-encased weevil (Nemonychidae: Coleoptera) (Cano et al. 1993). It was the oldest DNA from an insect in ancient amber, and the paper was published by *Nature* on June 10 of 1993 – one day after the *Jurassic Park* premiere and one day before its public release in movie theaters across the United States (Cano et al. 1993; “Jurassic Park (1993)” 2017; Kirby 2013). The press took notice, as well as practitioners internal and external to the field of ancient DNA research. Browne, who consistently covered ancient DNA research for *The New York Times*, commented on the connection: “The report of the achievement is being published today in the British journal *Nature*, one day before the opening of ‘Jurassic Park,’ a much-publicized movie based on the notion of cloning extinct dinosaurs from their surviving DNA […]" (Browne 1993a). However, some saw the timing in a far from positive light. One interviewee, for example, offered a rather negative remark about this event: "[…] I thought it absolutely extraordinary that a scientific journal – there was no way it was a coincidence – that a scientific, *a prestigious scientific journal*, like *Nature* would hold on to an article to wait for the opening day of a movie. […] [O]f course, that caused a *huge* media splash." While the timing helped promote Cano and colleagues' work, it also propelled rival research on DNA from ancient amber. This researcher recounted, "It just kind of propelled our work and that fed it even more. […] [E]ver since the movie came out, everyone knew what amber – [laughs] – was all of the sudden! [Laughs]. They knew about amber because of *Jurassic Park*" (17-01:02:15). But others viewed this interplay between science and media as a positive phenomenon. Paleontologist Stephen Jay Gould, for example, offered his own observations at the time about the timeliness of it all: "The nearly complete blurring of pop and professional domains represents one of the most interesting spinoffs – a basically positive one in my view – of the *Jurassic Park* phenomenon. […]. When a staid and distinguished British journal uses the premiere of an American blockbuster to set the sequencing of its own articles, then we have reached an ultimate integration" (Gould 1996, 225–226; Kirby 2013, 139). Media, for better or for worse, was a component of this developing discipline, and this particular interplay between the two placed the technoscience in a celebrity spotlight (discussed in detail in Section 2.4.2).

In the summer of 1993, *Jurassic Park* was released to audiences across the world. The opening weekend in the US earned approximately $50 million alone. It grossed over $1 billion worldwide that year, taking the title of the highest-grossing film ("Jurassic Park (1993)" 2017b). It won three Academy Awards for Best Sound, Best Sound Effects, and Best Visual Effects, as well as over twenty other awards, including international awards ("Jurassic Park Awards," 2017). The blockbuster was also accompanied by a colossal marketing campaign of approximately $65 million in deals with around 100 companies distributing 1,000 products from toys and sleeping bags to a ride at Universal Studios in Florida. *Entertainment Weekly* joked, "If dinosaurs had been marketed half as well as they're going to be in Steven Spielberg's $60 million-plus *Jurassic Park*, they would never have become extinct" (Broeske 1993). The success of *Jurassic Park* and its impact on both popular and professional science can also be attributed to the Hollywood blockbuster phenomenon (Hall and Neale 2010). As scholars suggested, the rise of the blockbuster in the 1980s and 1990s led to a desire for realism in film and an increasing interest of filmmakers to make the unreal images or events appear real (Hallam and Marshment 2000; Black 2002; Kirby 2013). *Jurassic Park* was fitting of the time and filmmakers were feeding into audience anticipation of the big-budget blockbuster.

Indeed, much of *Jurassic Park*'s success relied on realism, as well as the reputation of the writer, Crichton, and definitely that of the director, Spielberg, who had already delivered at least four Hollywood hits (McClintock 2015). However, much of the movie's fame also relied on technology, namely the use of computer-generated images (CGI) (Pierson 1999; Pierson 2002). Michele Pierson, film studies scholar, wrote, "In the build-up to *Jurassic Park*'s release, speculation about the film's computer-generated dinosaurs generated by far and away the most publicity for the film" (Pierson 1999, 166). Pierson explained, "In the first scene in which one of the much-anticipated computer-generated dinosaurs is finally unveiled – both to the characters in the film and to the audience in the cinema – the narrative all but comes to halt, the music gradually builds, and shots of characters reacting to the appearance of the dinosaur with wonder and amazement are interspersed with long takes displaying the computer-generated brachiosaur centre-screen" (Pierson 1999, 167). The innovation of CGI and its introduction in *Jurassic Park* was a technical and aesthetic achievement. Here, the minds behind the movie used technology to help make the dinosaurs brought back to life look like a reality. GCI helped make this happen. Kirby explained the movie's commercial and critical success this way: "The appeal of *Jurassic Park*, for example, is entirely predicated upon breaking people's ingrained beliefs of dinosaurs as slow, lumbering, and dull beasts. To achieve this, though, they had to create a film where the plot, dialogue, character interactions, special effects, and sound effects – as well as a high-profile PR campaign utilizing their science consultant Jack Horner and other famous paleontologists – all conveyed the idea that agile, smart, dinosaurs were natural." Therefore, "For this movie, challenging deeply held cultural beliefs provided far greater box office rewards than giving the public what they expected dinosaurs to be" (Kirby 2013, 106). The spectacle of state-of-the-art CGI technology, in terms of its anticipation as well as its production, was a a crucial component to movie's success.

Yet timing and the actual science behind the science fiction was part of Crichton's and Spielberg's success, too. In a four-page print in *Newsweek*, "Here come the DNAsaurs," Sharon Begley noted that the popularity of the book and movie dependeding on timing: "All great science fiction must be science first and fiction second. Even more, it must tap into the reigning scientific paradigm of its era. For Mary Shelley's 'Frankenstein,' that paradigm was electricity […]. For Godzilla, it was radioactivity and the Bomb. For 'Jurassic Park,' it is biotechnology." Begley quoted Crichton, "'Biotechnology and genetic engineering are very powerful,' he says. 'The film suggests that [science's] control of nature is elusive. And just as war is too important to leave to the generals, science is too important to leave to scientists. Everyone needs to be attentive.'" In addition to the timing, the plausibility of the story rested on the science behind it. Begley quoted Spielberg: "'This movie depends on credibility, not just the special effects,' Spielberg told NEWSWEEK. 'The credibility of the premise – that dinosaurs could come back to life through cloning of the DNA found in prehistoric mosquitoes trapped in amber – is what allowed the movie to be made.'" (Begley 1993, 57). The fact that the plot was so closely connected to the search for DNA from fossils, a real and burgeoning technoscience, made the outrageousness of bringing dinosaurs back to life by means of DNA preserved in ancient amber seem theoretically possible. Scientists involved in the technoscience recognized the link between their research and *Jurassic Park*. Some recognized the high-profile status of the movie among the public and took advantage of opportunities to align themselves with the spotlight.

The press and public interest in *Jurassic Park* offered opportunities for scientists to publicize their research. George Poinar, reflecting on the publicity of their publication, said it "gained instant popularity" and "made the front pages of 257 newspapers in the United States and 400 newspapers worldwide" which made it the "most complete coverage of a single science news item in the past twenty years" (Poinar Jr. and Poinar 1994, 154). But he claimed the timing was "coincidence" (Poinar Jr. and Poinar 1994, 154). Coincidence or not, the news hit headlines and the movie offered publicity opportunities that scientists could, and in this case did, take to their advantage (King 1993; Kirby 2013). The *Los Angeles Times* covered the opening weekend of *Jurassic Park* and specifically noted the intimate interaction between science and science fiction. Journalist Peter King set the scene with a lobby outfitted with movie goers and bits and pieces of amber for sale for $2.00 a piece. Hendrik Poinar was running the raffle for the pragmatic purpose of raising funds for future research. King recalled Hendrik Poinar and his role in publicizing the science and science fiction behind *Jurassic Park*: "'Step right up,' barked the stocky, fresh-faced young man in a polka-dot tie. 'Step right up and see the real science. We got it. Right here'" (King 1993). However, the real star of the show was Cano. King said, "From Newsweek to 'Nightline' to a newspaper in Lebanon, everyone wanted a piece of the professor" (King 1993). King quoted Cano: "'And what they all really want me to say,' Cano said, 'is that this is possible, that we can clone dinosaurs'" (King 1993). Cano's answer, like Pääbo's and Higuchi's, was far from positive. King wrote, "Unfortunately, he explained, this cannot be done now, will not be done ever and, even if it could be done, probably should not be done – for a whole host of moral, ethical and practical reasons. But why spoil a good story?" (King 1993). In this case, these scientists saw *Jurassic Park*'s popularity as a chance to promote their own image and that of technoscience by placing their research front and center with the recent movie release.

In addition to publicity, *Jurassic Park* influenced grant funding. In 1993, Jack Horner, paleontologist, as well as scientific advisor to Spielberg on *Jurassic Park*, proposed a project to NSF to search for DNA not from insects in amber but from dinosaur bone. The proposal was in part inspired by some odd observations that Mary Schweitzer, a graduate student working with Horner at Montana State University and Museum of the Rockies, encountered when analyzing a few bone fragments in the lab. Under the microscope, Schweitzer observed strange shapes in a thin section of bone from *Tyrannosaurus rex*. Those strange shapes looked like red blood cells, and additional analyses suggested other soft tissue structures, perhaps even proteins or DNA, might be preserved too. Schweitzer and Horner wanted to test this hypothesis (16; 39; Horner and Vyse 1993). The project, "An Attempt to Extract DNA from a Cretaceous Dinosaur *Tyrannosaurus rex*," asked for approximately $35,000 over a two-year period to try to discover dinosaur DNA (Horner and Vyse 1993). The grant was funded the same year the movie was released. According to one person on the project, the correlation between funds and film was no coincidence: "It's hard to get money. I think NSF gave us money at that time just because of the movie. […] [I]t was the perfect time for it. They weren't going to do it before then" (16-00:25:25). Not only did NSF approve the award, but they also scheduled a press release to coincide with the opening weekend of *Jurassic Park* (Macintyre 1993, 16; Kirby 2013, 139). *The New York Times* reported the same story quoting NSF: "'We thought it would be a good opportunity to get the word out on 4 of the 10 dinosaur research projects the N.S.F. is funding this year, including that of Mr. Horner […]'" (Browne 1993b). *Science* participated in the publicity with an article, "Dino DNA: The Hunt and the Hype," featuring Schweitzer and a section called "The Arduous Game of Extracting Dinosaur DNA" which included step-by-step instructions for determining whether or not you have dinosaur DNA. In the end, Schweitzer and Horner found DNA but could not confirm if it was dinosaur DNA. One interviewee explained, "[…]. I mean, I could tell you there was DNA in the bone. […]. It's just whose DNA was it was the question. […]" (39-00:15:30).

These interactions between scientists and media produced positive, as well as negative, effects.[30] One author who was part of the project to try to extract DNA from insects in ancient amber recalled the research as "exciting" but also "annoying" and "very troubling" (31-00:50:30). This author attributed concerns to the extreme media exposure that accompanied their publications: "I had more than my fifteen minutes of fame […]" (31-00:46:30). For example, "In one day, we must have spoken with 200 different journalists […] – and it was just amazing! In a way, I'll never do that again. Next time I'll just go to Hawaii and leave my cell phone behind. […]" (31-00:50:30). For this scientist there was a marked difference in the reception of past work and this recent research on ancient DNA. Prior to publication of the 1993 study, this researcher had received very few media inquiries: "[T]he work that I was doing was not particularly interesting to anybody. […]. I never did anything that was earth-shaking. Nobody cared. […] [T]he work that I did was *good* work, but it was a non-issue from the point of the media" (31-00:48:15). The difference between the former and latter was a difference in news value: "That's the difference – work that you do that sells newspapers or gets airtime" (31-00:49:00). Another interviewee searching for dinosaur DNA felt the effects of fame, too. This researcher remembered coming home to the answering machine full of calls from the press: "[…] [I]t was full – *completely* full with media people. […] I was like front page news *everywhere*. […] It was awful. Awful, awful, awful. Horrible!" (39-00:11:30). For this researcher the pressure was too intense and influenced a decision to turn away from ancient DNA: "I also learned that I don't want to work with DNA – ever. Ancient DNA. Nuh uh. Not ever" (39-00:17:45). Here, practitioners subjected to intense public interest and extreme media exposure opted out. Others took notice too, arguing that attention was disproportionate and distracting from soberer but significant research. *Science*'s "Dino DNA: The Hunt and the Hype" wrote, "Several groups are racing to get the first DNA out of dinosaur bones, but other researchers say their efforts are taking attention away from the real scientific value of ancient DNA" (Morell 1993, 160). Overall, the race for dinosaur DNA was a nexus of attention, competition, and even frustration for the growing group of ancient DNA researchers.

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[30] Researchers like Raul Cano and Jack Horner expressed positive and negative effects of media regarding their research and its association with the release of *Jurassic Park*. David Kirby highlighted these interactions and their implications for understanding relationships between science and media, specifically in how science influences, or is in turn influenced by, Hollywood. This will be analyzed in the discussion (Sections 2.4.2 and 2.4.3). See Kirby (2003a), Kirby (2003b), Kirby (2013), and Kirby (2014).

In the professional sphere, conversations were less about dinosaur DNA and more about how to transform the emergent technoscience into an established practice. To professionalize the practice, researchers focused on methods and the role of their research within evolutionary biology broadly. In 1993, the Second International Ancient DNA Conference was hosted at the Smithsonian Institution in Washington, D.C. ("Ancient DNA: Second International Conference" 1993). Over the course of three days, the meeting mainly featured technical talks on the biochemistry of DNA in terms of oxidation and radiation damage, chemical modifications, as well as sampling, extraction, and amplification techniques. Some explored theoretical explanations for why certain sources, like amber or dentin of teeth, might be better storehouses for DNA. Others highlighted the relevance of ancient DNA in evolutionary biology in terms of testing hypotheses about human evolution, migration, and colonization. While an exploratory era of research with practitioners attempting to recover DNA from various sources and apply the data to numerous areas of study in evolutionary biology, standardization was becoming increasingly important. The novelty of recovering DNA from fossils was exciting, but some scientists stressed the need for novelty to be replaced by evidence of reproducibility. This was something stressed by their focus on the molecular behavior of DNA as well as techniques to that may inhibit or increase the chances of recovering DNA. Understanding the regularities of biochemical behavior and perfecting experimental processes was a crucial component to the field's credibility.

Nonetheless, the hunt for the oldest DNA, particularly dinosaur DNA, continued. In "Going for the Old: Ancient DNA Draws a Crowd," Joshua Fischman, reporting on the meeting, wrote, "While rejuvenated celluloid dinosaurs have grabbed headlines this year, these scientists were more concerned with topics such as tracing ancient human populations and understanding how DNA can survive the millennia" (Fischman 1993, 655). While Fischman tried to foreground scientists' shift to methods and topics like human evolution and migration, the race for recovering multi-million-year-old DNA continued to be a hot topic: "Time and again, the best preservative for ancient DNA and the ancient tissue that holds it has proved to be amber. The current longevity record belongs to DNA from a weevil entombed in amber 120 million to 135 million years ago (*Nature*, 10 June, p. 536) […]" (Fischman 1993, 655). In 1994, however, scientists reported the recovery of 80-million-year-old DNA, not from an insect in amber but from a bone fragment from a coal mine in Utah (Woodward, Weyand, and Bunnell 1994). Although scientists did not claim that the bone or the DNA from it was dinosaurian in origin, they did make a slight suggestion of it. Towards the end of their article in *Science*, they wrote, “On the basis of the circumstantial physical and geologic evidence, it is likely that the bone fragments belong to a Cretaceous period dinosaur or dinosaurs” (Woodward, Weyand, and Bunnell 1994, 1230). For the most part, the press noted that the bone and the DNA from it remained unidentified. However, the story that hit the headlines was that scientists, despite this uncertainty, were confident that sequences were of dinosaurian origin. In the press, Scott Woodward – first author on the article – was quoted as being “confident” that this was the case (*New Scientist* 1994; Hotz 1994; Wilford 1994). *Science News* featured the feat in an article titled “Dinosaur DNA: Is the Race Finally Over?” (Monastersky 1994).

2.2.5 Conclusion

This section spotlighted the search for the oldest DNA. Both the technology of PCR and the celebrity around *Jurassic Park* drove professional and public interest in the practice. First, I argued that PCR facilitated the foundation of ancient DNA research as scientists used it to test its theoretical and technical limits. PCR made research routine, but its primary problems were its sensitivity and tendency towards contamination. Pääbo, as early as 1989, offered a short but succinct list of criteria while other studies, like the thylacine study, demonstrated the utility of ancient DNA research as applied to evolutionary history and phylogenetics. The NERC Biomolecular Palaeontology Special Topic, a £600,000 four-year fund granted to UK scientists and scientific institutions, was the first to fund it on a significant scale. Second, I argued that the technoscience developed into a discipline under the influence of press and public interest. The first meeting, in 1991, put ancient DNA research on the map. However, *The New York Times* announcement of the meeting alongside a recipe to bring dinosaurs back put the technoscience in the media spotlight. This interplay between science, speculation, and spectacle intensified as a series of studies in the US, published in high-profile journals like *Nature* and *Science*, reported DNA from multi-million-year-old fossils from a 20-million-year-old leaf to a 135-million-year-old insect in amber. Hype peaked when *Jurassic Park* was released, influencing scientists’ and scientific institutions’ visibility, publishing, and funding. For example, institutions and individuals at the AMNH capitalized on the book’s and movie’s popularity to raise their public profile and that of the technoscience. So did Cano, George Poinar, and Hendrik Poinar, as well as Horner, Schweitzer, and the NSF. This section also argued that ideas like the preservation and extraction of DNA from fossils and the resurrection of extinct creatures aided disciplinary development. Crucially, scientists were not trying to clone dinosaurs. Indeed, they often ousted the idea as impossible or ethically and practically questionable. They were, however, working within a context in which science and science fiction were intimately intertwined and their work was often framed within a rhetoric of resurrection. Together, the interplay between science and media helped co-construct a technoscience and a celebrity science.

2.3 IMPOSING LIMITS

2.3.1 Introduction

This section is about defining limits of ancient DNA research from the early to late 1990s as researchers struggled with credibility concerns. First, I argue that enthusiasm turned to skepticism as scientists internal and external to the technoscience questioned the authenticity and reproducibility of results. Second, I argue that researchers responded to *Jurassic Park* hype with a more conservative movement. Here, researchers urged the use of criteria of authentication and the study of less geologically ancient but still scientifically significant species under 100,000 years old. For practitioners internal and external to the field, replication was key to the success of ancient DNA activity as a rigorous and reliable approach to studying evolutionary biology. Finally, I suggest that during this decade, ancient DNA research developed into a discipline via conferences, collaborations, newsletters, and financial initiatives. However, this growth played out publicly in the media spotlight, shaping the discipline's development through a response to increasing issues associated with the technoscience and its celebrity status. In an evolving and expanding community, policing became public. Within this community, a sub-community formed that tried to down play spectacle and speculation by emphasizing methodology and replication via standards of experimental expertise. By the end of the 1990s, the community confronted two issues; how to control contamination and how to control celebrity.

2.3.2 "The PCR police"

In 1993, NERC's Biomolecular Palaeontology Special Topic, the first official funding towards ancient DNA research, came to a close. In 1994, researchers reflected on it. While the initiative focused on the preservation of proteins and other organics in fossils, the search for ancient DNA emerged as a highly popular and promising component of the program. The Biomolecular Palaeontology Special Topic report read: "Finally, there is the brave new world of DNA/RNA analysis. This field is moving very fast and sometimes receives major media coverage e.g. Michael Crichton's 'Jurassic Park' book, film and associated newspaper and TV coverage" (Eglinton 1994, 3). The report also referenced funded research by Hagelberg and John Clegg at Oxford along with Terry Brown and Keri Brown at Manchester that demonstrated the value of DNA from archeological and paleontological material. Overall, the report saw the once embryonic but now evolving technoscience as a shining success via recognition from prestigious publishers: "The area is already warming up nicely and promises well in terms of stimulating scientific discussion (see *Nature* 1993, *365*, p 700 and *New Scientist*, 29 January, 1994, pp 38-41)" (Eglinton 1994, 4). So, with its ending came the beginning of a second funding strategy, also awarded by NERC in 1995, called the Ancient Biomolecule Initiative (ABI) (Eglinton 1995; Eglinton, Knowles, and Edmunds 1998). Two interviewees said the ABI was rumored to have been awarded because of *Jurassic Park* (9-01:34:15; 46-00:42:25). One article announced, "The world-wide success of the film *Jurassic Park* has highlighted the need for projects funded by the Ancient Biomolecules Initiative." This article argued, "Partly in response to the high profile that the film brought, the Natural Environment Research Council is providing the Ancient Biomolecules Initiative with about £2m for this area of research over a period of around five years." The film attracted public attention but engendered scientific skepticism too: "Since the film broke box office records, many learned articles and reviews have sought to undermine its hypothesis, but have failed to detract from the public's interest" (*Molecular Biology* 1994, 5). Indeed, one specific ABI study was designed to test the hypothesis of DNA from insects in ancient amber based on their own work that seemed to challenge the previous papers claiming multi-million-year-old DNA (Smith 1995). The idea of *Jurassic Park* influenced research, even if it was research trying to disprove the *Jurassic Park* concept once and for all.

The expanding community acknowledged their role as a technoscience in the limelight, and some saw a reason to respond to the more sensational, perhaps questionable, studies. In the second "Ancient DNA Newsletter," circulated in 1992 (after the book but before the movie), "Dr Russ' problem corner" featured both a technical and editorial concern for the field. The former discussed PCR and primer problems, while the latter dealt with issues around the increasing public interest in cloning dinosaurs. Higuchi said, "Unlike many people, I am not eagerly awaiting the imminent opening of the movie version of 'Jurassic Park', even if it's directed by Steven Spielberg." Higuchi argued, "This sort of thing has given and continues to give me an uneasy feeling. Maybe I'm being paranoid, but I believe the public acceptance of this and other gross overstatements of the capabilities of DNA technology leads to an unreasonable fear of it" (Wayne and Cooper 1992b, 6). Higuchi wanted to draw distinctions between science and science fiction. For Higuchi, current technical capabilities were a way to define the boundary. For Higuchi, there was a time and place for speculation, and as a scientist in certain contexts, too much speculation did more harm than good. Higuchi advised, "When you get asked (and in the wake of Jurassic Park, the movie, it seems inevitable that some of you will) whether the resurrection of dinosaurs from ancient DNA is possible, I hope you will say it is not." He admitted, "Although it is fun to say, 'in theory, it may be possible (nudge, nudge – wink, wink)', let's get real" (Wayne and Cooper 1992b, 6). *The New York Times* wrote about this worry, too (Browne 1993c). However, expressing, then enforcing, these boundaries was difficult: "I myself have been guilty of allowing this romantic – if not gothic – notion, the resurrection of extinct species, to colour reports of our work (it is hard to keep the Media from focusing on that)." Yet Higuchi urged scientists to find a balance between professional and popular expectations: "It now seems clear to me that the responsible thing to do is to try as much as possible not to overstate the power of new technology, in the field of ancient DNA or elsewhere" (Wayne and Cooper 1992b, 6). Overall, practitioners in pursuit of ancient DNA were conscious they could determine the direction of dialogue.

Ancient DNA's unconventional claims and public profile attracted suspicion from scientists external to the community. In 1993, Tomas Lindahl, a specialist in DNA degradation and its implications for human health, published a paper in *Nature* about the chemical instability of DNA and its consequences for ancient DNA research, noting that processes of hydrolysis, oxidation, and nonenzymatic methylation posed serious problems (Lindahl 1993a). In another article, Lindahl questioned the reliability of results claiming to have recovered DNA tens to hundreds of millions of years old, results that he cynically called "antediluvian DNA" (Golenberg et al. 1990; Cano et al. 1992; DeSalle et al. 1992; Cano et al. 1993; Lindahl 1993b). He argued that biochemistry could not support such longevity. Even if it could, contamination was concerning. To control contamination, he suggested the sharing of both positive and negative data, reproducible results, negative controls, and appropriate chemical analyses. He explained, "Recent claims of recovery of 100-million-year-old DNA have overshadowed the valuable and important studies on moderately ancient DNA." For Lindahl, the next step must be a conservative step: "Rather than proceed spectacularly further and further back in time with anecdotal reports on single samples, using the notoriously contamination-sensitive PCR, I suggest that the next goal be a convincing report on the amplification of small DNA fragments, say, 100,000 years old" (Lindahl 1993b, 700). As Pääbo recalled, the label of "antediluvian DNA" was initially intended as a form of "ridicule" in the field. In reference to Lindahl's label, Pääbo said that he and his lab "loved it, applied it, and it stuck" (Pääbo 2014a, 58). Pääbo welcomed Lindahl into the conversation about contamination: "It was a great help to have a respected scientist from outside the field point this out – especially given my concern that the ancient DNA field tends to attract people without a firm background in molecular biology or biochemistry who, lured by the media attention that accompanies many ancient DNA results, simply apply the PCR to whatever old specimen they happen to be interested in." Pääbo referred to this as "'molecular biology without a license'" (Pääbo 2014a, 52). These scientists' concerns about contamination caused others to be circumspect, too.

In 1994, Scott Woodward, Brigham Young University in Utah, and colleagues claimed to have extracted and sequenced DNA from an 80-million-year-old bone (Woodward, Weyand, and Bunnell 1994). The media reported it as DNA from a dinosaur bone (Monastersky 1994). Interestingly, the publication did not confirm that the DNA was from a dinosaur. However, it did suggest it, and researchers did not deny it when reporters related it to the public. One interviewee said, "[…] We called it a Cretaceous period bone. We never called it a dinosaur bone. I mean, I didn't necessarily *stop* anybody – [laughs] – from saying that or anything like that, but we tried to be quite careful in the publication that we always called it this Cretaceous period bone […]" (50-01:20:00). But sensationalism turned to skepticism when several independent studies called the authenticity of dinosaur DNA into question (Woodward, Weyand, and Bunnell 1994; Hedges and Schweitzer 1995; Steven 1995; Allard, Young, and Huyen 1995; Zischler et al. 1995). Blair Hedges, Pennsylvania State University, and Schweitzer critiqued the study on grounds that it lacked appropriate phylogenetic analyses and additional attempts to replicate results prior to publication. Instead, phylogenetic analyses suggested that the DNA was not dinosaurian but mammalian, likely human and the product of contamination (Hedges and Schweitzer 1995, 1191). Others confirmed this conclusion. The issue was that Woodward and colleagues had extracted and sequenced an unusual and unidentified sequence which did not match any other known sequence. For this reason, among other reasons, they determined it to be of ancient, perhaps dinosaurian, origin. The news played out in the press, too (Connor 1995). However, it played out in the press for a different reason, as a big claim revealed to be an embarrassingly big mistake. One report – "Critics say Presumed Dinosaur DNA is Actually Human" – quoted DeSalle: "'The technical aspects of the work are excellent, but the inference of the DNA being dinosaur is flawed,' said Rob DeSalle […]. 'What they have is a very old piece of DNA and they don't know what it is'" (*Washington Dateline* 1995). Criticism stated extra efforts should have been conducted to determine the source of the sequence.

While contamination criticisms were leveled at Woodward from different directions, it was Pääbo's lab that established evidence for its source. In 1990, Pääbo was appointed a professorial position at University of Munich, and his lab was one of the chief challengers of dinosaur DNA. Hans Zischler, working with Pääbo and colleagues, strongly suspected that the DNA was actually a case of contamination, specifically human contamination. Woodward's team had targeted a mitochondrial gene, but sometimes segments of the mitochondria can be transferred, for various reasons, from the mitochondrion to the nucleus of a cell resulting in a special sequence referred to as nuclear mitochondrial DNA segment (NUMT). Zischler and colleagues hypothesized that this could be the case, thus explaining the extraction of an unusual and unidentifiable sequence, the supposed dinosaur sequence. They devised a clever but unconventional experiment to test this by asking male members of the lab to donate their sperm so they could search both the mitochondrial and nuclear genome for this specific NUMT sequence. When they compared the suspected dinosaur sequence to the sperm sequence, they found them to be a perfect match. Their reply in *Science* was sarcastic as they tried to rationalize the striking similarity between the sequences (Pääbo 2014a). First, they supposed that if Woodward's dinosaur DNA was in fact dinosaur DNA then that would mean their sequences were similar because their own lab in Munich was actually contaminated with dinosaur DNA. They found this scenario highly unlikely. Second, they hypothesized that dinosaurs and mammals might have hybridized at some point before their extinction, therefore exchanging their DNA, thus explaining why the supposed dinosaur sequence looked more mammalian than dinosaurian. This too they found highly unlikely. Finally, they suggested that the extracts or equipment used in Woodward's lab were not clean but contaminated by human DNA. Pääbo's lab found this conjecture most convincing: "In conclusion, these results strongly suggest that Woodward et al. accidentally amplified nuclear copies of human mitochondrial DNA" (Zischler et al. 1995, 1193; Jones 2001, 31–38). Dinosaur DNA, in this case, was debunked, leading to a dramatic drop in researchers' confidence in ancient DNA research's credibility.

Dinosaur DNA was debunked, but DNA from insects in ancient amber remained to be tested. Indeed, some scientists were critical of these claims. Some were even hostile. One researcher recalled a "pall of negativity" and "critical comments about the inability of

DNA to last that long" from the scientific community, "but the media ate it up – *big time* – because *Jurassic Park* was out and the movie was just coming out […]" (31-00:19:00). Pääbo's lab, not surprisingly, was one lab that contested the reproducibility of results. In 1994, Hendrik Poinar, who had worked on one of the studies suggesting the preservation and extraction of DNA from insects in ancient amber, went to work with Pääbo in Munich to try to reproduce results. Here, he worked on a method called amino acid racemization; a test that used amino acids as biomarkers to determine DNA decay and its potential preservation in fossils. Robert Service, in an article called "Just How Old Is That DNA, Anyway?" explained the experiment: "[A]n international team of researchers reports that a chemical change that converts amino acids in proteins from one mirror-image form to another – a process known as racemization – takes place at virtually the same rate as the degradation of DNA." Therefore, "If the amino acids show this conversion to even a modest degree, then the original DNA in the sample is likely long gone, suggesting that any remaining genetic material is a contaminant" (Bada et al. 1994; Service 1996, 810; Poinar et al. 1996). The team tested the racemization of amino acids against the degradation of DNA using twenty-six different specimens from 50 to 40,000 years of age. Interestingly, amber specimens did not exhibit evidence of racemization. Indeed, researchers recovered what appeared to be endogenous amino acids. In the paper, they hypothesized that "amber matrix may provide conditions conducive to the long-term preservation of nucleic acids" (Poinar et al. 1996, 866). Researchers, including Pääbo, reasoned that retention could be attributed to the preservative properties of the resin itself (Poinar et al. 1996; Service 1996). While amber appeared to be a potential preservative of DNA, Service said that DNA from dinosaurs was less than likely. Indeed, Service quoted Woodward admitting that the prospect "looks bleak." But Service quoted Woodward adding a caveat: "He adds, however, that the new test is not a direct measure of DNA integrity but a correlation, so there is still room for hope. And hope, if not DNA, springs eternal" (Service 1996, 810).

Concerns of contamination played out publicly across the traditional scientific avenues of publication as well as in more private venues like conferences. As excited scientists entered the field, Pääbo found his role changing from researcher to regulator. During this decade, Pääbo and his lab found themselves spending time reviewing, then responding to a number of questionable publications (Golenberg et al. 1990; DeSalle et al. 1992; Cano et al. 1993; Woodward, Weyand, and Bunnell 1994). One scientist said they found themselves playing the role of "the PCR police" (12-01:12:00). As the lab took on a conservative and critical cover, Pääbo's name took on an almost menacing meaning (36-00:15:40). As one scientist said, "He's called the 'Dark Lord of Ancient DNA'" (12-01:41:20). The problem of contamination came to the forefront in 1995 at the Third International Ancient DNA Conference at Oxford University in England ("Ancient DNA III" 1995). Together, Lindahl and Pääbo tried to enforce professional and philosophical values of protocols and precision. An interviewee presented this perspective of the conference: "Tom Lindahl gave his talk about it being impossible for DNA to survive for too long, and Svante [Pääbo] made a really eloquent talk about the need for rules, regulations […] and criteria and rigor within the field." This interviewee explained, "And it was really quite impassioned. And everybody took that on board. And that was I think the most largely attended ancient DNA conference – there must have been 200 people there. So, everybody went away really impressed with the fact that we had to sort of self-regulate ourselves. And I think the message that Svante was trying to get across was that if we don't self-regulate ourselves then we will lose credibility and the field will completely die" (4-01:33:35). However, the need to self-regulate was not just a private plea to the community. It was a public one, too. A *Science* report read, "But the hype – and the embarrassment when some claims did not hold up – is causing ancient DNA researchers to fear that their field won't be taken seriously" (Williams 1995, 923). Replication was principle part of this call for regulation. This demand for self-regulation was Pääbo's last message to this community at this conference. As a founding father of the field, Pääbo is rumored to have never attended these meetings again (6; 15; 36; 37; 42; 43). From then on he set his sights on his own work, leaving the rest of the community to police the technoscience for themselves.

In 1994, the ABI funded one particular project intended to test the preservation and extraction of DNA from insects in ancient amber (Eglinton 1996, 37). In 1996, a team of researchers at the Natural History Museum in London took *Jurassic Park* to task. One interviewee presented this perspective: "[M]y […] job that I got at the Natural History Museum was all down to *Jurassic Park*[.] […] [T]he museum probably would never have got the funding to try and do this DNA from amber if it hadn't been for *Jurassic Park* in the first place. *Part* of my kind of entry into the ancient DNA world was all due to a movie; a fanciful fictional movie" (25-01:05:00). To be clear, however, the objective was not to recover dinosaur DNA: [W]e weren't trying to get dinosaur DNA – we were trying to get insect DNA out of insects in amber" (25-00:45:45). Nonetheless, *Jurassic Park* was in part an inspiration. At the Natural History Museum, Jeremy Austin, Andrew Ross, Andrew

Smith, Richard Fortey, and Richard Thomas attempted to replicate previous results. To do this, they sampled fifteen insects in amber from different resins and ages. In the end, they announced that in every instance they "failed to recover any authentic ancient insect DNA" (Austin et al. 1997, 470). A researcher on the study said they "jumped through all kinds of hoops and did all the protocols" to "make sure" they had "believable results" and in the end, they "failed pretty comprehensively" to demonstrate that DNA preservation and extraction from insects in ancient amber was possible (24-01:36:45). A small section in *Science* read, "'No Go' for Jurassic Park-Style Dinos" (Holden 1997). An article in *Nature* read, "Lights turning red on amber" (Sykes 1997). Following years of investigations and publications, amber as a time capsule – as a dinosaur capsule and means of dinosaur resurrection – was debunked.

2.3.3 "The wild west"

The 1990s was the race for the most ancient DNA from the most iconic fossils, particularly as the practice coincided with and was catalyzed by *Jurassic Park*. One scientist called it "the *Jurassic Park* phase" (4-00:45:35). Another called it the hunt for DNA in "the red and the dead" or the near extinct and extinct (9-01:22:30). One interviewee called it "the wild west" (10-02:18:30). By 1992, researchers were conscious of their tendency to focus on specific species that would attract press and public attention. "Ancient DNA Newsletter" editors, Wayne and Cooper, called these species "disco species" (Wayne and Cooper 1992b, 3). They also noticed that 35% of the field's papers were published in high-profile and high-citation journals like *Nature*, *Science*, and *Proceedings of National Academy of Sciences* (Wayne and Cooper 1992b, 6). Although the technoscience had acquired considerable attention regarding studies on species that captured public curiosity, there was also important research in the world of plant domestication and its implications for studying human culture, evolution, and migration (Rollo 1985; Rollo, LaMarca, and Amici 1987; Goloubinoff, Pääbo, and Wilson 1991). For example, by tracing the genetic distribution of modern and ancient wheat, scientists suggested these findings could help towards understanding the origins of farming and its expansion around the world (Brown et al. 1993). These findings were scientifically significant, yet from a press perspective it attracted less attention than DNA from other enigmatic organisms like quaggas, thylacines, or dinosaurs. In the "Ancient DNA Newsletter," Robin Allaby and Terry Brown, Department of Biochemistry and Applied Molecular Biology at University of Manchester, highlighted the disproportionate interest: "The problem with plant remains is that they are just not sexy. Compared to a chunky, media-attractive dinosaur bone, or a cute 'n' cuddly furry marsupial, a bucket of charred wheat is just plain unattractive" (Wayne and Cooper 1992b, 18). Even within the publicity prone field of ancient DNA research, specific specimens enjoyed preferential press coverage as opposed to others.

In light of publicity around the practice, researchers realized that the integrity of their technoscience depended on a policing of it. Some researchers responded by trying to explain the science and science fiction aspects of ancient DNA research. For example, Adrian Lister, a paleontologist and collaborator with ancient DNA practitioners, wrote an article called "Ancient DNA: not quite *Jurassic Park*," while Schweitzer and Tracy Staedter wrote another article called "The Real Jurassic Park" (Lister 1994; Schweitzer and Staedter 1997). For example, DeSalle and David Lindley wrote *The Real Science of Jurassic Park and the Lost World: Or How to Build a Dinosaur* to explain to a public audience the science and science fiction of *Jurassic Park* and its 1997 sequel, *The Lost World*, when compared to the reality of the research itself (DeSalle and Lindley 1997). A review of the book read, "It debunks the whole scenario very effectively and is a perfect antidote to all the ridiculous hype surrounding these films. Steven Spielberg is a fantastically successful film-maker; he has created some of the best fantasy movies ever; and that is all that *Jurassic Park* and *The Lost World* are – pure fantasy, no more, no less" (Norman 1997, 22). As one scientist said, "[…] [T]hat book comes directly from the media interest in ancient DNA work" (18-00:23:10). They responded with criteria of authenticity, which was as much of a response to celebrity as it was to contamination. As an interviewee argued, "So, a lot of the kind of work which was done in the 1990s with Svante Pääbo coming out with criteria for authenticity […] I think was not just a defense against *Jurassic Park*. It was a defense against the rest of the scientific community who were starting to look at ancient DNA as, like I said, a sort of charlatan type of research" (4-01:19:50). Researchers wanted to establish criteria to establish credibility, and the mismatch between professional and popular expectations of the technoscience made it difficult to do so.

Following "the *Jurassic Park* phase" and the race for the oldest DNA came a creative but more conservative movement as researchers set their sights on less geologically ancient but scientifically significant, and arguably publicly appealing, specimens. In 1994, two papers were published back-to-back in *Nature* on DNA from the woolly mammoth, something that had been a professional and public interest since the late 1970s and early 1980s. One article was by Matthias Höss and Pääbo at the University of Munich, and Nikolai Vereshchagin at the Institute of Zoology in St. Petersburg, Russia (Pääbo, Höss, and Vereshchagin 1994). The other was by Hagelberg with Mark Thomas and Charles

Cook Jr. of University of Cambridge, and in collaboration with Andrel Sher, Gennady Baryshnikov, and Lister (Hagelberg et al. 1994). In light of extraordinary but erroneous reports of "antediluvian DNA," Höss and colleagues made their position on contamination clear. Citing Lindahl's criteria and caution, they extracted, sequenced, and confirmed DNA from five mammoths ranging from 9,700-50,000 years of age: "We suggest that the much older sequences reported in the past few years, for example in amber, be submitted to the same kind of tests as the faunal remains presented here" (Pääbo, Höss, and Vereshchagin 1994, 333). Hagelberg and colleagues cited Lindahl's criteria too as part of their process for recovering DNA from two mammoths, one of which was at least 47,000-years-old and presumed to be the oldest DNA from a vertebrate to date: "Lindahl has suggested that moderately ancient DNA (about 100,000 years old) should be targeted for analysis to bridge the temporal gap that exists between DNA sequences from relatively recent biological remains and those many millions of years old" (Hagelberg et al. 1994, 333). However, both studies did not recover enough DNA to definitively determine the evolutionary relationship of the extinct mammoth to the extant African and Indian elephant, and the cause of their extinction remained enigmatic too. Nonetheless, these studies suggested DNA from the Pleistocene could be recovered reliably.

Yet another paper made a special splash among professional and public audiences when researchers, for the first time, claimed to have extracted and sequenced DNA from a Neanderthal, another accomplishment that Allan Wilson in the 1980s had hoped would, one day, be a research reality (Krings et al. 1997). In the 1850s, an ancient, extinct, and unknown hominin was found in the Neander Valley in Germany (Fuhlrott 1859; King 1864; Schmitz et al. 2002; Madison 2016). This discovery in the late nineteenth century, among others throughout the twentieth century, garnered serious scholarship and incredible public interest (Gibbons 2007; Schmalzer 2008; Manias 2015; Rees 2016; Madison 2016). However, by the end of the twentieth century, the relationship of Neanderthals to ancient and modern humans was still unclear (Stringer 2012). In the 1990s, however, Ralf Schmitz at the Rheinisches Landesmuseum Bonn initiated a new study of the type specimen, and enlisted Pääbo and Matthias Krings as part of the project. Their job was to try to get DNA. They succeeded but replication of results was key for credibility. For this, they turned to Mark Stoneking, a geneticist specializing in human history and evolution, who had worked with Wilson and Pääbo at Berkeley. Stoneking, a professor at Pennsylvania State University, agreed to attempt to replicate results, and his doctoral student, Anne Stone, took on the task. Stone attempted to amplify the DNA using an alternative approach. The first round of results was contaminated, but the second proved more promising. However, she still did not know if the sequence from her lab matched the sequence from the Munich lab. The two teams would have to compare their sequences to confirm their authenticity. Over an anxious phone call, the two teams compared the differences in sequences one by one. As one researcher remembered, "[X] would say one and [Y] would say, 'Yay!'" The reading of sequences went on, one by one, and match after match they came to the conclusion that both labs, independently of one another, had recovered Neanderthal DNA (30-00:55:15). The Neanderthal mtDNA when compared to mtDNA of primates and modern humans from Africa, Europe, Asia, and across the world demonstrated that their differences (based on a single sequence) were distinct. They interpreted this as evidence that Neanderthals did not contribute their DNA to the modern human gene pool. These results also suggested that modern humans had their origin in Africa not Europe, and that Neanderthals had lived and died without contributing their DNA to modern humans. But researchers reasoned that this did not completely rule out the possibility of a genetic contribution from extinct Neanderthals to extant humans, and that further data would be necessary to resolve this question. Nonetheless, this study and its attempt to use molecular data to rewrite a history written with morphology added heat to an already heated debate in evolutionary anthropology.[31]

The Neanderthal DNA study was significant for a range of reasons. First, the results were incredibly important to human evolutionary history, but where those results were published was especially noteworthy. Pääbo recalled the reason for submitting this paper to *Cell* and not *Nature* or *Science*: "Publication there would send a signal to the community that the sequencing of ancient DNA was solid molecular biology and not just about the productions of sexy but questionable results […]" (Pääbo 2014a, 18). In the late 1990s, ancient DNA researchers wanted to show that their work was rigorous and relevant. They wanted to position themselves as rigorous researchers within molecular and evolutionary biology. For scientists, this paper demonstrated this. It was intensely technical and methodological in the authentication of research results. Second, despite Pääbo's decision to publish in *Cell* versus *Nature* or *Science*, the study did not escape the celebrity that accompanied the technoscience, the fossil, or the implications of its conclusions. *Science*

[31] The debate in evolutionary anthropology centered around the origins of human history with evolutionary anthropologists usually subscribing to one of two hypotheses; the Out Of Africa Model or the Multiregional Continuity Model. The former proposes that humans originated in Africa and then migrated to other parts of the world, while the second suggests that prehumans originated in Africa but then evolved into modern humans after they migrated out of the continent. See Stringer (2012).

called it “a technical tour de force” (Kahn and Gibbons 1997). *The Guardian* read “We’re African, no bones about it” while *Daily Mail* reported “DNA Bone Tests Show We Aren't Descended from Old Beetlebrows” (Mihill 1997; Derbyshire 1997). *The Times* read, “A breakthrough in genetic analysis has shown that modern human beings are not descended from Neanderthal man. It proves the contention that Neanderthal man was an evolutionary dead end” (Hawkes 1997). Roger Lewin published a piece in *New Scientist* called “Back from the Dead” where he explored the implications of this study for the future of the technoscience (Lewin 1997). Lewin quoted Lindahl saying the study was a “‘landmark discovery’” and “‘the greatest achievement so far in the field of ancient DNA research’” (Lewin 1997, 43). Ancient DNA always ran the risk of contamination, but for Lindahl this paper was “compelling and convincing” (Lindahl 1997, 2). In light of the evolving expectations, Lewin speculated, “We’ll never resurrect dinosaurs but what about Neanderthals?” (Lewin 1997, 42). In response to “the Jurassic Park phase,” ancient DNA researchers embraced a creative but conservative effort to try to tone down celebrity and establish the credibility of the technoscience. Yet both were difficult to do. Though researchers appeared to avoid contamination, they could not avoid celebrity or speculation of resurrection.

2.3.4 “The believers and non-believers”

Within several short but significant years, the pursuit of ancient DNA had developed into what looked like a discipline. Terry Brown, the “Unappreciated Archivist” for the “Ancient DNA Newsletter,” recorded approximately ninety articles from 1970 to 1992 of which over a quarter were showcased in journals like *Nature*, *Science*, and *Proceedings of National Academy of Sciences* (Wayne and Cooper 1992a, 36–38; Wayne and Cooper 1992b, 3 and 36–37). Although a relatively modest record, the rate in which papers had been published over a short time span was impressive. The newsletter also noted the first publications of textbooks on the topic, like Geoffrey Eglinton and Gordon Curry’s *Molecules through Time: Fossil Molecules and Biochemical Systematics* and Bernd Herrmann and Susanne Hummel’s *Ancient DNA: Recovery and Analysis of Genetic Material from Paleontological, Archaeological, Museum, Medical, and Forensic Specimens* (Eglinton and Curry 1992; Herrmann and Hummel 1994). Moreover, the technoscience had acquired an audience with a mailing list of over 700 scientists from archeology to molecular biology (Wayne and Cooper 1994, 2). The ANCIENT-DNA-L, a world-wide electronic forum to complement the world-wide-web, had subscriptions from the US, UK, Canada, and Australia to Denmark and Japan (Wayne and Cooper 1994, 36).

By 1994, the technoscience had acquired sufficient attention to consider converting the newsletter into a journal.

After the First International Ancient DNA Conference at University of Nottingham in 1991, "Ancient DNA Newsletter" editors, Wayne and Cooper, were approached by publishers about turning the newsletter into a formal publication forum (Wayne and Cooper 1994, 2). It was considered at the Second International Ancient DNA Conference at the Smithsonian Institution in 1993, but community consensus seemed to suggest there were more disadvantages than advantages in making this move. Concerns included a lack of articles to sufficiently support the journal as well as practitioners' preferences to publish within their own scientific specialties. Some argued that the newsletter focused on "techniques" rather than a "defined scientific discipline," while others worried that a journal would be a "self-serving vehicle" to promote the editors' own profession and publicity. Editors conceded these concerns but argued that a journal would result in better quality and quantity of research and would send a statement of technoscientific credibility to the rest of the scientific community (Wayne and Cooper 1994, 2). In the height of hype, researchers realized that the novelty of the practice and its time as a technoscience in the limelight might dissipate. In the second "Ancient DNA Newsletter," editors made this particular point noting that the technoscience enjoyed prolific publication in journals like *Nature* and *Science*: "However, before we bask in self-congratulatory splendour we should realize many of the papers concern just a few samples of disco species. The novelty of ancient DNA will soon disappear, requiring that we address more fundamental evolutionary questions" (Wayne and Cooper 1992b, 3). In the third "Ancient DNA Newsletter," editors also argued that a journal could define ancient DNA research as a discipline, citing the recent "paleobiology" revolution and the success of its journal *Paleobiology* (Wayne and Cooper 1994, 2). In an attempt at specialization, researchers tried to shift from spectacle to a more technological, methodological, and theoretical focus.

Yet it was this combination of science and spectacle that attracted new and young researchers to the practice. The idea of recovering DNA from fossils especially encouraged researchers to investigate its potential as applied to archeology, anthropology, and epidemiology. One scientist said, "[W]hen I started working on ancient DNA – so this may sound facetious, but I don't think it is particularly – *Jurassic Park* had just come out. I read the book when I was at the [X] in '93 and the movie came out about then. It would be easy to underestimate the impact […]" (2-00:31:20). But it was more than hype that encouraged entry into the field: "There was also a paper by Terry and Keri Brown called

‘Ancient DNA and the Archaeologist’ […] [a]nd that also hooked my interest […].” (2-00:31:20). In the UK, Terry Brown and Keri Brown sought to build bridges between molecular biologists and archeologists through the search for ancient DNA (Brown and Brown 1992). From this nexus, ancient DNA activity flourished as practitioners tried to learn about human evolution, populations, migrations, diet, and disease as well as determine the sex, age, and kinship of past people (Eglinton 1995; Eglinton 1996). For example, researchers across the UK to France, Germany, Israel, and the US investigated the preservation and extraction of DNA in ancient humans (Vigilant et al. 1989; Hänni et al. 1990; Hummel and Herrmann 1991; Hagelberg and Clegg 1993; Stone and Stoneking 1993; Hagelberg et al. 1994; Gill et al. 1994; Handt et al. 1994; Faerman et al. 1995; Faerman et al. 1998). Practitioners also published on evidence of *Mycobacterium leprae* and *Mycobacterium tuberculosis* in ancient humans (Spigelman and Lemma 1993; Rafi et al. 1994; Salo et al. 1994; Baron, Hummel, and Herrmann 1996). Together, these works seemed to suggest the success of the technoscience in archeological, anthropological, and epidemiological contexts, but contamination, again, was a concern (Wayne, Leonard, and Cooper 1999). In these contexts, it was especially difficult to determine whether modern DNA was contaminating ancient DNA.

The controversy over contamination, and budding celebrity of the technoscience, played out publicly. In 1995, Stoneking published “Ancient DNA: How Do You Know When You Have It and What Can You Do With It?” (Stoneking 1995). To answer these questions, Stoneking considered a case by Elaine Béraud-Colomb, Institute of Developmental Biology in Marseille, and colleagues who reportedly recovered DNA from several human specimens up to 12,000 years of age (Béraud-Colomb et al. 1995). He praised their procedures taken to control contamination, yet the only step the lab did not take was to reproduce their results in a separate lab. Stoneking suggested that although this step is preferred, to make it a requirement for every study in every lab was far from practical. He said it would “cause more problems than it would solve” because multiple independent replications would be too expensive, destructive, and restrictive. Stoneking said that “attention” to “precautions” and “multiple independent extractions from each sample” should “suffice” (Stoneking 1995, 1260). In addition to issues of authenticity, Stoneking also spoke to the utility of ancient DNA and specifically the novelty and celebrity of the field: “After all, isn’t it a neat enough trick to show that DNA can indeed be obtained from ancient specimens?” In answer to his question, Stoneking said, “Alas, if ancient DNA is to become a legitimate field of scientific inquiry, then the answer must be no" (Stoneking 1995, 1260). He pointed out that recent research in the field, including Béraud-Colomb and colleagues' work, simply showcased the anomaly of ancient DNA from one or several samples with little insight or impact into the larger looming questions in evolutionary biology: "Even the main contribution of the recent analysis of mtDNA from Ötzi the iceman (Handt et al. 1994) was essentially that Ötzi was indeed of European origin and not an Egyptian mummy that had been fraudulently placed in the Tyrolean Alps; while this study attracted wide-spread interest and attention, this finding is not exactly a great leap forward for ancient DNA!" (Stoneking 1995, 1261). Stoneking proposed that the authenticity and utility of the technoscience must extend beyond its novelty: "[I]f ancient DNA is to be more than a technological curiosity, then we don't need any more such papers" (Stoneking 1995, 1261). With rising interest in ancient human DNA came rising issues regarding authenticity and utility. Instead, he recommended producing more sequences from many samples to address anthropological questions on a population rather than individual level.

During this decade, researchers were responding to "the wild west" by constructing criteria and expectations that would transform the technoscience from an emergent into an established practice with standards to ensure rigorous research. Cooper, now a postdoctoral researcher in the Department of Biological Anthropology at Oxford University, was becoming an influential individual. In 1997, Cooper replied to Stoneking in *The American Journal of Human Genetics*, reinforcing independent replication in light of ancient DNA's short but sensational history: "Several ancient DNA 'triumphs' (Golenberg et al. 1990; Cano et al. 1993; Woodward et al. 1994) that have turned out to be embarrassingly unrepeatable, or contaminated, might have been prevented if independent verification had been sought prior to publication" (Cooper 1997, 1002). Cooper argued that adherence to hard-and-fast criteria ensured credibility: "In summary, there are currently several methods available to test the authenticity of ancient human DNA sequences. I suggest it is the responsibility of the ancient-DNA community, and archaeologists working with them, to insist that they are fully utilized. Failure to do so threatens the credibility of ancient-DNA research" (Cooper 1997, 1002). Cooper and Stoneking differed in the degree to which criteria should be required for research, but they both agreed on the celebrity of the technoscience. Cooper, in another article with Wayne and Jennifer Leonard at University of California, Berkeley, situated the technoscience within a context of contamination and celebrity. In this research review, they noted the technoscience's professional and popular appeal: "From the beginning, ancient DNA research was a populist science. Reports of DNA from ancient remains led to wild speculation in the press and film that life could be restored to ancient creatures. Each new discovery served to reconfirm the public impression that scientists were moving quickly toward this goal. New reports of ancient DNA, although often of limited evolutionary significance, were published in the most prestigious journals" (Wayne, Leonard, and Cooper 1999, 458–459). For the authors, the issue was contamination: "The power of PCR was and is the problem: A single intact contaminating sequence from a recent source can potentially outcompete ancient degraded and damaged DNA in the process of amplification" (Wayne, Leonard, and Cooper 1999, 460). However, celebrity was the issue, too: "The honeymoon period has passed for ancient DNA research, and the difficulties associated with a maturing field need confronting" (Wayne, Leonard, and Cooper 1999, 464).

The problem of contamination slowly started to divide the community. In the mid-to-late 1990s, credibility meant controlling contamination, and some took the task of self-regulation into their own hands. For example, one interviewee shared this story: "I remember there was a […] conference […] where [X] was […] fourth or fifth speaker in a session and [X] was going to present some of his work. And he changed his talk. I saw him redoing his slides just before his talk. And he stood up and instead of talking about the work he was doing, he talked about how *rubbish* the field was and how human ancient DNA was becoming completely discredited" (4-01:36:00). This interviewee explained, "Everybody hated him for it because he was just so rude. But it needed to be done […]. We've just listened to four talks by people who […] said, 'We've done this' and 'We've done that.' Is it actually genuinely true? […]" (4-01:36:00). At the Fourth International Ancient DNA Conference at University of Göttingen in Germany in 1997 the differences in scientific and epistemic values were becoming more apparent ("Ancient DNA IV" 1997). As one researcher recounted, "By the time we went to Göttingen […] it was this bigger field and the cracks were starting to show […]. You started to have the feeling that if nobody was going to talk about the interesting technical stuff, in a field based essentially on technical stuff, it wasn't really much point in going" (2-49:00:00). The community felt their transition from an evolving to an established technoscience required a turn from spectacle and a turn towards methodology that took into account theoretical and technical challenges.

However, the celebrity of the technoscience posed a problem. According to one scientist, ancient DNA appealed to practitioners across disparate disciplines, but as it did it drew an audience of "amateurs" (5-01:00:00). This scientist said, "The trouble with ancient DNA is that you get people thinking they can do it; people who were forensic scientists, […] people who were doctors […], the sort of doctors who like to retro-diagnose what Mozart died from. [Laughs]. […]." For the community, amateur activity was a challenge to their credibility and the boundaries they tried to build around the technoscience: "So you get these people who think they can do DNA, and they don't have the right facilities or the right knowledge or the right understanding of ancient DNA […]" (5-00:58:30). The problem of contamination, exacerbated by celebrity, was a point of contention that divided the discipline: "You have this divide which is sort of crystallized by having these two different conferences and two different types of scientists […]; the ones who do proper work in laboratories in clean rooms and the other ones who […] work in forensic labs or even medical labs where there is no proper […] thinking about controls and contamination [...]. So, anyway, that's […] this division – the believers and the non-believers […]" (5-01:01:00). From enthusiasm to skepticism, ancient DNA research had emerged, evolved, and now struggled to become an established technoscience. After two decades, three newsletters, and four conferences, the ancient DNA community was growing, but it was growing in different directions.

2.3.5 Conclusion

This section focused on researchers' reactions to the pursuit for the oldest DNA. Interviewees described this decade as "the *Jurassic Park* phase," the hunt for "the red and the dead," "the wild west," and the "honeymoon" phase. First, I argued that claims of multi-million-year-old DNA generated intense public interest. However, these claims also attracted attention from scientists who were skeptical of these claims. Notably, "the PCR police" challenged the authenticity of those claims, labeling these sorts of studies as "antediluvian DNA." This was a rhetoric of ridicule. Second, I argued that while *Jurassic Park* hype mobilized the technoscience, it destabilized it, too. *Jurassic Park* infiltrated the professional and popular consciousness, and the search for the oldest DNA raised expectations of the technoscience that were ultimately mismatched with reality. In "The Ancient DNA Newsletter," Higuchi privately cautioned colleagues against encouraging wild speculations among the press and public about bringing dinosaurs back to life, while DeSalle and Lindley responded publicly with *The Science of Jurassic Park*. Others confronted contamination concerns and suggested searching for DNA in specimens hundreds to thousands, not millions, of years old. But the celebrity of the technoscience was not lost, particularly as the recovery of Neanderthal DNA garnered worldwide attention. Third, I demonstrated that during this decade, the technoscience started to professionalize via funding, conferences, collaborations, newsletters, discussions of a journal, and debates over criteria for policing the practice. Here, science, spectacle, and speculation came into tension. Stoneking tackled questions about the authenticity and utility of ancient DNA, highlighting the need to focus more on methods, applications, and implications of the technoscience. Cooper too argued that the "honeymoon" phase had come and gone, and pressed for more rigor. This was in part a response to the real theoretical and technical concerns about contamination, as well as a response to concerns about celebrity. However, for some scientists in the community it was becoming clear that standardization was far from straightforward. Researchers were starting to disagree, privately and publicity, about the criteria of authentication and ultimately their colleagues' competence in the search for ancient DNA.

2.4 ANCIENT DNA AS A SHARED CONCEPTUAL SPACE

2.4.1 Introduction

In the 1990s, the search for ancient DNA developed under the influence of intense public interest and extreme media exposure. The interplay between science and media contributed to the co-construction of a technoscience and a celebrity science. In this interplay, both researchers and reporters entertained the spectacle around DNA from fossils and encouraged speculation about bringing back extinct organisms. This vision helped shape the scientific and sociological development of the discipline. The discipline evolved within a shared conceptual space of professional, press, and public interests and incentives. Also in the 1990s, hype for ancient DNA research hit its high, and came to a crash with increasing concerns over contamination, celebrity, and competition. As expectations conflicted, researchers responded by building boundaries between what they saw as sound and unsound research, focusing on concerns for contamination, and at times trying to distance themselves from the media spotlight.

2.4.2 "Ancient DNA research's need for the press"

From a press perspective, the spectacle of DNA from fossils, and speculation about bringing back the past, held a high news value. One interviewee said, "[I]t just gives you access to what you might intuitively think is unreachable, unknown, and mysterious. I hate to use the word mysterious, but I'm quite sure that one – that word – is running around in the heads of a lot of people. It is like a key to a mysterious room or a looking glass or something like that" (1-00:52:25). Ancient DNA could reveal evolution in action: "The power […] of ancient DNA is, of course, that you can go back in time and by going back in time […] you can directly test hypotheses […]" (7-00:07:30). Ancient DNA instigated media imagination: "The media love ancient DNA. *They love it.* They *absolutely* love it. Usually, the key question is about cloning. They just can't get enough of it" (3-01:13:00). The media repeatedly returned to and reported on the topic: "[…] [I]t probably goes *right* back to the earlier days when we first got mammoth DNA […]. I've been amused by the fact that the press seem to have such a short memory. It's like, 'We went through all this nine months ago. You rang me up with the same question.' And they'll print it again. *They'll print it again*" (3-01:14:45). Ancient DNA's news value resulted in consistently high coverage, and researchers recognized this.

Researchers were aware of, even accustomed to, the news value of their technoscience. One scientist, who moves in and out of ancient DNA research, noted the disproportionate amount of attention his own publications on ancient DNA received from reporters when compared to his hundreds of others on different subjects. In reference to the hunt for DNA from fossils, this scientist said, "[…] People have tastes and it's a flavor they love" (44-00:26:20). Another interviewee presented this perspective: "No one really wants to read about the peptidoglycan in bacteria cell walls. It might be very important – probably much more important […] – but […] your average person is not going to read that. But you can always write a good story about a king or a mammoth or whatever" (6-01:10:25). Consequently, "We always have journalists ringing us and saying […], 'I need a story for something. What have you got?' […]" (6-01:10:15). Another added, "[I]f you're working on particle physics […] and you […] try to […] explain it to the general public a lot of them might just fall asleep or say, 'Why the hell are we funding this?'" (25-01:02:15). Conversely, "[…] [A]ncient DNA is a very easy thing to talk to both the media and the general public about. […]. It's an easy sell for the media to talk about. It's an easy sell for scientists who are in that area to talk to the media about" (25-01:02:15). In this technoscience, attention was a given, but its association with a blockbuster movie raised that attention to a new level.

Jurassic Park coincided with, then catalyzed the search for ancient DNA into the media spotlight. It became a textual and visual illustration of the potential power of ancient DNA and generated interest in the novel and controversial practice. One scientist said "*Jurassic Park*" became "a symbol" or "an analogy" to "explain" the technoscience or "inspire" others to become interested in it (12-02:03:15). A second said that "*Jurassic Park*" created "good press" and a generation of "geeky" but "glamorous" scientists (4-33:45:00). Another researcher recalled becoming interested and then involved in the technoscience because of the book and movie: "I think it drove public interest […]. I didn't want to make a dinosaur but labs looked cool. […]. Ancient DNA sounds cool; sounds like it should be cool. Part of that really does stem back to *Jurassic Park*. It is still the legacy of that. That's when it entered the popular consciousness" (2-01:31:30). In the UK, the ABI funded by NERC, one of the first financiers of the technoscience, drew on the blockbuster to bolster its professional and public status: "Where do dinosaur bones, insects in amber and molecular biologists co-exist? In *Jurassic Park* of course – but also in the Ancient Biomolecules Initiative (ABI), a £1.9 million research programme funded by the Natural Environment Research Council" (ABI Report 1998). In the 1990s, *Jurassic Park* was synonymous with the search for ancient DNA. One interviewee put it this way: "The media think about *Jurassic Park* when they think about ancient DNA. […]" (23-02:11:00). *Jurassic Park* and its acute association with the technoscience cast ancient DNA activity in the celebrity spotlight. As a result, scientists working in and around the practice were often given opportunities to publicly promote their research.

Ancient DNA's close connection with *Jurassic Park* elicited press and public enthusiasm, but what was important was how researchers reacted to the celebrity of their technoscience. It was more than the media that cultivated this connection. Indeed, researchers and research institutions were an active part of the process. In fact, some cultivated celebrity because it translated into publicity and publications in high-ranking journals, or in some instances, funding. When offered opportunities, some scientists were active in the media spotlight, adopting the news value of ancient DNA to fit press and public expectations. Writing for *The New York Times*, Browne closely followed the technoscience with repeated reports of ancient DNA activity and its potential to bring back the extinct. In *The New York Times*, among other articles, George Poinar entertained this speculation about resurrection. In a *Science* research news report, Grimaldi and DeSalle were seriously skeptical about resurrection, but George Poinar engaged the idea: "But Poinar, who set Crichton off on his fictional wild-dinosaur chase, is more inclined to let his imagination run wild" (Morell 1992, 1861). Reporter Virginia Morell highlighted George Poinar's new book, *Life in Amber*, where he detailed the idea of bringing dinosaurs back: "So if a big green flesh-eater goes cruising past your bedroom window one of these dark nights, you'll know just who to blame: Michael Crichton and George Poinar" (Morell 1992, 1861). From a press perspective, George Poinar, as a researcher, lent a line of credibility to the idea. Even if he conceded caution, the act of entertaining the idea was a way of encouraging it.

However, some scientists and scientific institutions also actively crafted their own potential for publicity which resulted in visibility or further funding. In New York, Grimaldi faced intense media interest, writing a book on amber in response, while the AMNH enjoyed positive publicity around recent research which could be credited to their lab. AMNH also produced a world-wide exhibition on amber in the wake of *Jurassic Park*. In California, Cano, George Poinar, and Hendrik Poinar also felt fame when their research reporting the extraction and sequencing of DNA from an insect in amber, the oldest DNA to date, was published in *Nature* at the time *Jurassic Park* was released in theaters. They also set up shop by the theater for the movie release. Further, Schweitzer and Horner found funding through NSF to search for dinosaur DNA. NSF deliberately designed its award announcement to coincide with the movie release. This interplay between science and media, while unique in that it contributed to the co-construction of a technoscience and celebrity science, was not new. Indeed, science communication scholar David Kirby tracked this interplay between science and major blockbuster movies in general, and analyzed the activity around *Jurassic Park* in particular (Kirby 2003a; Kirby 2003b; Kirby 2013). Kirby, for example, argued that "'coincidental'" professional publications with major movies was "common" (Kirby 2013, 139). He also argued that Horner's role as scientific consultant to Spielberg's *Jurassic Park* and its sequels, provided a source of "generous research grants" to finance his paleontology (Kirby 2013, 58). Ancient DNA's news value, heightened by hype around *Jurassic Park*, offered academics opportunities to step into the media spotlight.

Yet some scientists, in light of *Jurassic Park*'s popularity, tried to work against the narrative by making a clear distinction between science and fiction. Kirby argued that while cinema may be an "alternative" and "informal" form of "science communication" that it should not be considered "insignificant" (Kirby 2013, 227). Far from it, the movie had a profound impact on popular and professional perceptions of the search for DNA from fossils and what was real or potentially possible for this line of research. As Kirby argued, "Film's reality effect renders scientific representations plausible because it naturalizes images and events within the fictionalized world." Further, "Cinema is a powerful medium of communication because its reality effect provides it with a capacity to serve as a virtual witnessing technology. The more cinematic technologies advance, the better cinema becomes in serving as a virtual witnessing technology" (Kirby 2013, 227).

CGI in *Jurassic Park* played a part in this realism, influencing public ideas of ancient DNA activity that some scientists found particularly problematic because some of the depictions were deemed to be inaccurate. Kirby explained, "This cinematic naturalization can have an important influence over audiences' perceptions of science and the natural world by legitimizing scientific depictions. The naturalization of scientific images and scenarios in cinema poses a problem because cinema's reality effect naturalist both accurate and inaccurate science" (Kirby 2013, 228). In other words, the science we see in movies can take on "a life of its own outside the confines of the screen" (Kirby 2013, 228). *Jurassic Park*, as Kirby argues, is an excellent example of "in its incarnations in novels, films, comic books, and computer games as well as its incorporation into television documentaries and news articles" and the "high degree of intertextuality in science-based media" (Kirby 2013, 228). This impact, and the degree to which scientists found it a positive or negative impact, caused researchers to respond in a variety of ways.

At the time, researchers recognized *Jurassic Park*'s powerful potential to influence the public perception of ancient DNA research. While some responded by working with the narrative, others tried to work against it to present an alternative, and what they saw as a more accurate depiction, of their research and its applications. Some spoke privately and others publicly. Higuchi, for example, wrote privately to peers in the "Ancient DNA Newsletter," advising against playing into the rhetoric of resurrection post *Jurassic Park*'s publication as a book but before its release as a movie: "When you get asked (and in the wake of Jurassic Park, the movie, it seems inevitable that some of you will) whether the resurrection of dinosaurs from ancient DNA is possible, I hope you will say it is not." He admitted, "Although it is fun to say, 'in theory, it may be possible (nudge, nudge – wink, wink)', let's get real" (Wayne and Cooper 1992b, 6). The movie presented a very distinct vision of how researchers could use DNA from fossils. But some scientists disagreed with this vision, even if intended as fantasy, and tried to present a different depiction of the technoscience. For example, one scientist wrote an article called "Ancient DNA: not quite *Jurassic Park*," while others published a piece titled "The Real Jurassic Park" (Lister 1994; Schweitzer and Staedter 1997). Additionally, DeSalle and Lindley wrote *The Real Science of Jurassic Park and the Lost World: Or How to Build a Dinosaur* to explain to a public audience the science and science fiction of *Jurassic Park* and its 1997 sequel, *The Lost World*, when compared to the reality of the research itself (DeSalle and Lindley 1997). Kirby demonstrated that researchers often respond to "[c]inema's forced consensus" with their own "conceptions" of "true scientific representation" through popular rather than professional works like *The Science of Jurassic Park* (Kirby 2013, 167). Researchers wanted to establish criteria to establish credibility, and the mismatch between professional and popular expectations of the technoscience made it difficult to do so.

In light of *Jurassic Park*'s and ancient DNA activity's popularity, and despite this resistance from some scientists, professional and prestigious scientific journals were interested in capitalizing on the hype. During the 1990s, the community acknowledged the high percentage of ancient DNA research showcased in high-profile journals like *Nature* and *Science*. Consequently, some scientists capitalized on the celebrity of the technoscience by setting their sights on the most celebrated but competitive journals. One researcher remarked, "With ancient DNA […] you just have to say 'mammoth,' 'Neanderthal,' and people […] want to hear more […]. [B]ecause of that any story you have to tell could potentially be a cover picture of a big journal. So *that*, plus ambition, was for many […] [a] booster for their career in terms of getting full professorship really fast and in terms of […] getting access to really amazing samples" (8-00:25:00). The idea is that top journals were quick to capitalize on the celebrity associated with the topic. For journals, it attracted attention to the publications, and it brought commercial, as well as reputational, advantages. This was a convergence of interests. One interviewee put it this way: "[W]hatever you do with the same cleverness, the same money, the same techniques, whatever, you can't help the fact that if you have an archaic hominin it will go in *Nature* or if you have a sort of beetle of some sort it wouldn't go into *Nature* necessarily, right? [Laughs]. It's a sort of imbalance" (8-01:01:00). Overall, making it in *Nature* and *Science* meant professional prestige, but it also meant making it big in the popular press, which could result in future funding. One interviewee shared this story: "I was collaborating on a *really* nice ancient DNA project – good project, good results. They said, 'We're sending this to *Nature*.' I said, 'It's not a *Nature* paper. You're wasting your time.' I said, 'Send it to such-and-such journal.' 'Nope! No!' It went to *Nature*: rejected. Then they tried, I think, PNAS ('previously submitted to *Nature* and *Science*'): rejected. [Smiles]. And eventually it ended up in the journal I'd first recommended. [Laughs]." This scientist said: "[…] [T]he top journals […] are *almost* the link to the popular media. If you look at *Nature*, it is more than a science journal. […] Although because they do publish high-level science, they also like a damn good story. *They do*. You know, *short* papers with a *punchy* headline." This interviewee summarized the situation with this explanation: "So, I think the attempt to get their work into these top journals, repeatedly (with a lot of success I might add in some cases), to some extent colors people looking for what you might call 'sound-bite-research.'

‘Let’s sequence that hominid.’ You know that’s going to be a *Nature* paper if that’s got DNA in it” (3-01:34:10). Here, technical literature also had a commercial component, and researchers tried to play to both expectations of scientific significance and press and public interest accordingly.

This interplay was influential in the co-construction of ancient DNA research into a technoscience and a celebrity science. One researcher rationalized the relationship this way: “You’ve got to separate the ancient DNA research’s need for the press, and the press’s need for ancient DNA. So, the press *loves* ancient DNA because it’s often on stories that are very attractive to the general public. […] Whenever they’ve got nothing, they come to us because they know it’ll be something interesting, right? Because we work on history. We work on anthropology, archeology. We work on weird shit, dinosaurs, whatever” (6-01:10:00). Dorothy Nelkin, science communication scholar, explained that while media seeks to educate, they also strive to entertain, so for the media, science and technology is “more a source of entertainment than of information” (Nelkin 1995, 162). Therefore, the press spotlights science that is timely, novel, or controversial. Sharon Dunwoody, science communication scholar, explained, “Science journalism, again in ways typical of other types of journalism, seeks to hang stories on *traditional news pegs*, characteristics of real-world processes that are proven audience attention-getters” (Dunwoody 2014, 32). In 1921, Edwin Scripps founded the Science Service, the first official forum for science writing in the US, and in 1924, Edwin Slosson, first editor, summarized science in the media as “‘[t]he fastest or the slowest, the hottest or the coldest, the biggest or the smallest, and in any case, the newest thing in the world’” (quoted from Nelkin 1995, 82). According to Nelkin, much is the same today. The search for ancient DNA, especially the first or the oldest DNA, hit headlines for this reason. Ancient DNA was, and is, science that sells.

The relationship between science and media is reciprocal. One researcher remarked, “But the ancient DNA researchers, I mean, that’s how they justify getting their money, right? That’s how [X] gets his money because the [Y] government wants to show that [Y] science is world class. How better to do that [then] to have *Science* report it, or *National Geographic* or *Discovery Channel* or *Scientific American* […]. So, [X] gets that press, [Y] government is happy, give [X] more money” (6-01:11:00). Again, this is not new. According to Nelkin, researchers and research institutions are well aware of and accustomed to this reality and exploit it for a range of reasons: “Individuals try to attract press attention for a variety of reasons – to influence public views, to attract funds, or to establish their competitive position in 'hot' fields of research" (Nelkin 1995, 129). Nelkin argued that in the 1970s, scientists in the new field of recombinant DNA research launched a serious media show to hype their research as well as combat criticisms, even fears, about the promise or perils of genetic engineering. Nelkin also argued that scientists have learned how to position their science, through the press and for the public, in a favorable framework for funding: "Geneticists today, seeking to maintain support for costly research, have become skilled in rhetorical strategies designed to attract the media. They describe the genome as a 'bible,' a 'medical crystal ball,' a 'blueprint of life.' They promise that the Human Genome Project will 'unlock the secrets of life,' allowing the prediction and control of disease" (Nelkin 1995, 130). Ancient DNA faced something similar. Scientists working in and around the search for DNA from fossils recognized this relationship and played to it for publicity, funding, and prestige.

I argue that the hunt for DNA from fossils developed into a discipline within a shared conceptual space between scientific, press, and public interests. One interviewee presented this perspective: "[…] I think media's played a *huge* role in ancient DNA. I think that it was intentionally used to play a big role in ancient DNA because, if you think about it, ancient DNA started as this field that was crazy! […]." Further, "[A]t the time, we didn't have the methods. We didn't have the know-how. […] [W]e needed that tie to build up to it and I think media was used to help generate interest, to maintain funding, until we got to that point. […]" (27-02:14:45). Scholars in science communication studies have referred to the relationship between scientists and journalists as a symbiosis: "Perhaps the most accurate term for scientist-journalist interactions is 'symbiosis,' that condition in which diverse entities coexist for mutual benefit" (Friedman, Dunwoody, and Rogers 1986, xiii). Dunwoody specifically suggested the idea that journalists and their sources, including scientists, intersect and interact within a "shared culture" (Dunwoody 1986, 13).[32] Peter Broks, cultural studies scholar, called this a shared "conceptual space" and offered this space as a "new model for understanding how the meanings of scientific knowledge are challenged and negotiated" (Broks 2006, 144).[33] While some might see the relationship as a dichotomy of "us" and "them," this history has highlighted that scientists and the media were intimately involved in a dialogue for their mutual benefit. Within this shared

[32] Sharon Dunwoody said this idea was specifically suggested by sociologists of science. See Blumler and Gurevitch (1981).
[33] The use of the term "popular science" has a contentious history. Recently, James Secord has argued for abandoning the term "popular science." See Secord (2004). However, Peter Broks has reconsidered the term by redefining how we think and talk about popular science. See Broks (2006).

conceptual space, researchers were active agents in using the media to further their field. But this convergence of professional and popular interests quickly became strained. As hype turned to disillusionment, some scientists started to build boundaries around the practice to establish, then enforce, standards of credibility.

Jurassic Park posed problems for researchers because it raised expectations among the press and public that were ultimately mismatched with research reality. One interviewee shared this story: "[…] The problem that *Jurassic Park* produced was […] that other scientists started to lose faith in the credibility of ancient DNA. […] I remember one meeting […] just before Tomas Lindahl's paper came out on the degradation of ancient DNA that showed that million-year-old DNA wasn't possible, and these two guys were really ecstatic that Tom Lindahl had debunked the whole ancient DNA thing." Moreover, "[…] [T]hey were saying, 'You know, this shows all the ancient DNA work is rubbish and it always has been rubbish.' Their interpretation was that ancient DNA was sort of a flakey area of research. It wasn't really serious because it was so tied up in the media, so tied up with *Jurassic Park*" (4-01:17:20). For some, the close connection between the technoscience and the media compromised the research and its reputation in light of contaminated or irreproducible results. Another research remarked, "Yes, I think it's been a bad influence because it raised those levels of expectations about DNA and what DNA – ancient DNA – could do. […]. And of course because of *Jurassic Park* you had this guy, [X], trying to get dinosaur DNA out and it being shown to be a human contamination. So yeah, it's had a bad influence, I think. We're still living it down. […]" (5-01:20:00). For this researcher, this has had a profound effect on the field and the public understanding of it, too: "[…] [W]hen I give a talk about ancient DNA, they put up a poster and it has a *dinosaur* on it. I've actually objected a couple of times. I've said […], 'There's no dinosaur DNA. You should *not* show the dinosaur.'" (5-01:20:00). Another interviewee, however, offered a more nuanced opinion: "[…] [W]here *Jurassic Park* has been a difficulty has not been *Jurassic Park's* fault, but the notion 'that by doing my research I can become a big media star' is quite seductive to some scientists. […] The papers which came out in the '90s on amber DNA and on dinosaur DNA and on 20-million-year-old *Magnolia* leaf DNA, the people who did that work believed that the results were correct" (4-01:14:40). For this scientist, the celebrity of the technoscience and appeal of fame or fortune played a part: "[X] who did the Magnolia leaf is […] one of the top plant evolutionary biologists […] – a highly rigorous scientist – but [X] just suspended his critical judgment to a certain extent and didn't ask, 'Is this likely to be a contaminant or a mistake?' And I think part of that was because [X] thought, ‘I can get this in *Nature* and a lot of people are going to interview me.’ And I think the same is with the amber results” (4-01:14:40). In other words, the celebrity spotlight that helped build the field also challenged its credibility.

Context is critical when examining failed expectations. Ancient DNA during this decade was an exploratory and evolving practice. However, some scientists did not seem to take this into account when discussing its successes or failures. For Lindahl, “The Fiasco of DNA from Insects in Amber” was a result of media compromising science: “In early attempts at retrieval of ancient DNA, several excited reports appeared in leading scientific journals on the apparent recovery of DNA from 100-million-year-old insects in amber, as well as from dinosaur bones and very ancient leaves. The popular ‘Jurassic Park’ book and movie gave an impetus to those studies that was hardly scientifically motivated. In retrospect, the work appears somewhat naive and lacking in the rigorous controls to exclude contamination with modern DNA that have become routine in more recent investigations of ancient DNA” (Lindahl 1997, 2). Here, it is important not to impose the present on the past; what is known now on what was not known then. In the 1990s, ancient DNA research was an exploratory practice. Its limits had not been tested or defined. One interviewee illustrated this point about the 20-million-year-old *Magnolia* leaf DNA: “It’s a *smart* paper. It’s not a stupid paper. […] They’re not just saying, ‘Does DNA survive?’ But they actually tried to quantify the *damage* of the DNA and they’re trying to understand the whole system, and that’s what I admire about that work. […] And maybe it is rubbish that it doesn’t survive, but we didn’t know at the time it didn’t survive. Who knew?” (9-01:28:25). As another interviewee explained, “I don’t agree if people make it sound like it was crappy science in the early ‘90s. It was just not realizing what could go wrong, which is very different to going out and doing crappy science” (6-00:57:20). For this interviewee, success and failure was a part of the process. However, the high-profile hype around successes that turned out to be failures had consequences for the credibility of the field. As hype turned to disappointment, it caused a dramatic drop in confidence.

Nik Brown, sociologist of science, defined this as a dilemma between hope and hype. Brown put the predicament this way: “On the one hand, we accept that expectations are constitutive or performative and that hype plays a fundamentally important role in organising our future presents. On the other hand, hype is a source of ‘overshoot’, ultimately damaging credibilities and reputations” (Brown 2003, 17). It is not just scientists’ reputations at stake, but the reputation of the research as a whole that can suffer from failed expectations: “In so many cases, the present fails to measure up to the expectations once held of it. This can have disastrous consequences for the reputation not only of individuals but entire innovation fields" (Brown 2003, 9). Brown argued that this is common characteristic of technoscientific development, and other scholars pointed to a graph called the "hype cycle" that illustrated this idea (Brown 2003; Borup et al. 2006). Scholars put the point of the "hype cycle" this way: "Here technologies are seen to move along a path from trigger, to a peak in expectations, the plummeting into a trough of disillusionment before eventually giving rise to a range of somewhat more modest applications" (Borup et al. 2006, 291). However, they also argued that this linear rise, fall, and stasis merely simplifies the situation and does not take into account the variability or unpredictability of technoscientific innovations. Nonetheless, the search for DNA from fossils appeared to produce a strikingly similar pattern of development. As a researcher remarked, "I'd say this research discipline has developed the way that all science – new scientific disciplines – develop in that you have an initial wonderful discovery, you have lots of hype and high expectations, and then you come down to it with a bump, and then you do the hard work of working out what it all means and what you can really do: what is realistic and what isn't. […]" (5-01:52:50). In the late 1990s, scientists, faced with challenges of contamination and celebrity, found themselves in this precise position.

2.4.3 "Distorted by the probability of getting spectaculars"

There were also subtler adaptations of press and public interest in the search for ancient DNA. In a way, celebrity was embedded in the technoscience and publication practices around it. Reflecting on the history, one interviewee presented this perspective: "I do think it was a community that was distorted by the probability of getting spectaculars. It was interested in getting spectaculars; famous fossil, bit of DNA, *Nature* or *Science*." Both scientists and scientific institutions, along with media, played a role in encouraging this: "So, I think it was distorted by the attention that *Nature* and *Science* give, and maybe those journals deserve a little bit of criticism because they like the headlines and let some things in that weren't that scientifically interesting because they had the 'wow' factor." For this interviewee, this sort of spectacle was a characteristic of an emerging and exploratory practice: "I think when you're a field like that perhaps the wrong sort of person is attracted to it. If you work in a more mature field, the route to a big paper in *Nature* and *Science* is years of *painstaking* work. We had phases where you get your bone, do your PCR, sequence it, send your paper off – so it attracted and rewarded people who liked a short-cut to success, as papers in *Nature* and *Science* are perceived as a success." However, this interviewee capitalized on celebrity, too: "I have one paper with [X] where we got a couple hundred base pairs of mitochondrial DNA from [Y] in *Nature.* So, I can't preach and I don't mean to be preaching, but I think it was a distortion" (21-00:41:50). When asked if it felt like a distortion only in retrospect, this interviewee said, "Oh no. It felt like it then. We knew. So, in some ways it attracted some crazy dudes – some successful crazy dudes and some unsuccessful crazy dudes" (22-00:44:00). Celebrity influenced community culture.

The media spotlight increased the number of people interested in becoming involved in the field. It also attracted a particular type of person interested in a technical challenge and an eye for the spotlight. This shaped the sociology of the technoscience. Of course, technology was fundamental to the explosion of experiments but so was celebrity. *Jurassic Park* was an important impetus behind this. One interviewee speculated that the field may not have evolved to the extent that it did without *Jurassic Park*'s influence: "[…] If there was no *Jurassic Park*, I don't know how ancient DNA would be today. It could be that it would just drop. I've said before I'm surprised that it actually survived the time with the PCR, and I think if there was no *Jurassic Park* it might not have come to anything at all. I'm not saying that that would be the case, but it is possible" (34-01:04:00). Hype was both a cause and consequence of ancient DNA activity. In the process, it attracted particular personalities interested in a technical challenge or even fame. Interviewees remembered the 1990s as an exciting time. One scientist said "I think […] part of the reason it was fun was because you had to be slightly crazy in 1990 to start getting into this field. [X] and I were slightly crazy, but we did it […]." For this scientist, there was a shared sense of risk: "Everybody was slightly crazy – well, crazy isn't the right word, but it does kind of describe what I mean. […]. I think it's the fact that these people were naturally risk takers, and so that's the quirky kind of scientist" (4-01:48:00). Remembering the community during this decade, another added, "In the early days of the field, it was *ripe for failure*. Who's going to try and go out and get *DNA* from a *dinosaur*? […] If you wanted to set up a successful research career, and you wanted something safe, ancient DNA was *not your thing*. In the mid-to-late-90s? No way!" According to interviewees, the risk associated with this research influenced community culture: "So, it probably attracted a kind of scientist who was about the exact opposite of risk-averse […]. You can think of a cowboy mentality, a bit of like, 'I can do whatever.'" (22-00:44:50). Yet the risk of technology was offset by the reward of celebrity.

According to interviewees, the high-risk-high-reward nature of the search for DNA from fossils also attracted a competitive community. The race to be first in the field created competition. One scientist said, "Everyone else was running towards the prize. One of the things that has annoyed me about ancient DNA […] was that people were just running around to get the next sequence […]. And this [was the] whole problem in the field of 'the red and the dead' […]" (9-01:22:30). As another interviewee added, "[Y]ou can sequence an extinct species for the first time only once. […]. So, the second headline is always smaller than the first one" (14-01:12:20). The novelty of the technoscience translated to celebrity, and celebrity bred competition. One scientist said, "[…] [I]n ancient DNA it is relatively easy – getting a lot of attention – because it's a topic that naturally lends itself to media attention somehow. And I think that's one part that makes it so competitive and competition does not always bring the best out of people. […] I mean, many people are also very interested in media attention, so if it's important to be the first to get media attention, people do a lot of being 'the first.' […]." This scientist, as well as others, had a hypothesis to explain this: "[X] once mentioned it also has to do with the fact that you don't have to be particularly intelligent to be successful in ancient DNA – you have to be scrupulous to get the samples and to be faster than somebody else and to convince people to give you enough money." For example, "It's not like theoretical population genetics when you have to be a really good mathematician to make major contributions. So, it does not necessarily attract the *most* intelligent researchers, but it definitely attracts very competitive ones who are also *more* interested in presenting themselves to the media." Further, "That's also one thing in theoretical population genetics – you might get high citation rates, but no newspaper will write about it. Whereas if you can present the first mammoth genome, every newspaper will write about it" (15-00:24:00). Reflecting on the first generation of ancient DNA researchers, another researcher remarked: "My suspicion has been that the field attracted characters in its first incarnation. There was a lot of media splash, there was a movie, and so on, and perhaps it attracted people who were – [pause] – more confident […]. It might be inevitable that people like that maybe don't all get on that well when set in a room together" (2-57:00:00). A second said, "It's fun, interesting, but the caveat – because it's fun and interesting – [is] it's attracted some weird characters – [laughs] – and some of them are not so pleasant. I think it's a generation thing" (21-01:25:30). The appeal to be first in the field was a serious source of community competition.

In the 1990s, celebrity bred competition so fierce that some scientists opted out or felt pushed out. One scientist said, "I could have gone and done ancient DNA or I could have gone and done ancient proteins, and thank God I chose to do ancient proteins because everyone who started off doing ancient DNA has left the field because it didn't work." Reflecting on the 1990s to today, this same scientist said: "[W] […], she's had a really tough time. All the people early on […] are no longer in the field. [X's] supervisor's no longer in the field. [Y's] there but he was the junior to [W]. He went out and came back again. […] [Z] went out and came back in again" (9-01:13:15). While the celebrity surrounding ancient DNA activity could be a short cut to success, as some researchers referred to it, life in the spotlight could also be short lived: "A lot of people went straight to the super sexy sites […], and surprise, surprise! None of them are here anymore" (9-01:13:15). In a field of big risk and big reward, competition was, and is, severe: "Everybody's trying to become the big person in ancient DNA […]" (9-01:15:00). Another interviewee also opted out of ancient DNA for ancient proteins because of competition: "[…] [T]here was this enormous competition. I think it was very much, 'I'm going to get sexier DNA than you have.' […] I was horrified by what I saw as a student in that community which was the other thing that pushed me to proteins. […]" (39-01:10:00). Reflecting on the history of ancient DNA research, scientists suspected that the extreme enrollment, then drop out of researchers in the field was due to short term success of being first in the field: "People of average intellect had done incredibly well because they were the first into the field. There are very few intellectual super stars in the field: [X] is one of them, [Y] is another one – […] the real super stars are outside like [Z]. What's interesting is that everyone else, like me, is very average. […]. [Laughs]. Yet they've done really well because they were the first into the field" (9-01:16:00). However, another researcher remarked that success for some was more than luck: "In that first generation actually – amazingly – there were some *very* clever people. I mean, it's not just by chance that they discovered the field, right? And some of them *are* still active, still like leading the field, not just because they were the 'first.'" (8-00:52:10). Nonetheless, the opportunity to be first in the field, especially a field in the media limelight, created community competition.

For some scientists, there were disadvantages working within a field so closely connected to a major blockbuster movie. Cano's promotion of his paper with the release of *Jurassic Park* is a case in point. Writing for *The Los Angeles Times*, King highlighted the tension between science and the spotlight. Referring to Cano, King described the perks and problems he encountered: "At first, it had been fun – a trip to San Francisco for a special preview, congratulatory calls from other scientists, even the interviews. […]. But the 'hype,' as he called it, had worn thin. 'I can't get any work done,' he complained. He also seemed unsettled by sniping from some scientific quarters, questions about ethics and priorities" (King 1993). King noted, "There's a line between seizing the moment and forfeiting professional dignity, and Cano could only hope he had not crossed it" (King 1993). King quoted Cano: "'I got the fame,' he told a well-wisher in the lobby, 'but not the fortune.' In private, he put it more bluntly: 'I got shortchanged, and I also got burned. Why? Because I am naive.' Well, professor, that's show business" (King 1993). One interviewee also targeted this tension: "But in the scientific community there was a lot of skepticism, and to a certain extent, which I don't understand, there was this *anger* – that you had the audacity to publish this in the scientific literature" (31-00:19:00). Kirby specifically analyzed the positive and negative effects of too much media in this specific situation. Kirby noted that during this decade there was a "rise of celebrity culture" that made "associations with popular media desirable" (Kirby 2013, 57). He argued that there were times when science, press, and public interests conveniently coincided. However, Kirby also argued that some media situations could conjure a storm of dissent among colleagues. Kirby noted that Cano "became disenchanted with his agreement to help promote *Jurassic Park* because of his colleague's negative reactions" (Kirby 2013, 57). According to Kirby, close connections with the media were primarily problematic when it portrayed or promoted unrealistic expectations of real research. Yet for better or for worse, celebrity played a part in the development of ancient DNA research into a discipline.

In some situations, celebrity destabilized the technoscience, making it hard for researchers to "discipline" the discipline. By the mid-to-late 1990s, contamination was a serious source of concern and researchers found the credibility of their careers, as well as the reputation of the technoscience, on the line. On the one hand, the interplay between science and media had helped build ancient DNA research into a technoscience and even a celebrity science. On the other hand, the interplay of these interests sometimes came into conflict, challenging the technoscience's status as a rigorous and reliable practice. Researchers were caught in between a need to appeal to press and public for support, but simultaneously distance themselves from the hype that had come to characterize the field. Broks, speaking of science in general, put the predicament this way: "To maintain its authority it needs to be set apart from the general public, but to maintain its legitimacy it needs to appeal to the general public. Being set apart increases its alienation; making it more 'popular' undermines its authority" (Broks 2006, 107). Jan Golinski, historian of science, argued for a similar situation through his study of the history of chemistry in England in the late seventeenth century (Golinski 1989). He highlighted that natural philosophers, who once exploited the phenomena of phosphorescence as a spectacle to attract public audiences for legitimacy, now struggled to retain their authority as men doing more than "magic." Golinski argued that natural philosophers needed to seek but separate themselves from the public when expectations of legitimacy and authority came into conflict: "Wonder could be the parent of philosophy, but only if the spectators passed beyond simple admiration and began to exercise their reason to judge the significance of a phenomenon" (Golinski 1989, 24–25). Scientists in search of DNA from fossils encountered this tension, too. While spectacle and speculation had helped in the emergence, then evolution of a technoscience and a celebrity science, they posed problems, especially as practitioners tried to transform ancient DNA activity into an established practice.

Their response to contamination concerns was to build boundaries around the technoscience through material strategies based on criteria, technologies, and techniques, while their response to celebrity was to build boundaries through rhetorical strategies in an attempt to isolate the science from media interest or influence – something that scientists were starting to feel was contaminating the quality of the technoscience, too. This activity can be analyzed as boundary-work, attempts to demarcate science from non-science or less credible activities or approaches in science (Gieryn 1983). Thomas Gieryn, sociologist of science, wrote, "Put bluntly, a sociological explanation for the cultural authority of science is itself 'boundary-work': the discursive attribution of selected qualities to scientists, scientific methods, and scientific claims for the purpose of drawing a rhetorical boundary between science and some less authoritative residual non-science" (Gieryn 1999, 4–5). For Gieryn, there is no one and only way to do science, but rather there are different ways to draw or redraw the boundaries of what we see as science. Crucially, the debate was not about whether ancient DNA research was science or non-science, but whether it was a credible or non-credible practice. The answer was far from simple as ancient DNA activity was tied up in a mix of scientific, press, and public interests. This meant that scientists could not solely rely on technology or methodology as a way to draw lines between credible and non-credible research. Equally noteworthy, the point is not to argue whether media was "good" or "bad" for the discipline but to argue that intense interest in the search for DNA from fossils required researchers to address their successes and failures on a public platform. While boundaries were built using material strategies in response to contamination concerns, like criteria of authentication, they were also drawn using rhetoric, like "antediluvian DNA," in response to celebrity.

Here, it is crucial to recognize there is a subtle but significant difference between researchers' responses to "the *Jurassic Park* phase" and "the wild west." On the one hand, "the *Jurassic Park* phase" referred to the heyday of hype. It referred to the race for the oldest DNA, particularly DNA from the days of the dinosaurs. On the other hand, it is vital to recognize that in the 1990s ancient DNA activity was in an early, exploratory, and evolving phase of trial and error. It was "the wild west" as researchers tested, then defined the theoretical and technical limits. It was a necessary part of the process of disciplinary development. One scientist said, "It's like perhaps nineteenth-century beetle collecting, or something like that, where if you don't know what's out there – if you don't have some broad brush general idea of the what landscape looks like genetically – then […] you can't go into high level investigation. So, it's necessary to some degree to do that" (2-01:05:30). Yet because of its status as a celebrity science, researchers often associated contamination as an issue influenced by celebrity. In a celebrity science, it could be difficult to disentangle the two. Overall, celebrity influenced the development of ancient DNA research into a discipline, but by the end of the decade, some sought a turn to the technical in response to both issues of contamination and celebrity.

2.4.4 Conclusion

The hunt for DNA from fossils developed into a discipline under the constant gaze of press and public enthusiasm. From a media perspective, DNA from ancient and extinct organisms held a high news value which resulted in consistent media coverage. Yet it also held a high news value for scientists because its popularity translated to publicity, opportunity, and funding. The image of *Jurassic Park* was a textual and visual caricature of the potential power of ancient DNA that both reporters and researchers used as a pivot point for showcasing ancient DNA activity. Here, the press created opportunities for publicity which scientists could, and often did, take advantage of for the pragmatic purpose of obtaining research resources. Furthermore, researchers, research institutions, and journals like *Nature* and *Science* crafted their own opportunities for visibility. Yet there were also subtle adaptations of celebrity that were less deliberate, but nonetheless influential in placing the practice in the media spotlight. Media was a given and researchers recognized, then responded, to it. Celebrity was embedded in the technoscience.

This interaction between science and media resulted in a shared conceptual space. This space was productive but not without tension. According to some scientists, there were negatives to life in the spotlight, particularly when media expectations failed to coincide with research reality. In the mid-to-late 1990s, concerns about contamination put the technoscience's credibility on the line. Practitioners tried to maintain their authority over the practice by downplaying spectacle and speculation, and turning to the technical and theoretical issues instead. They started to build boundaries via technologies and methodologies to address contamination concerns. But this was not enough. As scientists of a technoscience in the media limelight, researchers were starting to react to celebrity, something that some thought was starting to compromise the practice. They thought popular interest and influence was a second source of contamination that undermined their credibility. Scientists tried to control celebrity by turning to rhetoric to distinguish differences between what some saw as science versus sensation. It appeared that contamination and celebrity went hand-in-hand. The two were difficult to disentangle. Ancient DNA's wide appeal to a wide audience bred a competitive community of practitioners in a way that shaped the sociology of the technoscience. Its celebrity status attracted a range of researchers who wanted to test its limits, making it difficult for scientists to oversee the discipline. Although credibility concerns were complicated by celebrity, they were also a product of the technoscience's status as an early, exploratory, and evolving practice.

2.5 CONCLUSION

This chapter argued that in the 1990s ancient DNA research developed into a discipline under intense public interest and extreme media exposure. Scientists were interested in investigating the relationships between extinct and extant organisms, and testing hypotheses about evolution, variation, and migrations of past populations. PCR was fundamental to their ability to answer these questions. But press and public interest in the search for DNA from fossils, present since its start, played a part in driving the field forward, too. *Jurassic Park* put the practice in the media spotlight and researchers worked in and around this celebrity. The media followed ancient DNA activity and framed claims of multi-million-year-old DNA in terms of a race or rivalry for the first or the oldest DNA. However, what mattered most was that scientists responded to the attention, positively or negatively and to differing degrees. The result was a shared conceptual space where professional, press, and public interests and expectations coexisted, then coevolved, into or in response to one another. Crucially, my argument for the role of celebrity does not diminish debates around contamination concerns. Rather, by highlighting the influence of the media, I have argued that contamination – while a legitimate theoretical, methodological, and technical issue – was much more than that. Practitioners reacted to contamination concerns by also reacting to the celebrity status of the technoscience, something they thought challenged, or at least complicated, their efforts to demonstrate that ancient DNA activity was credible. The media mobilized ancient DNA activity but destabilized it too, making it challenging for researchers to professionalize the practice. In this chapter, I argued that the interaction between science and media contributed to the co-construction of ancient DNA research into a technoscience and a celebrity science. However, by the mid-to-late 1990s, speculation and spectacle surrounding the practice – once a positive part of ancient DNA's disciplinary development – posed a problem for researchers trying to transform the evolving practice into an established one. How they handled the contentious status of the technoscience at the turn of the century would determine the future of the field.

CHAPTER THREE

RESPONDING TO CELEBRITY SCIENCE: BUILDING BOUNDARIES IN ANCIENT DNA

3.1 INTRODUCTION

This chapter is about the search for ancient DNA from 2000 onwards. It describes how a handful of practitioners produced a strict set of scientific standards for how to properly practice the search for DNA from fossils at a time when its credibility was challenged by contamination concerns. In this chapter, I argue that researchers reacted to credibility concerns by building boundaries around the practice to assert the technoscience's legitimacy within evolutionary biology. In 2000, a paper published in *Science*, "Ancient DNA: Do it Right or Not at All," established criteria for ancient DNA authenticity as a response to concerns about contamination. Criteria emphasized standardization and replication. Criteria was intended to ensure experimental expertise. However, these rules were also in part a response to the celebrity that surrounded the science. Further, some scientists used the criteria as a way to control competition within the field. In this chapter, I also argue that boundaries, designed according to the technology of the polymerase chain reaction (PCR), were challenged by a new technology called next-generation sequencing (NGS). NGS changed the search for ancient DNA into the search for ancient genomes. It introduced scientists into a new era of exploration. In the early 2000s, boundary-work was frequent as scientists tried to transform the technoscience from an emergent to an established practice. Yet boundary-work extended beyond scientists' day-to-day activities. It existed in their memories, affecting their individual and institutional identities, especially as they tried to make sense of their history by drawing a line between the discipline's emergent and more or less established status today. I argue that boundary-work, in response to contamination concerns and celebrity, was a crucial component of disciplinary development and how scientists tried to make sense of the future of the field.

3.2 PLAYING THE GAME

3.2.1 Introduction

This section is about the hunt for ancient DNA over a short but significant period from 2000 to 2005, as researchers responded to the evolving status of a technoscience in the limelight. First, I argue that they responded by setting strict standards in reaction to issues of contamination and authentication. Second, I highlight that at the turn of the century, criteria to detect contamination divided the community, resulting in two factions with two conferences and different places of publications. These criteria of authentication were used by scientists to control contamination, but criteria also served to control competition by defining how to, or who could, participate in the practice. Third, some scientists, in response to contamination concerns, attempted to restore the reputation of the technoscience within evolutionary biology by making the most of the theoretical and technical constraints of PCR. Practitioners also attempted to restore their credibility by reacting against celebrity, particularly as curiosity around the idea of resurrecting long-lost organisms continued to attract popular and professional attention.

3.2.2 "Do it right or not at all"

In summer 2000, scientists convened for the Fifth International Ancient DNA Conference at the University of Manchester. According to an article in *Science* by Erik Stokstad, it seemed the practice had emerged from the race for the oldest DNA and entered into a new phase of technological and methodological development (Stokstad 2000, 530). In his report, Stokstad recorded discoveries of new sites and sources for DNA, and new methods for recovering it without contaminating it. For example, he showcased a study by Hendrik Poinar, Svante Pääbo, and colleagues on a method that allowed the amplification of DNA from coprolites, or fossilized feces (Poinar et al. 1998). Using the chemical compound *N*-phenacylthiazolium bromide (PTB), they recovered DNA from feces found in a dry, cool cave in Nevada. DNA revealed that the coprolites were from an extinct ground sloth that died during the Pleistocene, offering scientists opportunities to study the diet of a long-lost species. Overall, caves seemed to be a unique site for molecular preservation, while the application of the PTB method to coprolites revealed a unique source of molecular information. Stokstad also spotlighted recent research by Alex Greenwood, Ross MacPhee, and colleagues who claimed to have recovered the first evidence of nuclear DNA (nuDNA), not just mitochondrial DNA (mtDNA), from the extinct woolly mammoth (Greenwood et al. 1999).[34] They also salvaged partial sequences of an endogenous retrovirus within the mammoth nuDNA (Stokstad 2000; Greenwood et al. 2001). Stokstad explained that although endogenous retroviruses are common across all creatures, and

[34] Since the 1980s, ancient DNA activity had been largely limited to the recovery of mtDNA primarily because of its abundance in animal or plant cells. However, mtDNA provides information for only one side of an organism's genetic history. mtDNA in combination with nuDNA provides a more complete picture of the genetics and population genetics of ancient and extinct species. See Hofreiter et al. (2001).

unlikely to offer information or insight about the life of an organism, this study led this team to consider the possibility of tracing the evolution and extinction of species through ancient pathogens (Stokstad 2000). Prior to this study, MacPhee and Aaron Marx had proposed a creative, but speculative, hypothesis that suggested that pathogens from humans or animals could be responsible for the mammoth's extinction (MacPhee and Marx 1997; Stokstad 2000). This hypothesis sent some scientists on a hunt for a preserved and infected mammoth that may exhibit evidence for the idea (Stokstad 2000). In spotlighting these studies, the article depicted a discipline striving towards specialization through theoretical and technical developments for the reliable recovery of DNA from less ancient, but still evolutionarily enlightening, organisms.

However, a month after the meeting, *Science* published another article with a distinctively different stance on the state of the field. Alan Cooper, University of Oxford, and Hendrik Poinar, working with Pääbo at the Max Planck Institute for Evolutionary Anthropology in Leipzig, published a piece called "Ancient DNA: Do it Right or Not at All" (Cooper and Poinar 2000). In it, they painted a picture of skepticism, criticism, and frustration with the development of the discipline. In general, the conference was responsible for their reaction, but they were specifically spurred by "one presentation" that "boldly opened with the claim that the field was now mature and could move ahead with confidence" (Cooper and Poinar 2000, 1139). Cooper and Poinar disagreed: "This optimism is unfounded, as demonstrated by the notable absence of 'criteria of authenticity' from many presentations at the conference" (Cooper and Poinar 2000, 1139). They cited the customary series of studies in high-profile journals that suffered from problems of contamination or replication, highlighting the damage it did to the discipline in terms of credibility. Although standards for ancient DNA authenticity had been suggested, and while some scientists adopted them, Cooper and Poinar felt that others ignored them. They even noted that researchers, editors, and reviewers of high-profile journals continued to publish work without proper checks and controls. They blamed them for failing to employ or enforce criteria that would help specialize the technoscience (Cooper and Poinar 2000, 1139). Cooper and Poinar were responding to a long legacy of issues that continued to plague the practice.

In response, Cooper and Poinar proposed a set of rules – "criteria of authenticity" – to reinstate a reputation of credibility for the technoscience. First, they required a "physically isolated lab" – an especially dedicated lab for ancient DNA activity to circumvent contamination from modern material. In the lab, checks and controls were required.

Generally, “control amplifications” or multiple extractions should be performed to test for contamination. Other checks included “quantitation” to first determine if enough DNA was available for amplification, as well as “cloning” to estimate the amount of endogenous or exogenous DNA in the PCR product. Further, sequences should exhibit evidence of degradation or “appropriate molecular behavior.” In other words, ancient sequences were expected to be short sequences of less than 500 base pairs. Longer sequences were questioned as contamination, or required justification. Additionally, they suggested employing indirect evidence of DNA preservation via “biochemical preservation” of other molecules like amino acids. Also, in the case of human ancient DNA where contamination concerns run rampant, sequences should be extracted and analyzed from “associated remains” or animal remains to confirm preservation from the same environmental setting. Further, results must meet criteria of “reproducibility.” This meant performing a second study with the initial extraction, as well as additional extractions of the same specimen. Finally, results should be reproduced through “independent replication.” This meant that a second sample of the specimen should be taken, extracted, sequenced, and confirmed in an independent lab by independent researchers (Cooper and Poinar 2000, 1139). Cooper and Poinar admitted that completing all nine criteria would be expensive and time-intensive, but that doing so was vital to the future of the field.

With concerns for ancient DNA authentication, the “Ancient DNA Lab” became known as a “clean lab,” a space special where contamination could be avoided. Drawing on guidelines outlined in previous publications, Cooper and Poinar argued that labs handling ancient material must be physically isolated from other molecular or microbial labs containing modern material (Cooper and Poinar 2000; Handt et al. 1994; Pääbo, Higuchi, and Wilson 1989). Other publications expanded on these expectations (Pääbo et al. 2004; Willerslev, Hansen, and Poinar 2004). For example, some scientists said that ancient DNA activity from extractions to experiments should be conducted in a physically isolated lab with positive air pressure and specific ventilation systems to prevent contamination via air flow when entering or exiting the lab. Ideally, these clean labs should be housed in a separate building from any building with a PCR lab. All equipment brought into the clean lab must be decontaminated via bleach or UV irradiation as appropriate. Further, the clean lab should be decontaminated with bleach before and after each entry, and every evening UV lights should be used to further sterilize the space. With every entry to the clean lab, researchers were required to dress in full body suits with gloves, shoe covers, hair nets, and face masks to avoid contamination during experimentation. Researchers were also advised to never enter the clean lab after working in the PCR lab to avoid cross-contamination between these work spaces. The physical separation of the "Ancient DNA Lab" from other labs, as well as the precautions that researchers were required to take when working in the lab, became a hallmark of the proper practice of ancient DNA activity. The presence or absence of a clean lab became a way in which some scientists measured the credibility of research results within this community.

Cooper and Poinar's "Ancient DNA: Do it Right or Not at All" was an influential though not isolated attempt to address contamination concerns. Rather, it was a reaction to a history of scientific, technological, and sociological developments in the search for ancient DNA. Since the late 1980s to late 1990s, researchers from disparate disciplines had come together to form a new area in evolutionary biology. During those decades, scientists sometimes found themselves in bitter disagreements over criteria for ancient DNA authenticity. As a budding technoscience, practitioners tried to produce proper protocols to ensure reliability of results (Pääbo 1989; Lindahl 1993; Herrmann and Hummel 1994; Handt et al. 1994; Höss et al. 1996; Krings et al. 1997). Indeed, the paper published on Neanderthal DNA in 1997 outlined the methodological precision that some scientists saw as necessary for demonstrating ancient DNA authenticity (Krings et al. 1997). Regardless, practitioners struggled to standardized the technoscience. Therefore, "criteria of authenticity" as mandated by Cooper and Poinar's article was a collation of criteria introduced over the years and from a frustration on behalf of some scientists who felt the field was not taking standardization seriously. One scientist said, "Various people – like Svante [Pääbo] and Tomas Lindahl and Alan Cooper and […] Hendrik Poinar – […] said there are issues […] and most people listened to them, who were actually published in the early papers or the erroneous early papers. […] But a few people didn't and carried on, ignoring them, and so on. And then, in […] frustration […], these criteria get published" (6-02:17:45). For Cooper and Poinar, failure to follow "criteria of authenticity" would jeopardize the future of the field. As Cooper and Poinar argued, "[F]ailure to do so can only lead to an increasing number of dubious claims, which will bring the entire field into further disrepute" (Cooper and Poinar 2000, 1139). To combat contamination concerns, they said, "If ancient DNA research is to progress and fulfill its potential as a fully-fledged area of evolutionary research, then it is essential that journal editors, reviewers, granting agencies, and researchers alike subscribe to criteria such as these for all ancient DNA research" (Cooper and Poinar 2000, 1139). It was a call for standards for disciplining the discipline.

The Fifth International Ancient DNA Conference was the event that provoked the publication of “Ancient DNA: Do it Right or Not at All.” As one researcher recalled, “At the ancient DNA conference […] in Manchester in 2000, [X] just let people present […] and [X] thought people in the audience would be asking questions: ‘How did you get that result?’ ‘That can’t be right!’ But nobody did. Everybody was very British and polite and just sat there” (5-01:34:00). According to this researcher, contamination concerns, as well as celebrity, were the reasons behind “Ancient DNA: Do it Right or Not at All”: “So, this is why Cooper and Poinar published that paper. That’s where it all came from and it’s this lack of self-criticism […], standing back from your work […] and saying, ‘Is this believable? Is this right?’ […] I think most scientists do, but some don’t because they just think, ‘Wow, this is great! I’ve got a great result and I can get a paper in *Nature* or *Science*. […].’” (5-01:34:00). Another interviewee shared a similar story: “We were […] at the lunch of an ancient DNA meeting […] bitching away about how most reports […] were repeating the same errors that we’d seen in the late ‘80s and in the mid ‘90s. It was just like, ‘Ah, for fuck’s sake! We’re doing it again!’” This interviewee cited contamination concerns and celebrity as causes for the paper’s publication, too, explaining that some scientists were “attracted by the sexiness of the work […] and publishing in *Science* or *Nature*” (32-00:28:00). The Fifth International Ancient DNA Conference may have provoked the publication of criteria of ancient DNA authenticity, but it also stemmed from years of concerns regarding contamination and celebrity.

Contamination was a real issue, but it was an issue further problematized by the celebrity that followed the field throughout the 1990s. To be clear, contamination was a real theoretical and technical problem, but it was more than a professional concern. It was also a public concern because some studies that seriously suffered from contamination had been, in the 1990s, published in high-impact journals, such as *Nature* and *Science*, and broadcasted across the mass media. For researchers, their reputation and the technoscience’s reputation was at stake. In “Ancient DNA: Do it Right or Not at All,” Cooper and Poinar argued that adoption and adherence to the proper protocols was essential. It was not just a call for criteria, but a demand to discipline ancient DNA activity in its development as a technoscience and celebrity science.

3.2.3 “Do it with me or not at all”

In 2000, the Fifth International Ancient DNA Conference in Manchester was a crossroads for the community. During the 1990s, the conferences transitioned from an atmosphere of enthusiasm to one of skepticism. Issues of contamination ultimately divided the conference and community. One scientist said, "You have this divide which is sort of crystallized by having these two different conferences and two different types of scientists […]; the ones who do proper work in laboratories in clean rooms and the other ones who […] work in forensic labs or even medical labs where there is no proper […] thinking about controls and contamination [...] So, anyway, that's […] this division – the 'believers' and the 'non-believers' […]" (5-01:01:00). This division between "believers" and "non-believers" centered around debates about contamination and scientific standards for avoiding it. While both sides were aware of contamination, they differed in the degree they employed methods to test for ancient DNA authenticity. Roughly, the "non-believers" were suspicious, even dismissive, of research results produced by the "believers." The "non-believers" – which included individuals such as Pääbo, Cooper, and Poinar as well as their students – more or less viewed research by the "believers" – which could be considered to include individuals such as Hagelberg, Hummel, Herrmann, and others – as less rigorous and therefore, their work was less believable. These terms – "believers" and "non-believers" – are categories that interviewees on both sides of the schism use in reference to themselves and others. There are some scientists who also refer to the schism as a difference between the "haves" and "have nots," and while not all interviewees used both or even one set of terms to describe the split, they all recognized the split, though to differing degrees, and its influence on the sociology of their science. However, it is important to note that the line between the "believers" and "non-believers" is not necessarily hard and fast. It is permeable. Indeed, some scientists tried to collaborate across the schism. Nonetheless, the caricature of "believers" and "non-believers" helps scientists make sense of an important issue, concerns about contamination, and its influence on ancient DNA's disciplinary development. However, it is by no means the only map of interactions that interviewees try to draw throughout the history of this community (discussed in detail in Section 3.2.4).

By the Fifth International Ancient DNA Conference in Manchester in 2000, the tension was tangible. One scientist on the side of the "believers" recalled, "I went to this awful conference […] and none of us were invited to talk, so we all had to have posters and they ignored the posters" (23-00:41:00). According to this scientist, "It really looked like a closed shop. You came away with a very strong message: 'Don't bother coming back.'" (23-01:10:00). In response, the "believers" and "non-believers" went their separate ways and two separate factions were formed. This schism resulted in two different conferences;

the International Conference on Ancient DNA and Associated Biomolecules and the International Symposium for Biomolecular Archaeology (ISBA), with the "believers" attending the former and the "non-believers" attending the latter. The schism was acknowledged at the time but also expanded on and reinforced by interviewee's retrospective references to it.

In 2002, the Sixth International Conference on Ancient DNA and Associated Biomolecules, a continuation of the original meetings, was hosted at the Hebrew University of Jerusalem in Israel, but some scientists chose not to attend ("The 6th International Conference on Ancient DNA and Associated Biomolecules" 2002). As a "non-believer" explained, "[…] [T]here was a divide – there was a split – because you had the next ancient DNA conference going to Israel with people like [X] […], and so […] [Y] and I said, 'Well, we're not going to that one.'" (5-00:57:30). In 2004, the First International Symposium for Biomolecular Archaeology was hosted at the University of Amsterdam in the Netherlands as an alternative avenue for the "non-believers" who doubted the authenticity of work by the "believers" ("The 1st International Symposium of Biomolecular Archaeology" 2004). Both conferences continue today. As another "non-believer" explained, "The ISBA only started off because of the ancient DNA meetings. […]. Apparently it's still going, and apparently people are still publishing stuff about insane stuff which is completely wrong! […] But that's why ISBA started – because of the split in the community" (22-00:56:00). A "believer" also reflected on this schism: "[W]e had a whole period which we call period of confirmation […] which is still being challenged by the way – the bastards out there – [laughs] – who say, 'We don't really believe.'" (28-00:17:00). While conferences were loci of community conflict, contamination further divided the discipline into alternative places of publication.

Scientists on the side of the "believers" turned to alternative avenues for publication and research recognition because they found difficulty publishing through other outlets. One interviewee presented this perspective: "There are two major divisions. […] You can see where they publish. The people on the more critical side ["non-believers"] tend to publish in higher impact journals. The people who are less critical ["believers"] tend to publish in journals no one has ever heard of" (6-01:02:40). In reference to the "non-believers," one "believer" said, "[…] They threw the baby out with the bath water and they just disregarded any work that didn't have dedicated air-conditioned facilities for the sole use of ancient DNA work. […] [T]hey just negated anything that people like me found" (23-00:37:25). This same scientist explained, "They just disregarded it. They would say, 'Well, of course you can't believe that because they don't do this and they don't do that.' It got very annoying" (23-00:39:15). In response, "believers" found other publication opportunities: "[W]e just ignored them and published in medical microbiology journals or multidisciplinary journals" (23-00:37:25). The split, based on different types of scientists from different epistemic and scientific traditions, impacted the community culture.

"Ancient DNA: Do it Right or Not at All" represented a divisive moment in the technoscience's history. Though criteria were intended to control contamination concerns, they also functioned to control professional competition. There were several scientists who used criteria to determine who could properly participate in ancient DNA activity. One researcher described the divide as a "religious schism" where people "stopped talking to each other" and "dissed each other" and "sabotaged each other." According to this researcher, there were "[d]ifferent conferences and different prophets" which was "[j]ust like religion" (6-02:17:45). For theoretical and technical reasons, criteria were enforced to control contamination concerns. Sociologically, however, criteria also controlled competition, forcing the field into different directions, separate conferences for presenting research, and specific journals for publishing results. As one researcher recalled, "[…] '[Ancient] DNA: Do it Right or Not at All' is like the nucleus of one era of *total proprietary*: 'Ancient DNA belongs only in Ancient DNA Labs. If you don't have this – if you don't do it *exactly* as we say – you're out. We won't publish your research.'" (27-01:14:45). One interviewee, as well as others, called "'Ancient DNA: Do it Right or Not at All'" – the "criteria of authenticity" – "'Do it With Me or Not At All'" (11-01:29:15). The hard line that some scientists sought was a response to the fact that the credibility of the technoscience was on the line.

Pääbo's and Cooper's labs, on the side of the "non-believers," embodied a powerful conservative philosophy in an attempt to transform the search for ancient DNA from an emergent into an established practice within evolutionary biology. In 1997, Pääbo became Director of Evolutionary Genetics at the Max Planck Institute for Evolutionary Anthropology in Leipzig. In 1999, Cooper became Director of the Henry Wellcome Ancient Biomolecules Centre at the University of Oxford. These labs became politically powerful but conservative centers for ancient DNA activity. Both labs advertised strict adherence to protocols, but their conservatism did not preclude productivity or publicity. Rather, Leipzig and Oxford published prolifically in journals such as *Nature* and *Science.* Referring to Oxford, one scientist said productivity and publicity were the expectation: "*Science* paper. *Nature* paper. […] And that was like the expectation. Everybody had to get a *Science* or *Nature* paper. […] Everything that was being generated was either going to be *Science* or *Nature*" (22- 00:22:10). Leipzig and Oxford tried to balance rigor with high productivity linked to publicity. Their professional but public position on the technoscience appeared to set a standard for ancient DNA activity. In building technical boundaries around the technoscience, these labs tried to send a statement to others that ancient DNA activity was an exclusive business.

Pääbo's and Cooper's labs were part of the critical community of "non-believers." One interviewee described Cooper as the "Chief Challenger" while another called Pääbo the "Dark Lord" (28-00:17:30; 12-01:41:20). Their ideologies influenced a generation of rising researchers, but the ways they operated in the discipline differed. One scientist said, "I think they, together, had a pretty strong influence on this conservatism, with the difference that Alan propagated more aggressively than Svante did. […]" (15-01:18:00). With Cooper at Oxford, students were instructed to disassociate themselves from the "believers." One researcher recalled, "When I showed up, I said, 'Oh, look Alan, there is an ancient DNA meeting.' He was like, 'You can't go.' He forbid everyone in the lab from going to that meeting because that schism had already taken place […]" (22-00:56:00). Pääbo at Leipzig, despite his role in founding the field, separated himself, his work, and his lab from the rest of the community, "believers" and "non-believers" alike. His disassociation from the community came through his absence at conferences and disregard for most, although not all, work outside of Leipzig. In reference to the disciplinary divide, one researcher recalled, […] [I]t was present, definitely, but it took me some years to realize it because I started in Svante Pääbo's lab […]. So, for some time I didn't even realize that the other part of the community existed […]. […] [I]n Svante's world, it basically didn't exist. It was nothing one needed to cite, nothing one needed to read. So, I kind of knew there was something, but it was something completely unimportant (15-00:19:00). Both schools of thoughts and their followers were built on strategies of negation. This philosophy of "Do it With Me or Not at All" shaped how students, as well as colleagues and collaborators, viewed their own work in relation to the rest of community.

3.2.4 "Bound up by the limits of the technology"

Following the Fifth International Ancient DNA Conference and "Ancient DNA: Do it Right or Not at All," "non-believers" focused on restoring their reputation by demonstrating that ancient DNA activity could be a rigorous and reliable practice. For many, this was a technical task. It was also conceptual. Eske Willerslev, promoted to professor at University of Copenhagen following his postdoctoral position with Cooper at Oxford, was an early career scientist trying to make his way in a competitive community. To do so, he and colleagues looked for creative ways to make their mark on the field. In 2003, they decided to search the dirt for DNA by taking soil samples from the permafrost in Siberia (Willerslev et al. 2003). From these sediments, they found an array of plant and animal DNA from the Holocene to the Pleistocene, including sequences from mammoths, bison, and horse, as well as the oldest plant DNA to date. Even without evidence of macrofossils, the dirt in which these plants and animals once lived could provide information about their evolution. Similarly, Tom Gilbert, doctoral researcher at Oxford, searched other sources likely to yield DNA. In 2004, scientists suggested that hair was a reliable reservoir of DNA and had the additional advantage of minimizing destructive sampling of paleontological or archeological material (Baker, McCormick, and Matteson 2001; Bonnichsen et al. 2001; Gilbert et al. 2004). Scientists also sought to improve the amount of information they could recover from fossils by generating ancient genomes from only short sequences of DNA. In 2001, the first complete ancient mitochondrial genomes from two species of moa were published; a task that required reassembling the nearly 17,000 base pairs of the mitochondrial genome from short damaged sequences of DNA (Cooper et al. 2001; Haddrath and Baker 2001). Other accomplishments included the first genome of the approximately 100-year-old Spanish flu virus and the first genomes of 40,000-year-old Pleistocene cave bears (Taubenberger et al. 1997; Reid et al. 2002; Tumpey et al. 2005; Noonan et al. 2005). These were technical and conceptual developments that founded the feasibility of converting short DNA sequences into genomes to infer more information about evolutionary history, including the diversity and divergence of species. Working within the constraints of contamination and PCR, researchers sought a shift towards genomics and population genetics.

This shift towards population genetics reflected a realization among researchers that if the technoscience were to become a relevant practice, it would need to address questions on the population, not just the individual level. By the late 1990s, researchers realized that phylogenetic questions based on a single sample of a single iconic species were of limited value. By the early 2000s, some tried to switch from studying the individual to the group. One of the earliest examples was a paper by Jennifer Leonard, Robert Wayne, and Cooper on Ice Age brown bears (Leonard, Wayne, and Cooper 2000). As one scientist said, “The big change in terms of moving into population genetics – away from phylogenetics – [was]

to provide population genetics with a time scale which it had never had before […]" (32-00:25:40). The brown bear study accomplished this. It demonstrated that the present distribution of brown bears in terms of demography and geography was distinctly genetically different from its past. Ancient DNA showed a side of the story that was invisible with inferences from modern genetic material alone. This same scientist said, "That was a big conceptual breakthrough at that point. It was like, 'Shit. We can do real population genetics rather than systematics.'" (32-00:21:00). Another early example of this transition was a study by Beth Shapiro, doctoral researcher at Oxford (Shapiro et al. 2004). This study was conceptually and technologically important for its large number of samples, use of statistical demographic modeling, and conclusions that challenged assumptions about bison evolution and extinction. These developments, among others, demonstrated that the value of the technoscience was its ability to test hypotheses about the evolution and extinction of past populations (Loreille et al. 2001; Vila et al. 2001; Barnes et al. 2002; Hofreiter et al. 2002; Leonard et al. 2002; Ritchie et al. 2003; Hofreiter et al. 2004; Shapiro et al. 2004). They demonstrated ancient DNA's research relevance to evolutionary biology, conservation biology, and our understandings of climate change and extinction.

Yet within these developments that seemed to suggest the technoscience's research relevance, some scientists continued to feel the need to build boundaries around the practice in response to the celebrity that still followed the field. Pääbo, Cooper, and Willerslev were three dominant forces in the field who used their influence to demarcate their work from what they saw as sensational science. In independent publications, Pääbo and coauthors, as well as Willerslev and Cooper, reminded readers of the short but sensational history of the field (Pääbo et al. 2004, 661; Willerslev and Cooper 2005, 3). According to Pääbo and coauthors, ancient DNA research was a specialty technique defined by criteria on the use or misuse of ancient DNA technology: "The study of ancient DNA has the allure of time travel and attracts much attention and many practitioners. However, the generation of results that are reliable, reproducible, and interesting requires more than the mere application of methods that are commonplace in most molecular laboratories." Yet ancient DNA activity required more than specialized skills. For Pääbo and coauthors, a project's purpose mattered, too. They argued that researchers should try to answer scientifically significant questions: "The first prerequisite of any ancient DNA project should be a clear understanding of the biological question at hand and how analysis of ancient DNA is an essential aspect of addressing the question." To put this point into perspective, they cited a series of studies they thought favored popular interest over scientific importance: “Other projects such as ancient DNA analyses of public personalities such as Christopher Columbus, Jesse James, or former U.S. presidents may be novel and of interest to the public. However, they are devoid of any larger scientific contribution and sometimes ethically questionable” (Pääbo et al. 2004, 670). Published in 2004, two decades following the discovery of DNA from the quagga in 1984, this paper demonstrated a professional perception that ancient DNA’s populist past was far from forgotten. In fact, the allure of time travel continued to attract professional and popular interest to the extent that scientists felt the need to address it.

To address credibility concerns, researchers played up the science by downplaying the spectacle and speculation associated with it. Like the previous paper, Willerslev and Cooper referred to the sensational studies that characterized the 1990s but challenged the credibility of the practice (Willerslev and Cooper 2005, 5). However, they argued that the search for DNA from fossils, despite its populist past, was developing into a discipline: “Despite this somewhat tarnished history, recent advances in knowledge about the tempo and mode of DNA template damage, sample contamination and biochemical diagenesis have improved standards and aDNA is now emerging as a viable scientific discipline” (Willerslev and Cooper 2005, 5). There were several studies, for example, demonstrating ways to recognize or characterize ancient and authentic DNA by postmortem damage patterns (Gilbert et al. 2003a; Gilbert et al. 2003b). According to Willerslev and Cooper, these studies, among others, suggested that the search for DNA from fossils was developing into a discipline.

However, new developments also spurred popular, even professional, interest in the idea of bringing back extinct creatures. For example, as researchers recovered nuDNA sequences from ancient and extinct organisms (Greenwood et al. 1999; Hofreiter et al. 2001; Poinar et al. 2003; Willerslev and Cooper 2005), some speculated that this information could be used towards reconstructing ancient genomes and resurrecting extinct species. Yet Willerslev and Cooper engaged the topic only to say it was an impossible reality: “It is important to note that even if it becomes possible to reconstruct ancient nuDNA sequences, it would still be impossible to bring extinct organisms back to life. Among many other requirements, the cloning of complex organisms needs a complete and undamaged nuDNA genome, packaged correctly, and a compatible maternal host.” For Willerslev and Cooper, the idea was “unfeasible” (Willerslev and Cooper 2005, 10). Nonetheless, some scientists outside the discipline were interested in the idea. Taxidermist

Reinhold Rau, for example, founded a program to back breed the quagga, while Michael Archer, a paleontologist, had dedicated himself and his research to bringing back the thylacine (Colgan and Archer 2000; "The Quagga Project" 2016). In Japan and Russia, scientists searched for the sperm of a permafrost-preserved mammoth in hopes of viable DNA to help bring it back to life (*Associated Press* 1996; Johnson 1998; Stone 1999). Overall, the media portrayed these projects by linking them to *Jurassic Park*. *Sydney Morning Herald*, for example, reported, "Jurassic Park techniques may bring back thylacine," while *Discovery Channel* showcased the end of extinction with documentaries on resurrecting both the thylacine and mammoth ("Raising the Mammoth" 2000; "Land of the Mammoth" 2001; "End of Extinction: Cloning of the Tasmanian Tiger" 2002; Smith 2002). Geophysicist Sergey Zimov also announced Pleistocene Park, a potential place for the mammoth to live if brought back to life (Ryall 2002; Lovegren 2005; Zimov 2005). *Newsweek* called it "A Real-Life Jurassic Park" (Margolis 2006). In other words, the search for DNA from fossils was still connected with the spectacle of resurrection.

By 2005, however, the community started to see that the rules for determining ancient DNA authenticity were not infallible. DNA degradation patterns and processes were far from understood, concerns about contamination still plagued the practice, and PCR was limited in the kinds of questions it could be used answer. Overall, researchers were realizing that criteria of authenticity did not, after all, certify authenticity. Interestingly, Willerslev and Cooper were two practitioners that pointed this problem out (Willerslev and Cooper 2005). For example, amino acid racemization, developed in the 1990s, had been used throughout the 2000s used as an indirect proxy to estimate DNA degradation in fossils (Poinar et al. 1996). However, several studies questioned its reliability, arguing that processes that controlled or contributed to DNA degradation were still poorly understood. For example, some studies exhibited evidence that DNA depurination did not consistently correlate to amino acid racemization in all tissue types (Poinar et al. 1996; Collins, Waite, and van Duin 1999; Collins et al. 2002; Schmitz et al. 2002; Serre et al. 2004; Pääbo et al. 2004; Willerslev and Cooper 2005). This mattered because amino acid racemization, although used as an indirect proxy to estimate DNA degradation, had been a crucial component for ancient DNA authentication. Reflecting on this, one scientist said, "[…] [I]f you look at the field of ancient DNA […], also methodologically speaking […], *a lot* of that stuff has turned out after five or ten years to basically be *wrong*. […] [F]or example, something like amino acid racemization […], I mean, now we know it's complete crap, right? […] Everybody laughed. I mean, papers were *rejected* by *hundreds* based on whether they had done that, right? And now we know […] it's complete *bollocks*" (7-00:33:00). As uncertainty surfaced about the reliability of indirect and direct methods for ancient DNA authentication, criteria became contested.

In 2005, several scientists challenged the criteria of "Ancient DNA: Do it Right or Not at All" through one particular publication (Gilbert et al. 2005). Ultimately, this paper was a counter claim to the boundaries that had divided the community into "believers" and "non-believers." It was an attempt to draw different distinctions amongst credible and less credible research in the field in light of continuing contamination concerns. In this paper, Gilbert and coauthors – Hans-Jürgen Bandelt, Michael Hofreiter, and Ian Barnes – traced the evolution of criteria for ancient DNA authentication "from a few relatively simple suggestions" to "a more detailed and extensive list of requirements, resulting in the well-known nine key criteria of Cooper and Poinar" (Gilbert et al. 2005, 541). They noted that Cooper and Poinar had created criteria as a guide for ancient DNA authenticity, but in practice these guidelines did not actually guarantee authenticity. In fact, it could, in some instances, construct a false façade of reliability. One scientist said that "criteria of authenticity" began as a "guide" but became a "religious doctrine" that was "blindly followed." This produced two problems. First, scientists who fulfilled all nine criteria found their papers published. However, in some cases, despite completing all criteria, they were still "publishing bad results." Second, scientists who did not fulfill all nine criteria found their research rejected, and in some of those cases they were "failing to publish good results" (6-02:17:45). Gilbert and coauthors explained that the "authenticity and reliability of ancient DNA data arise from a complex interplay of several poorly understood areas of knowledge" and that "no clear-cut answer exists as to what makes a study reliable" (Gilbert et al. 2005, 542). In reference to "Ancient DNA: Do it Right or Not at All", a researcher remarked, "And the *big* mistake […] with that paper – *huge* mistake […] with that paper – was not putting at the end of that list: '[…] *If* the result […] passes the criteria […] it's probably still wrong. It's just that you failed to disprove it.'" (32-00:28:00). For this set of scientists, criteria of authenticity were far from infallible.

This counter claim was not necessarily rejecting the criteria for authentication. Rather, scientists were bringing attention to the fact that the criteria were imperfect and that dogmatic dedication to the criteria was problematic. Gilbert and coauthors suggested a solution: "It is our opinion that ancient DNA researchers should take a more cognitive approach with regards to assessing the reliability and conclusions of their data. Suggested criteria remain important, and should not be lightly discarded, but we advocate that, in place of planning or assessing studies by using criteria as check-lists, consideration should be given on a case-by-case basis as to whether the evidence presented is strong enough to satisfy authenticity given the problems" (Gilbert et al. 2005, 542). Here, the authors argued that scientists should assess their project by asking questions about the feasibility of the study. Does, for example, the age and environment of the sample suggest DNA preservation? Or is there information about the handling history of the sample that might suggest prior contamination that might be difficult to detect and therefore jeopardize ancient DNA authenticity? With these considerations, they made a further and slightly sarcastic suggestion: "In short, perhaps a tenth commandment should be added to the nine key criteria: 'Thou shalt interpret the veracity of the data by a critical consideration of all available information.'" (Gilbert et al. 2005, 544). By the mid 2000s, criteria for ancient DNA authentication were openly contested. A counter claim to the "Do it Right or Not at All" philosophy, this paper made the point that there could be, and should be, flexibility for evaluating the validity of ancient DNA studies on a case-by-case basis. The authors valued contamination concerns and employed procedures to avoid or detect but also argued against a dogmatic dependence on the criteria.

While the technoscience had evolved, it was still plagued by problems. Contamination, even five years following "Ancient DNA: Do it Right or Not at All," was still an issue. Considering PCR, one researcher remarked, "[…] [I]t's a mess. Half the time nothing happens. If something does happen, you can't repeat it. You get contamination you can't get rid of and you have no idea where it's coming from. So, it ends up being like voodoo in the lab. You have all these rituals and you can't figure out where anything is coming from. [Laughs]. And it's *so frustrating*! […]" (27-00:32:00). This had consequences for practitioners as they tried to transform the technoscience from an emergent to an established practice: […] "I think ancient DNA was on a dangerous path for a long time with PCR because we'd been given money and given money and given money and we weren't really making any advances and we weren't really saying anything that was that extraordinary […]." According to this researcher, the community felt constrained by PCR's limitations: "And we were kind of stuck. You had a whole generation of researchers – this is what they wanted to do – but they were bound up by the limits of the technology" (27-01:34:30). According to another interviewee, even the Leipzig lab – a hub for ancient DNA activity – struggled with the technology, too: "When I interviewed at Leipzig […], [X] told me […], 'Yeah. I work on ancient DNA, but whatever you do, don't get into this.

It’s a completely dead end thing” (42-00:16:30). PCR and it shortcomings posed problems for the future of the field.

The year 2006 captured this technological tension. In “The Year of the Mammoth” Cooper spotlighted three studies that reported the recoveries of the first mitochondrial genomes, over 16,000 base pairs, of one the most charismatic creatures; the woolly mammoth. This was an amazing achievement considering the first mammoth DNA sequences, approximately 400 base pairs, were recovered just over a decade ago (Hagelberg et al. 1994; Pääbo, Höss, and Vereshchagin 1994). Cooper wrote, “Mammoth mitochondrial (mt) genomes are apparently on a similar schedule to London buses – you wait for ages and then suddenly three come along at once” (Cooper 2006, 311). Each study accomplished similar achievements, but they did so through distinctly different techniques. Cooper spotted the significance of this: “The very divergent methods used in these three studies also neatly represented the past, present, and future of ancient DNA (aDNA) research” (Cooper 2006, 311). The first paper by Evgeny Rogaev and colleagues used PCR, while the second paper by Johannes Krause and coauthors used a multiplexing method, a variation of PCR that simultaneously amplifies multiple targets as opposed to just one target (Rogaev et al. 2006; Krause et al. 2006; Thomas, Bradman, and Flinn 1999). For Cooper, these studies represented the past and present of the field, but the third study offered an opportunity for technical transition into the future. This third study, by Hendrik Poinar and collaborators, recovered 13 million base pairs of mammoth mtDNA and nuDNA sequences using a new parallel pyrosequencing system developed by 454 Life Sciences (Poinar et al. 2006). This innovation made a dramatic difference in the sequencing of DNA from fossils. Collectively, these papers represented a conceptual and technological snapshot of the discipline’s past, present, and future: “This is an exciting time, as the opportunities by the new parallel sequencing system will allow researchers to contemplate large-scale studies of ancient genomes, and promise to finally release the full potential of aDNA to reveal evolution in action” (Cooper 2006, 313). With this development, scientists started to turn from PCR to these new sequencing technologies in hopes of transforming the technoscience from an evolving practice into an established one.

3.2.5 Conclusion

In retrospect, the year 2000 was a divisive year for the community. The Fifth International Ancient DNA Conference was a point of contention regarding contamination. Cooper and Poinar consolidated these concerns in their publication “Ancient DNA: Do it Right or Not at All." These criteria of authenticity, initially intended to discipline the discipline, ultimately divided the community into self-subscribed "believers" and "non-believers" concerning ancient DNA authenticity. Pääbo and Cooper were chief challengers, expressing conservatism and exercising their political power over its future through schools of thought and schools of followers. Further, criteria took on a scientific and sociological function as researchers used it as a way to control community competition. This schism was but one way that interviewees tried to depict disagreements over contamination concerns, although it was by no means the only way to characterize the entirety of ancient DNA activity during this decade. As some scientists endorsed criteria, they engaged in methodology, experimenting with unique sites and sources for DNA preservation in order to understand DNA degradation. The goal for some scientists was to make their research relevant to the genetics, genomics, and population genetics communities. In parallel, however, the idea of bringing back extinct creatures like the quagga, thylacine, and mammoth attracted attention. Ancient DNA's close connection with spectacle was still an issue for some scientists. Ultimately, the creation of criteria was a reaction to concerns about contamination and celebrity. Researchers built boundaries around the technoscience in an attempt to restore their reputation, and the technoscience's reputation, in response to its short but sensational history.

3.3 CHANGING THE GAME

3.3.1 Introduction

This section concerns ancient DNA research from 2005 onwards as researchers adapted the technology of next-generation sequencing (NGS) to suit their search for ancient DNA. First, I demonstrate that NGS offered new opportunities for researchers to overcome many of the contamination concerns associated with PCR. According to practitioners, the practice, formerly defined by PCR, was radically reformed by NGS. The new technology rescued the technoscience, but the new infrastructure that came with this technological transition meant that some labs were lost in the move. Second, although some labs were lost, others struck ahead. The Neanderthal Genome Project, for example, showcased the powerful potential of NGS as applied to fossils. The technoscience's celebrity continued to follow the field. Finally, the new technology introduced researchers to a new era of exploration. Practitioners, once in pursuit of ancient DNA, turned to ancient genomes. The race for the first ancient genomes, sites, or sources of molecular preservation shared striking similarities to the 1990's race for the oldest DNA. The media spotlighted these stories. *Nature* and *Science* frequently featured them. Practitioners nurtured the attention, too. Further, the search for ancient genomes renewed the press's and public's enthusiasm for bringing back extinct species, particularly as professionals gathered together to discuss its feasibility in light of technological advances. Overall, the boundaries scientists built around the technoscience, boundaries based on PCR protocols for addressing concerns about contamination, became less important, although not irrelevant, as many turned to the new technologies and techniques made possible with NGS.

3.3.2 "The big game changer"

Since the early 1980s, the search for ancient DNA had been mostly based on Sanger sequencing. Introduced in 1977, this method became the main method for DNA sequencing for the next three decades (Sanger, Nicklen, and Coulson 1977). Ancient DNA had been heavily dependent on Sanger sequencing, as well as PCR, but by the turn of the century the limitations of these techniques and associated concerns about contamination became problematic for practitioners. Researchers struggled between visions of what they wanted to accomplish and what was achievable. In 2005, however, a new method called parallel pyrosequencing, also known as next-generation sequencing (NGS), was introduced by 454 Life Sciences Corporation, a biotechnology company in Connecticut (Margulies et al. 2005). NGS could produce hundreds of megabases-to-gigabases of sequences in a single run, increasing the speed while decreasing the overall cost of sequencing.[35] There was a dramatic difference between the pre-NGS and post-NGS era. For example, the Human Genome Project, started in 1990 and finished in 2003, cost just under $3 billion. But NGS, a product of the project, dramatically cut required resources. In 2007, for example, scientists sequenced the genome of James Watson – one of the researchers responsible for discovering the helical structure of DNA – in two months and for less than $1 million, "a 1,000-fold improvement over the cost of the decade-long Human Genome Project" (Rothberg and Leamon 2008, 1123). Within a short time span, NGS surpassed Sanger sequencing because of its efficiency and massively parallel nature. Practitioners called this a "paradigm shift" (Voelkerding, Dames, and Durtschi 2009, 461-

[35] The technology of next-generation sequencing (NGS) is a general term to describe a variety of machines that use parallelized platforms to sequence more than one million short reads of DNA (50-400 base pairs) in a single run. There are a number of NGS platforms varying in their chemistry and sequence read technology. Two instruments that were widely used in ancient DNA research in the late 2000s were Roche (454) GS FLX, a technology based on parallel pyrosequencing, and Illumina (Solexa) Genome Analyzer, a method based on reversible terminators. The 454 technology generates longer reads of DNA (over 400 base pairs) but is somewhat error-prone in homopolymeric regions (e.g. CCCCCC), while Illumina generates shorter reads of DNA (100-150 base pairs), but in greater numbers. See Margulies et al. (2005) and Knapp and Hofreiter (2010).

462). With it came an unprecedented era of exploration as genetics transitioned into genomics (Schuster 2008; Voelkerding, Dames, and Durtschi 2009). Those in search of ancient DNA were paying attention, too, with some turning to the technology in search of ancient genomes.

In the mid-to-late 2000s, NGS changed the search for DNA from fossils in terms of scale and scope of data production (Margulies et al. 2005; Millar et al. 2008; Knapp and Hofreiter 2010). In 2005, for example, researchers recovered nearly 27,000 base pairs of ancient genomic data from two 40,000-year-old cave bears (Noonan et al. 2005). A second study using NGS was published less than a year later. In 2006, researchers used parallel pyrosequencing to recover 13 million base pairs of ancient genomic data from a 28,000-year-old woolly mammoth (Poinar et al. 2006). Published within six months of each other, these papers showcased the dramatic difference in techniques (Knapp and Hofreiter 2010). One scientist said, "[…] [T]he amount of data you could get actually completely exploded. […] [W]e published a paper in July 2005 that was shot-gun sequencing, but with the old method you clone your DNA into bacteria and you sequence the bacteria clone. The data that we got was 27,000 base pairs." But new techniques dramatically enhanced the quantity of data. This same scientist said, "Six months later, the first NGS data set on ancient DNA was published with 13 million base pairs. […] So, it changed by almost three orders of magnitude within half a year. And now people are publishing data sets which are […] another three to four orders of magnitude. So the change was massive, absolutely massive" (15-00:34:30). This study demonstrated NGS as a powerful set of technologies for genetic and genomic research.

NGS renewed scientists' confidence in ancient DNA research. As one scientist said, "I used to joke that I was a retired ancient DNA researcher, but then the big game changer, without a shadow of a doubt, has been ultra-high-throughput sequencing, or next-generation sequencing, and it has completely rescued the field." For some scientists, it was a model machine because it favored the short sequences that are characteristic of DNA from ancient material: "And in particular, the Illumina technology works very well. It's almost as if, as [X] said to me one time, it was designed for ancient DNA because it sequences small molecules and sequences hundreds of millions of them at a time. So, this *completely* rescued the field" (21-00:10:15). This same scientist said, "PCR allowed ancient genes. NGS has allowed ancient genomes" (21-00:40:00). One way NGS was a game changer was the way it changed contamination concerns. With PCR, particular primers were used to amplify specific sections of interest. With NGS, however, all DNA available was captured and sequenced. For example, researchers who recovered the mammoth metagenome in 2006 sequenced 28 million base pairs of DNA of which 13 million base pairs matched the mammoth. In other words, half the data was mammoth DNA while the rest was environmental, bacterial, or unidentified DNA (Poinar et al. 2006, 393). With NGS, researchers could recover millions of sequences and estimate the percentages of endogenous and exogenous DNA by searching for signatures of molecular degradation or damage characteristic of authentically ancient DNA. This changed their contamination concerns. One scientist said, "So, now it's not only a question of having controls […]. You can actually look at your data and determine whether you have a contamination problem or not, right?" (7-00:17:30). A second said, "[…] I remember […] *fighting* at conferences [about] if these sequences [were] feasible or not; if it was contamination or not. […] This is not really an issue anymore because people have contaminations, but they calculate it away. [Laughs]." Now the issue was not data contamination but data production: "At the moment, we are not discussing the authenticity of the results much anymore – it's still there and probably always will be […] – but at the moment we are rather discussing the correct filters that you have to apply to your data set and how to handle these huge amounts of data. […]" (13-00:58:50). NGS did not remove the problem of contamination, but it did resolve it in a way that made it manageable.

Practitioners used NGS to probe several controversial studies whose results remained contested within the community. In the 1990s, several scientists had reported the recovery of *Yersinia pestis*, a bacteria suspected to be the cause of the Black Death. However, there were arguments over authenticity. Debates seriously started in 2000 when a team including Didier Raoult and Michel Drancourt from University of the Mediterranean in Marseille reported the recovery of *Yersinia pestis* from a plague pit in France and argued that this was evidence for *Yersinia pestis* as the cause of the Black Death (Raoult et al. 2000). But others were concerned about contamination. One scientist said, "[…]. They were doing it in a modern lab […]. They weren't an ancient DNA lab and this was the era of 'Do it Right or Not at All' – you're out of our club so you can't do this work" (27-02:14:45). In light of contamination concerns, another team challenged their conclusions. In 2004, a team including Gilbert and Cooper tried to extract and analyze *Yersinia pestis* from over a hundred samples from various plague pits across Europe, but they failed to replicate positive results (Gilbert et al. 2004). Consequently, a debate over who was right ensued: "So, you had Didier saying, 'We found it!' And then Tom would say, 'You didn't find it!' And then they said, 'We found it!' 'You didn't find it!' And there's probably ten years of publications going back and forth about this" (27-02:14:45). Meanwhile, another team, independent of either side of the debate, sampled the plague pits and used NGS to generate the genome of *Yersinia pestis*; the first genome of an ancient pathogen. The team, including Kirsten Bos and Henrik Poinar from McMaster University in Ontario, as well as Johannes Krause from University of Tübingen, found extensive evidence for *Yersinia pestis* and claimed it was indeed the cause of the Black Death (Bos et al. 2011). The study demonstrated the decisiveness of NGS as applied to the search for DNA from fossils: "Once they had the whole genome then there was no question, right? […] This was real. And then it completely ended the debate. It was just like, 'Bam!' And it dropped like a *bomb* on the community – like a *huge bomb*. [Laughs]. And it was also one of the earliest demonstrations of 'next-generation sequencing is going to completely change this game.' Like we're in a different era and it just shut that whole thing down" (27-02:14:45).

NGS partially solved one problem, contamination, but it also created another. The move from ancient genetics to ancient genomes created an influx of data that scientists had to learn how to analyze. An interviewee presented this perspective: "We were going to have all the genes […]. It didn't matter if it was the host, the bacteria, the dog who peed on the bone, or whatever! We were going to have all this stuff and the question was, 'Could we do the bioinformatics?'" (28-00:23:30). This required a new set of skills to analyze it all: "Processing is completely different because before I could still look at each sequence by eye and edit them by hand, but now we have […] billions of sequences and you have to do everything by bioinformatics. So, that has changed completely. […]" (15-00:35:30). With change came challenge as researchers sought the skills of mathematicians, statisticians, and bioinformaticians: "Nowadays, […] you get a couple of million sequences. You can't interrogate them manually or visually, and you have to have all that bioinformatics knowledge to be able to actually filter and map and work out the quality of all those sequences […]. It's the people who are going to analyze it all that are going to end up with the all work, and all the fame and fortune" (25-00:21:00). However, this transition ushered in uncertainty as the community, previously defined by and dependent on PCR, tried to adjust to a new theoretical and technological platform that required a new set of skills.

For many, this transition was vital for the future of the field because it marked maturation. However, some labs were lost in this move. One researcher remarked, "I think that when we were our own discipline in our own corner doing our own thing it was very hard to justify why we existed […]. Why spend so much money to do so much work to generate so little data that has such little value?" Further, "And I think this move, although it's painful and it means that some people have lost status or lost control, ultimately means that the results of ancient DNA research are much more relevant in the modern world […]" (27-01:17:00). The move from PCR to NGS was risky, but some labs made it successfully. Pääbo's lab in Leipzig and Willerslev's lab in Copenhagen were two that made this transformation early on. Geneticist David Reich at Harvard University, although a newcomer, was also becoming a powerful collaborator, even competitor, in the field. A recent *Nature* report called Reich a "big thinker" who "helped to turn ancient genomics from niche pursuit to industrial process" (Callaway 2015a). One scientist said, "What has happened is that some labs have struck way ahead because they've made that transition. You know who they are. They're Leipzig, they're Copenhagen, and Harvard. They're the big productive labs." However, not every lab could make the move: "It took us a few years, and we're a genetics department. Whereas if someone is in an anthropology or archaeology department, it's quite a different story. It's become, I think, impossible for somebody to transfer from an archaeology or anthropology discipline to this field" (21-00:10:15). Even labs that made the move felt the tension, especially in terms of expertise. In reference to the need for statistics and bioinformatics in the lab, one interviewee explained, "I still have questions and I still have context and I still have understanding, but in terms of day-to-day operation of what it is they do and how they go about doing it, I don't know the first fucking thing. If all of them got hit by a bus right now, I would be really stuck! [Laughs]." Further, "Whereas five years ago, my first PhD student, he was doing what I did, so I could teach him. I can't teach anyone now. I don't know what the hell I'm doing. […]" (22-01:25:00). There were also financial considerations that prevented labs from making the move: "[…] The kits are expensive, the primers are expensive, and it's all very new. […]. It was really scary to a lot of labs, and a lot of labs haven't made that transition because it's expensive and it involves the development of a completely new tool set" (27-00:38:00). The technological transition to NGS, while expensive and time-intensive, was a critical component in ancient DNA's disciplinary development. Its adoption by scientists, including the way they would decide to advertise its powerful potential as applied to fossils, attracted the media spotlight.

3.3.3 "A self-inflicted pressure"

In 2006, Pääbo announced that the Max Planck Institute for Evolutionary Anthropology, working with 454 Life Sciences Corporation, would sequence the Neanderthal genome in two years' time, and they would use NGS to do it (Green et al. 2006; Noonan et al. 2006;

"Neandertal Genome to Be Deciphered" 2006; Pääbo 2014).[36] Pääbo first introduced his preliminary plan to a private and professional audience at the Cold Spring Harbor Symposium in May 2006. At that time, Pääbo was working with Edward Rubin – US Department of Energy Joint Genome Institute and Lawrence Berkeley National Laboratory in California – and their labs together had sequenced nearly 1 million base pairs of Neanderthal DNA. But 1 million base pairs was far from what was needed for reconstructing the whole genome. Pääbo recalled that the sequences, at that time, represented only 0.0003 percent of the whole genome. Nonetheless, Pääbo confidently claimed that it could, and would, be done. But he was also aware of the pressure this placed on himself and his lab. Pääbo remarked, "Now I had really stuck my neck out, publicly promising to sequence the Neanderthal genome. If we succeeded, it would clearly be my biggest achievement to date; but if we failed, it would be a very public embarrassment, almost surely a career-ending one. And I knew that succeeding would not be as easy as I had made it sound in my talk" (Pääbo 2014a, 117). The Neanderthal Genome Project went public in July 2006 when the Max Planck Institute for Evolutionary Anthropology and 454 Life Sciences Corporation decided to host a press conference and press release broadcasting their plan ("Neandertal Genome to Be Deciphered" 2006). Pääbo recalled, "The press conference was an electrifying event. The room was full of journalists, and media from across the globe followed it via the Internet" (Pääbo 2014a, 124). Yet celebrity was followed by stress surrounding the work to be done in a short time span. One scientist said, "I really thought this was crazy" (12-00:13:20). Not only was the project a complex challenge, but the publicity around it produced extreme pressure. This same scientist said, "The pressure we had […] was a self-inflicted pressure that Svante had created by announcing that we would publish the genome in two-and-a-half years or something crazy. […] [W]e didn't even have the material to do it, which of course he was basing this idea on improvement […]" (12-00:18:45). This research required more money, more machines, and more fossils with Neanderthal DNA. There was a way to go before a draft genome, much less a whole genome, could be sequenced.

The Neanderthal Genome Project was not necessarily an isolated idea. It rode the wave of research, like the Human Genome Project, interested in generating whole genomes of modern and ancient organisms for the first time (Schmutz et al. 2004). The Neanderthal Genome Project was packaged and pitched within this context, with an awareness of its

[36] Svante Pääbo detailed the development of the Neanderthal Genome Project from a personal perspective. See Pääbo (2014a).

scientific significance, as well as its news value. It also came from a decade of scientific, conceptual, and technological developments seeking to study the evolution and extinction of Neanderthals through genetics (Krings et al. 1997; Ovchinnikov et al. 2000; Höss 2000; Krause et al. 2007a; Krause et al. 2007b; Hofreiter 2008). It started in the 1980s with Wilson and his hope to one day use DNA to answer one of the biggest questions in evolutionary history; the relationship between Neanderthals and modern humans (Schmeck Jr. 1985). In 1997, that hope appeared within reach when scientists first sequenced Neanderthal mtDNA (Krings et al. 1997). In 1997, the recovery of Neanderthal mtDNA had provided no evidence for a genetic contribution from Neanderthals to modern humans, but mtDNA alone could not definitively show that there was no contribution; they needed genomic data and lots of it. In 2006, separate studies reported the recovery of tens to thousands of Neanderthal nucleotides from a 38,000-year-old Neanderthal bone found in a cave in Croatia. One paper, published in *Science*, was by James Noonan working with Rubin and colleagues from US Department of Energy Joint Genome Institute, Lawrence Berkeley National Laboratory, and the University of Chicago. They reported the recovery of 36,000 base pairs of Neanderthal DNA (Noonan et al. 2006). The other paper, published in *Nature*, was by Richard Green working with Pääbo and colleagues at Max Planck Institute of Evolutionary Anthropology and 454 Life Sciences Corporation. They reported the recovery of 750,000 base pairs (Green et al. 2006).

Both groups had extracted DNA from the same bone, but sequenced the DNA using different techniques and arrived at clearly different conclusions. Noonan and colleagues' data suggested there was no contribution of Neanderthal DNA to modern humans, while Green and colleagues' data suggested a significant amount of admixture between the two. One researcher recalled, "[T]he conclusions of the studies are pretty much completely opposite. One of them says there's no mixing with modern humans, one says there's a lot of mixing with modern humans. And the weird thing is they both analyzed the same bone. So, it wasn't even two different Neanderthals" (6-02:24:00). In 2007, a subsequent independent study reanalyzed both data sets in light of contrasting conclusions (Wall and Kim 2007). Based on their analyses, they argued that "something is wrong with the Green et al. data" and that the differences between data were likely due to "contamination" (Wall and Kim 2007, 1865). Indeed, Pääbo and his lab had worried about contamination before publication, but they determined the likelihood was low so published the research results anyway (Pääbo, 2006; Pääbo 2014a).[37] Pääbo and Rubin had started this study in collaboration, but they disagreed over which techniques, direct or indirect sequencing, would be best. Their disagreement was so severe that their partnership came to an end, resulting in the publication of two separate papers rather than one. The labs, once collaborators, were now competitors. According to Pääbo, the race to sequence the genome was on (Pääbo 2014a).

In 2010, four years after their announcement, Leipzig finally finished the Neanderthal Genome Project (Green et al. 2010). The project, conducted by over fifty scientists at a cost of approximately €5 million, successfully sequenced 4 billion base pairs of Neanderthal DNA ("The Neandertal in Us" 2010; Callaway 2010; Pääbo 2014a). Scientists, for the first time, had data to answer their questions about Neanderthal evolutionary history, specifically their relationship to humans. In this study, Pääbo started to work with David Reich, a population geneticist from Harvard University, and it was the combination of this genomic data and statistical methods developed by Reich and his lab that allowed them to detect signals of admixture between humans and Neanderthals. It was not just the data that was important, but the ability to analyze it was a critical component of the project. From this information scientists inferred that Neanderthals interbred with humans before their extinction 30,000 years ago. These research results highlighted the power of molecular data to answer questions about human evolutionary history. One researcher remarked, "For decades, people have argued whether there had been gene flow between modern humans and Neanderthals, and there was basically no answer to it because how would we determine it? And now with genomic data sets, you can actually simply analyze the data sets and see if there is evidence for it or not. So, it seems to be evidence for it. […]" (15-00:35:30). However, the evidence seemed to suggest that Neanderthals only interbred with a particular human population, those humans who had traveled out of Africa and into Europe. By comparing the Neanderthal genome with present-day human genomes, they determined that Neanderthals shared more similarities with present-day non-African populations than with present-day African populations. Neanderthal DNA existed in a small percentage (one to four percent) of a specific population (Eurasian population). In "Neanderthals, Humans Interbred – First Solid DNA Evidence," *National Geographic* reported, "The next time you're tempted to call someone

[37] In the community, there is considerable controversy over these publications and their results. According to interviewees, one reason there is controversy in the community is because Pääbo, a symbol of conservatism regarding contamination, appears to have submitted and published research results with knowledge of contamination, or knowledge of possible contamination.

a Neanderthal, you might want to take a look in the mirror. According to a new DNA study, most humans have a little Neanderthal in them – at least 1 to 4 percent of a person's genetic makeup" (Than 2010). While Pääbo expected to engage the archeological and anthropological community, it seemed he had not anticipated how the public would react. Their paper attracted attention from the creationist religious community in the US who reinterpreted results as evidence for or against their own private projections about Neanderthals' relations to humans and creation. Women also wrote to Pääbo with their speculations that their own husbands were Neanderthals. Playboy even spotlighted the science in a four-page piece titled "Neanderthal Love: Would You Sleep with This Woman?" (Pääbo 2014a, 221–222). The celebrity spotlight on the technoscience was far from lost.

The Neanderthal Genome Project was an amazing achievement, but it was not the first ancient genome nor the most ancient genome to be sequenced using NGS. Nor was it the only one to attract press attention. Over the past decade, the Centre for GeoGenetics at University of Copenhagen in Denmark, a lab led by Willerslev, had become an influential institution in the search for DNA from fossils. In 2010, just prior to publication of the Neanderthal genome, Willerslev and colleagues extracted DNA from a 4,000-year-old Paleo-Eskimo and sequenced the first ancient human genome from it (Rasmussen et al. 2010). In 2013, Ludovic Orlando, also at the Centre for GeoGenetics, and colleagues extracted DNA from a 700,000-year-old permafrost-preserved horse bone in Alaska and successfully sequenced its genome making it the oldest one to date (Orlando et al. 2013). This specific study offered an opportunity to reflect on how far the field had come since the 1980s. The practice of recovering DNA from ancient and extinct organisms, now recognized world-wide, had first attracted attention at Berkley in 1984 when Wilson and Higuchi extracted just 229 base pairs of mtDNA from a 140-year-old quagga. Now, three decades later, over 12 billion base pairs from a 700,000-year-old horse had been extracted, sequenced, and analyzed. This study pushed the preservation of DNA from fossils to the extreme. It broke the 100,000-year-old threshold that had defined the limits of the discipline. The press in particular highlighted this achievement (Draxler 2013; Lee 2013). *Wired* wrote, "700,000-Year-Old Horse Genome Shatters Record for Sequencing of Ancient DNA" (Hansen 2013). The press also speculated about the implications of reaching farther back into the prehistoric past. *The Guardian* heralded the headline, "Prehistoric DNA sequencing: Jurassic Park was not so wide of the mark," writing, "It is an extraordinary achievement, one that immediately raises the prospect that scientists might soon create the genomes of creatures that died more than a million years ago, possibly several million years. By that reckoning, Crichton and Spielberg would not seem to be so far out" (Mckie 2013).

Although journalists highlighted the genome's ancient age as a breakthrough, scientists on the study argued that the age was not the reason behind their research: "[T]he age is not the goal" (8-01:11:30). For scientists, the research's relevance was in its technological and conceptual developments. To generate the genome, they used Helicos sequencing which is a particular NGS platform that identifies, then amplifies specific DNA sequences. The ancient genetic data when compared to modern genetic data of horses provided evolutionary evidence that the *Equus* lineage – the lineage that includes extant horses, zebras and donkeys – actually arose about 4–4.5 million years ago. For researchers, these achievements, not the age, were what was significant. One scientist said, "I don't think we were really pushy in terms of the record. Of course, *Nature* made *all* the titles about it. […] [I]n the media interviews we played the card, of course, because it's just an easy thing to do. […] 'Time barrier is broken […].' But it was not the principle motivation." While it was pragmatic for this scientist to engage in the news value of the oldest genome, it was the replication of results, not holding the record, that was key: "[…] I'm just expecting someone else to break it again. […] And I want that to be broken simply to show *not* that we can go even like farther back in time but just to show the generality of it" (8-01:12:00). In fact, Leipzig came close to breaking the barrier when Jesse Dabney, Matthias Meyer, and colleagues published the mtDNA genome of 300,000-year-old cave bear and a 400,000-year-old hominin found in a cave in Spain (Wood et al. 2013; Dabney et al. 2013). These findings were specifically surprising because Spain, a hot climate as opposed to the cold conditions of Alaska, was assumed to be a unlikely environment for DNA preservation. In reflecting on this research, this scientist said, "[I]t's a dream come true because it shows that you're not just convinced about your work, but it's actually totally independent people – potentially *completely* competitors […]. (8-01:12:00). Even in the midst of competition, collaboration in terms of replication was vital to the future of the field. For the technoscience, their legitimacy resided not only in the recovery of ancient genomes, but in their ability to replicate and reanalyze those results. NGS helped researchers reconcile the field's past with its present in light of previous replication problems. However, these studies also showed that practitioners could play to both press and public expectations of news value. Indeed, life in the spotlight was not necessarily at the expense of research relevance or excellence.

3.3.4 “The first to do something”

NGS offered scientists opportunities to reinvent their research by taking it in new directions, or at least directions that although previously possible were challenging within the theoretical and technical constraints of PCR. NGS changed this by increasing the scale and scope of research from phylogenetic to population genetic studies, while decreasing contamination concerns and cutting sequencing costs (Hofreiter et al. 2015; Culotta 2015; Pennisi 2015a; Pennisi 2015b; Gibbons 2015; Service 2015). Since the late 1980s and early 1990s, the technoscience had been developed on and defined by PCR. Scientists had built boundaries around the technoscience on the basis of this technology. However, NGS introduced excitement, experimentation, and uncertainty as researchers were required to test its potential, as well as its problems. Today, practitioners are in the midst of this technological transition from PCR to NGS. With reference to the technoscience, one scientist said, “[…] It has recently exploded, and we are in the midst of this explosion” (12-00:29:15). A second said, “It’s just been a technology roller-coaster. The technology is just mind-blowing” (9-01:50:30). NGS placed practitioners in a new era of exploration. In looking forward to the future, one student imagined new possibilities of retrieving DNA from “more extreme samples” or “more extreme sites” that are “older or warmer” (GI-4-00:25:00). But as the scale and scope of research increased, so did the speed in which the discipline developed. One interviewee presented this perspective: “[…] [I]t’s quite interesting to see how fast the field is changing, but it also means that you have to reinvent yourself and your research over and over again” (13-00:11:00). In this era of exploration, researchers worked towards new technological and conceptual developments.

A number of researchers turned to methodology and other theoretical or technological advancements. For example, some tried to better understand DNA depurination and degradation, and even tried techniques to repair DNA damage (Green et al. 2006; Briggs et al. 2007; Briggs et al. 2009; Briggs et al. 2010; Jónsson et al. 2013). With a better understanding of the biochemical processes that contributed to DNA decay, others developed programs to search for patterns of decay or signatures of damage (Gilbert et al. 2003a; Gilbert et al. 2003b; Ginolhac et al. 2011). Several tried enhancing extraction techniques in the lab (Dabney et al. 2013; Schubert et al. 2014; Skoglund et al. 2014; Haak et al. 2015; Seguin-Orlando et al. 2015). These advancements were important to the field, but scientists still relied on finding good fossils with DNA. According to some scientists, dental calculus on teeth could be a rich reservoir for information about diet and disease (Jin and Yip 2002; Hardy et al. 2009; Henry, Brooks, and Piperno 2011). For example, Christina Warinner, University of Oklahoma and Max Planck Institute for the Science of Human History in Jena, and researchers recovered the first metagenomic and metaproteomic data for the ancient human microbiome; data that could inform the evolution of human diet and disease, particularly with major transitions from hunter-gatherers to farmers to industrialized societies (Warinner et al. 2014a; Warinner et al. 2014b). Researchers also argued that the petrous bone, the inner ear bone, was often optimal for preserving endogenous DNA. The team, including Cristina Gamba and Daniel Bradley at Trinity College Dublin, extracted and sequenced DNA from the petrous of several human samples from Hungarian, Neolithic, Copper, Bronze, and Iron Age burials and used it to study 5,000 years of human evolutionary history in Europe (Gamba et al. 2014).

Yet the technoscience was partly a race to be first in the field to generate whole genomes of ancient and extinct species. With NGS, researchers rushed to sequence the first genomic data from mammoths, plants, and the plague to Paleo-Eskimos, Aboriginal Australians, and famous figures like King Richard III (Gilbert et al. 2008a; Gilbert et al. 2008b; Miller et al. 2008; Rasmussen et al. 2010; Rasmussen et al. 2011; Bos et al. 2011; Pedersen et al. 2014; Rasmussen et al. 2014; Willerslev et al. 2014; King et al. 2014). Using NGS, scientists sequenced the first genomic data from the Neanderthal (Green et al. 2010). They also sequenced the first genomic data from a Denisovan, an early but extinct hominin that until recently had never been known before (Krause et al. 2010; Reich et al. 2010; Gibbons 2012; Gokhman et al. 2014). While these studies grabbed headlines, many also made considerable conceptual contributions to the study of evolutionary history. For example, several studies, like the Neanderthal and Denisovan papers, revolutionized human research and our understanding of human origins, evolution, and migrations (Stoneking and Krause 2011; Veeramah and Hammer 2014). Other research shed light on early human evolution in regards to lactase persistence and the origins of Mesolithic and Neolithic hunter-gatherers and farmers (Izagirre and de la Rúa 1999; Haak et al. 2005; Burger et al. 2007; Bramanti et al. 2009; Haak et al. 2010; Skoglund et al. 2012; Warinner et al. 2014; Jones et al. 2015; Malmström et al. 2015), while a series of studies explored pig, cattle, and dog domestication on a global scale (Leonard et al. 2002; Bollongino et al. 2006; Larson et al. 2007; Scheu et al. 2008; Larson et al. 2012; Larson et al. 2014; Skoglund et al. 2015). Together, these technological and conceptual developments helped renew ancient DNA's legitimacy among professional and public audiences.

Of these developments, however, it was the search for ancient genomes that hit media headlines and was frequently featured in journals such as *Nature* and *Science*. NGS and the race to be first in the field placed the practice in the celebrity spotlight. One interviewee offered this opinion: "I don't think it's unique to ancient DNA, but I think it's particularly prevalent in ancient DNA. […] I think part of that is because you know the media is going to like it when the first blah-blah-blah genome comes out. The media is going to jump on that. They think it's fantastic […]. And that also means that the journals think it's fantastic, the big journals: *Science*, *Nature*, […], and stuff. They tend to favor publishing things that they know the media is going to go bonanza about." Further, researchers recognized the benefits of aligning their work with this sort of publication and press: "So, therefore you know that if you do the first blah-blah genome, it's quite possible that it will end up in *Nature* and *Science* […]. Some people care about that because they want to be famous and some people care about that because if you happen to get one or two big papers you're likely to be able to secure big grants. It's more of a pragmatic thing" (38-00:58:00). Whatever the reason, researchers sought research with news value. They were active in their choice to test the technology on specific samples that would be likely to attract media attention, and by extension, likely to secure a prestigious publication in a journal like *Nature* or *Science*. Researchers capitalized on this.

The enthusiasm for NGS was reminiscent of the excitement for PCR in the early 1990s. Specifically, the race for the first or oldest genomes was reminiscent of the race for the first or oldest DNA from ancient and extinct organisms. One interviewee offered this opinion: "I think a lot of the whole genome stuff, at the moment, is just being driven by, you know, 'We're the first person to sequence the genome of extinct species X.' And it's almost like the very early days of ancient DNA when you could get a *Nature* paper by saying, 'Ancient DNA recovered from extinct thylacine or quagga or Egyptian mummy or mammoth or whatever.'" This scientist said, "It didn't really matter what the answer was. It was just the fact that you could do it. And I think that's possibly what's driving a lot of the ancient DNA community at the moment – is just again being the first to do something, not necessarily […] answering an intelligent question. […]" (25-00:23:30). However, this sense of spectacle was not necessarily superficial. Like PCR, NGS ushered in an era of exploration. Another interviewee presented this perspective: "[…] [T]his research discipline has developed the way that all science – new scientific disciplines – develop, in that you have an initial, wonderful discovery, you have lots of hype and high expectations, and then you come down to it with a bump, and then you do the hard work of working out what it all means and what you can really do; what is realistic and what isn't. And that may take the next ten to twenty years of that research discipline." This interviewee explained that the current community sits somewhere in the middle of this cycle: "[…] I think with these next-generation sequencing techniques we have to do it all again; come down to it with a bump, and sort out what we can and can't do. So, I think it's cyclical" (5-01:52:50). Over the past three decades, the technoscience had evolved into what some saw, despite this exploratory or experimental phase, as a more established practice in evolutionary biology. While there is certainly continuity regarding the interplay between science and the media from the PCR to the NGS era, namely scientists' need for the press to maintain momentum to continue to be competitive in this field, there does seem to be a distinct difference. After a short but sensational history, scientists are aware of and accustomed to the media limelight. Today, they appear to intentionally cultivate and control the spotlight in order to pursue, then promote, their research.

In November 2013, after a thirty-year history of technical transitions in the media limelight, and for the first time since the schism in the early 2000s, three scientists – Hagelberg, Hofreiter, and Christine Keyser – organized an occasion for everyone to gather together ("Ancient DNA: The First Three Decades" 2013; "Ancient DNA Applications in Human Evolutionary History" 2013). From the first quagga and mummy study in the mid 1980s, the technoscience had exploded into an era of genetics, genomics, and population genetics. Now was a chance to reflect on past and present research. Hosted at the Royal Society in London, the conference – "Ancient DNA: the first three decades" – consisted of over thirty talks over four days; the first half were open to the press and public, while the second half were privately presented at the Royal Society at Chicheley Hall. The meeting also resulted in a *Theme Issue* in the *Royal Society Philosophical Transactions* featuring eighteen publications on the search for DNA from fossils in evolutionary biology (Hagelberg, Hofreiter, and Keyser 2015). The meeting and the issue itself was more than a celebratory act (Smocovitis 1999). Rather, commemoration functioned as a way for scientists to reflect on and reinforce ancient DNA's place within evolutionary biology. However, while the meeting showcased the progress practitioners had made, it highlighted the fact that the practice was still in a state of technoscientific tension. For several scientists, the selection of speakers demonstrated the diversity of the discipline but not the research that some saw as the forefront of the field: "[…] [I]n a way it was a better representation of the field, but it was not the representation of best researchers in the field itself" (15-01:08:00). Another added, "[…] It was a bit surrealistic – almost. You had a bunch of people at the really cutting-edge of stuff […]. And then there was also these other studies […] that in terms of time were only a few years behind the curve, but right now a few years behind the curve is *ages*. So, it was a bit surreal in that sense because it perhaps didn't reflect […] what fantastic progress had been made. […]" (38-00:25:30). While the meeting seemed to suggest maturation, it was also evidence that the discipline was at a crossroads. With NGS, the search for ancient DNA appeared to have evolved from an emergent to a more established practice, but one in the midst of not only a technological transition but a fundamental shift in skills. NGS offered unprecedented opportunities for practitioners to incorporate ancient genomic data into population studies, something they had only hoped for in the past. NGS also allowed researchers to reconsider the idea of bringing back extinct organisms, something that had fascinated the press and public since the start.

In 2013, the National Geographic Society in Washington, D.C. also hosted a milestone meeting. This meeting, TEDxDeExtinction, was the first public forum where professionals from disparate disciplines gathered together in a serious setting to talk about de-extinction, the idea of bringing back extinct species (Macintyre 2013). The event was co-hosted by National Geographic Society, TED, and Revive & Restore, a non-profit founded by Stewart Brand and Ryan Phelan for studying biodiversity and reviving endangered or extinct species. The idea of resurrection had continued to inspire popular interest, but several scientists were interested in it, too. For example, Michael Archer at the Australian Museum had made it his mission to bring back the thylacine. In 2000, he announced a twenty-year trajectory for resurrecting the thylacine using DNA from a pickled thylacine pup (Colgan and Archer 2000). However, the project came to a close for a range of reasons (Fletcher 2008; Fletcher 2010). Since then, the thylacine genome had been sequenced and scientists are exploring the prospect of bringing it back, but Archer, while publicly promoting its potential, has yet to pick up the project again (Miller et al. 2009). Instead, Archer has turned to the Lazarus Project in an attempt to revive the Australian gastric brooding frog; a species of frog famous for giving birth through its mouth (Archer 2013). This and several other studies formed the foundation for TEDxDeExtinction, a one-day occasion featuring over twenty talks focused on bringing back extinct organisms.

Resurrection, once mere speculation, appeared to be a possibility in light of scientific and technological advancements. TEDxDeExtinction was a place where professionals could discuss the scientific, technological, political, and ethical implications of bringing back extinct species for the first time ("TEDxDeExtinction" 2013; Zimmer 2013). The meeting was an interface of individuals from paleontology and genetics to conservation biology, ecology, and synthetic biology. And some scientists from the ancient DNA community were present, too. Hendrik Poinar, for example, as well as Shapiro at University of California, Santa Cruz, both spoke on the possibilities but difficulties of mammoth de-extinction (Poinar 2013; Shapiro 2013). Ben Novak, a former student of Hendrik Poinar's and current collaborator with Shapiro, presented his project, sponsored by Revive & Restore, to resurrect the passenger pigeon (Novak 2013). However, for most of ancient DNA's history, the ancient DNA community had not been involved in real research on de-extinction. They engaged press and public interest in it, but did not pursue it themselves. TEDxDeExtinction and National Geographic's involvement changed this.

A year before TEDxDeExtinction, National Geographic held a private meeting for researchers to discuss the idea and implications of de-extinction ("De-Extinction Projects, Techniques, and Ethics" 2012). From the ancient DNA community, Hendrik Poinar, Hofreiter, Gilbert, and Schweitzer were asked to attend. For a few, they agreed to attend because the idea of de-extinction, for the first time since its suggestion in the 1980s, seemed feasible. Advancements in technology had made the idea increasingly imaginable, while some scientists with respected reputations had thrown their weight behind the idea, too. One researcher recalled, "The real reason I even agreed to go was that George Church was there. And George Church is what took it away from being crazy […] to credible – because, George Church, whenever he says anything, everyone listens because he really knows what he's talking about. […]" (6-02:00:00). For these ancient DNA researchers, de-extinction could no longer be dismissed as a dream.

Ancient DNA, since its start, was closely connected to the idea of resurrecting extinct species. The hunt for molecules from charismatic creatures like mammoths provided the backdrop for speculation about resurrection (Prager et al. 1980; Johnson, Olson, and Goodman 1985; Pääbo, Höss, and Vereshchagin 1994; Hagelberg et al. 1994; Cooper 2006; Poinar et al. 2006a; Krause et al. 2006b; Rogaev et al. 2006; Gilbert et al. 2007; Gilbert et al. 2008). These advancements in genetics and genomics, had some scientists claiming they would one day bring back the mammoth. In 2011, Japanese and Russian scientists announced they would bring back the mammoth by 2016 (Saenz 2011; McShane 2016). In 2012, Hwang Woo-Suk from South Korea emerged as a rival in the race to resurrect the mammoth (Woo 2012). These reports have yet to be realized, and for most ancient DNA researchers they appear to be more show than science. However, there is one particular project led by George Church, a geneticist at Harvard University, that has caused some scientists in the ancient DNA community to reconsider resurrection as a technoscientific possibility. Sponsored by Revive & Restore, the project proposes to resurrect and repopulate the woolly mammoth to its former habitat by tweaking the genomes of existing elephants to resemble that of its ancient ancestor ("Woolly Mammoth Revival" 2016). This project is made possible by a new technology, CRISPR/Cas9, that allows researchers to easily edit genomes by removing and replacing specific sequences of interest. In 2015, *Science* called CRISPR the "breakthrough" of the year (Travis 2015). After a thirty-year history, it seemed the search for DNA from fossils had come full circle, back to the idea of resurrecting extinct species. For some scientists, de-extinction was more than speculation. It appeared to be in a prime position to develop into a discipline in its own right.

3.3.5 Conclusion

In the early 2000s, practitioners struggled within PCR's theoretical and technical constraints, but NGS, according to some scientists, rescued the field from these limitations. Now the problem was not necessarily contamination. Instead, the challenge was how to handle all the data. NGS required a new infrastructure, as well as new skills in statistics and bioinformatics. Some labs made the technological transition. The Neanderthal Genome Project, for example, showcased the powerful potential of NGS as applied to fossils, capturing both professional and popular attention worldwide. Indeed, researchers involved in this effort purposefully played to media news values by hosting a press conference and press release announcing that their lab would sequence the Neanderthal genome in two years' time. Indeed, the technoscience as a whole flourished under the spotlight as researchers raced to sequence the first genomes of ancient and extinct organisms. Media was interested in these discoveries, and so were journals like *Nature* and *Science*. Researchers recognized this by playing into their expectations of news values. The enthusiasm was reminiscent of the excitement of the 1990s. Today, however, researchers seem skilled at cultivating and controlling the spotlight. "Ancient DNA: the first three decades" was a milestone moment as researchers reminisced on their past, present, and future. In some sense, it was a paradoxical moment, representing a tension in their transition from PCR to NGS. Now, ancient genetics was ancient genomics and with it came new potential. TEDxDeExtinction portrayed this potential. For the first time in its thirty-year history, the search for DNA from fossils came full circle with the quest to bring back extinct species as respected researchers like Church, Shapiro, and Hendrik Poinar lent credibility to its consideration, even if their stance was a critical one. In the midst of change, the boundary building that scientists had done to protect the practice from contamination concerns and its populist past began to define the discipline less. Researchers found themselves and the future of their field in a state of flux.

3.4 BOUNDARY BUILDING IN ANCIENT DNA

3.4.1 Introduction

By 2000, the technoscience's credibility was on the line and "Ancient DNA: Do it Right or Not at All" was a response to issues that some scientists felt threatened the future of the field. This paper and subsequent standards it promoted was an attempt to build boundaries around the practice in reaction to contamination concerns related to ancient DNA authenticity. However, boundary building happened on two fronts, in response to contamination and in response to celebrity. These boundaries were used to help structure, then specialize, the technoscience in its early exploration, and at a time when its future was far from certain. Replication, a traditional hallmark of scientific inquiry and legitimacy, was a feature that some scientists drew on to demarcate credible studies from less credible studies. Boundaries also functioned to control competition. Yet boundary-work extended beyond scientists' day-to-day activities. As interviewees retold their history, they reinforced these boundaries, especially as they tried to make sense of their past and present by drawing a line between the technoscience's emergent and more or less established status today. According to researchers, standardization was a part of stabilization. Some felt that with the transition from PCR to NGS, the practice had evolved from a technology and sample-driven activity to a more mature and question-driven one. At the same time, however, NGS ushered in a new era of exploration as scientists were naturally driven by the technologies' capabilities and the celebrity surrounding specific samples. Further, in this technological transition, researchers reconsidered their individual and institutional identity, and also reflected on ancient DNA's identity as a field, a technique, or a mix between the two. Interestingly, contamination concerns, despite the division they caused the community, were also a source of cohesion. But in the transition from PCR to NGS, some saw contamination as a lesser concern. Without this problem that defined the discipline for most of its history, some wondered what would hold the community together. Indeed, some predicted the discipline would die.

3.4.2 "It came from nothing"

Controversy around the technoscience was not about whether the search for DNA from fossils was science or non-science. Rather, the question was whether it could be a credible technoscience with research relevance within evolutionary biology. Authentication was the primary problem. Standardization of techniques was one way scientist tried to solve this problem. Here, researchers responded by building theoretical, technical, and physical boundaries around the practice. Cooper and Poinar's "Ancient DNA: Do it Right or Not at All" tried to do this. The presence or absence of an "Ancient DNA Lab" was used by some scientists to demarcate credible from less credible work. Indeed, it seemed they succeeded according to researchers' retellings of the history. The community split into self-subscribed "believers" and "non-believers" – their different conferences, collaborations, and places of publication are evidence of a division based on boundary building. While criteria were a response to contamination, boundary building was also a response to the discipline's populist past. As they tried to transform the technoscience from an emergent to an established practice, researchers felt its celebrity status posed problems for its acceptance as a serious science. According to several scientists, research that seemed to play to press and public interests distracted from what they thought was really relevant research. Researchers, like Pääbo and Cooper, used their political power in the field to demarcate their research from other work they thought less worth-while. These practitioners thought the technoscience, as a public-facing practice, required more than a response to issues concerning ancient DNA authenticity. The technoscience also required a response to the press and public interest that continued to influence it.

Thomas Gieryn, sociologist of science, called this boundary-work. Gieryn studied a series of credibility contests throughout history in which researchers employed boundary-work in order to establish their scientific authority over a particular domain or discipline (Gieryn 1983; Gieryn 1999). For Gieryn, boundary-work is a process by which scientists construct, deconstruct, or negotiate definitions of what counts as science: "Put bluntly, a sociological explanation for the cultural authority of science is itself 'boundary-work': the discursive attribution of selected qualities to scientists, scientific methods, and scientific claims for the purpose of drawing a rhetorical boundary between science and some less authoritative residual non-science" (Gieryn 1999, 4–5). Gieryn does not deny that there is something called science. Rather, he argues there are many sorts of sciences and different ways of drawing or redrawing the boundaries between them: "The boundaries of science have not, historically, been set in amber because – in the first instance – nature does not allow but one order of understanding, and therefore those serving up discrepant realities can draw discrepant cultural maps to legitimate their claims as uniquely credible and useful" (Gieryn

1999, 17). According to Gieryn, credibility contests are essentially contests for control and boundary-work is part of this process: “Boundary-work becomes a means of social control: as the borders get placed and policed, ‘scientists’ learn where they may not roam without transgressing the boundaries of legitimacy, and ‘science’ displays its ability to maintain monopoly over preferred norms of conduct” (Gieryn 1999, 16). However, authority can be threatened from more than one angle, requiring researchers to build boundaries on more than one front.

In the search for DNA from fossils, researchers built boundaries on two fronts, in response to contamination concerns and in response to celebrity. Gieryn called this double boundary-work, and cited the case of John Tyndall at the Royal Institution as an excellent example of it (Gieryn 1983; Gieryn 1999). In nineteenth-century England, science competed with religion and mechanics for public patronage and funding. Tyndall, a physicist and public-facing practitioner, wanted to increase science’s influence. He did this by consciously constructing a special space for it within the current culture by differentiating it from religion or mechanics: “Achievement of such a cultural niche required a representation in the form of boundary-work, in which audiences (real or potential consumers) learned not simply what science is, but why and how science is not-religion and not-mechanics” (Gieryn 1999, 62–63). Tyndall built boundaries on two fronts, in response to religion and in response to mechanics: “In distinctive ways, both religion and mechanics competed with Victorian science for cultural authority and for occupational resources. Yet the set for articulating the boundary between science and religion would not be effective for articulating the boundary between science and mechanics, and (of course) vice versa” (Gieryn 1999, 63). Consequently, Tyndall’s science had to be flexible enough to work in both contexts: “Tyndall selected from different characteristics of ‘science’ to build each boundary: scientific knowledge is empirical when contrasted with the metaphysics of religion, but it is theoretically abstract when contrasted with the commonsense, hands-on observations of mechanicians; science is justified by its practical utility when compared to the merely poetic functions of religion, but science is justified by its nobler uses as a source of pure culture and discipline when compared to engineering” (Gieryn 1999, 63). Crucially, this flexibility did not compromise authenticity: “The point is not that one or both representations are mere fictions: science is arguably all of these things (and much more). The point is rather to watch how features of scientific practice and knowledge are selectively deployed in a contingent contest for epistemic authority and resources among multiple makers of belief” (Gieryn 1999, 30). Gieryn suggested double boundary-work as a way to analyze the structuring and restructuring of science: “The simultaneous juxtaposition of science/religion and science/mechanics illustrates the flexible and not always consistent constructions of science that have nevertheless served the profession as an effective ideology justifying increased support of scientific research and education” (Gieryn 1999, 40). For a public-facing practice like the search for DNA from fossils, researchers encountered a similar situation. In their day-to-day activity, they used criteria to demarcate credible from less credible research. They used rhetoric to distinguish scientifically significant research from what they saw as mere speculation or spectacle.

For some scientists in this community at this time, replication and standardization of ancient DNA activity were hallmarks of stabilization. In reaction to contamination concerns, replication was one the main means by which practitioners judge the validity of claims. It was also the means by which they judged the legitimacy of ancient DNA activity within evolutionary biology. The extreme efforts of this community and their emphasis on reproducibility of ancient DNA analyses can be contextualized by drawing on other occurrences in the history of science. Sociologists Harry Collins and Trevor Pinch, for example, investigated the role of replication in scientific studies from parapsychology to the detection of gravitational waves and solar neutrinos (Collins 1985; Pinch 1986; Collins and Pinch 1993; Collins 2004). Using these case studies, they showcased why scientists rely on replication as a cornerstone for scientific inquiry and authority. Collins explained that for many scientists, sociologists, and philosophers, “reproducibility” corresponds to the “universality” of science: “Anybody, irrespective of who or what they are, in principle ought to be able to check for themselves through their own experiments that a scientific claim is valid” (Collins 1985, 19). In controversy, however, the “who,” “what,” and “how” of an experiment matters. These factors come into play when researchers are trying to judge the validity of experiments, particularly when those experiments are said to be evidence of controversial claims.

While Collins and Pinch showed how replication is relevant in scientific practice, they also argued that replication is far from straightforward. They showed how replication can be a serious source of discord within a community. Collins termed the trouble with replication the “experimenter’s regress” (Collins 1985). Collins argued that the “experimenter’s regress” is a “paradox which arises for those who want to use replication as a test of the truth of scientific knowledge claims” (Collins 1985, 2). Collins explained, “The problem is that, since experimentation is a matter of skillful practice, it can never be clear whether a second experiment has been done sufficiently well to count as a check on the result of a first. Some further test is needed to test the quality of the experiment – and so forth" (Collins 1985, 2). In summarizing this phenomenon as applied to their specific case studies, Collins and Pinch stated, "The problem with experiments is that they tell you nothing unless they are competently done, but in controversial science no-one can agree on a criterion of competence. Thus, in controversies, it is invariability the case that scientists disagree not only about results, but about the quality of each other's work. This is what stops experiments being decisive and gives rise to the regress" (Collins and Pinch 1993, 3). At this junction, replication as a certification of accuracy, is open to debate.

The issue of experimental effectiveness was the precise problem that researchers in search of DNA from fossils faced in the 1990s and even throughout the early 2000s. For some scientists, reproducibly guaranteed authenticity, but across the community, practitioners could not agree on the terms and techniques by which experimental expertise, and in addition repeatability, should be judged. The schism between the "believers" and "non-believers" is but one example in which groups of researchers disagreed on the extent of experimentation that should be taken to avoid or detect contamination. But there was even disagreement within the "critical camp" of scientists – the "non-believers" – when it came to replication (Chapter Two and Chapter Three). Stoneking argued that while independent replication by a different individual in a different lab is preferred, to make it a requirement for every study in every lab would be impractical. He argued it would "cause more problems than it would solve" because multiple independent replications would be too expensive, destructive, and restrictive. Stoneking said that "attention" to "precautions" and "multiple independent extractions from each sample" should "suffice" (Stoneking 1995, 1260). Cooper, on the other hand, disagreed: "Several ancient DNA 'triumphs' (Golenberg et al. 1990; Cano et al. 1993; Woodward et al. 1994) that have turned out to be embarrassingly unrepeatable, or contaminated, might have been prevented if independent verification had been sought prior to publication" (Cooper 1997, 1002). Later, some scientists issued a counter claim to criteria for authentication (Gilbert et al. 2005). They were not necessarily rejecting the criteria for authentication, but they were bringing attention to the fact that the criteria were imperfect and that dogmatic dedication to the criteria was problematic. The rules by which all should subscribe to were contested. This made it difficult, if not impossible, for practitioners to move towards some sort of consensus regarding credibility. Disagreement led to discord, and the issue of contamination became a means of controlling scientific competition.

A byproduct of boundary-work was that researchers, in their attempt to control contamination concerns and celebrity, also tried to control competition by making it more difficult for some to participate in the practice. One researcher remembered, "I entered the field at the height of skepticism. So the '80s were the high time of 'We can do anything! […]. We can do anything we want!' And I entered the field around the time Alan Cooper [and Hendrik Poinar] published the paper 'Ancient DNA: Do it Right or Not at All.' It was a really intimidating time. If you don't do it *exactly right* you're a failure for the field" (27-00:41:30). To be sure, contamination was an important issue. It jeopardized the technoscience's authenticity and authority within evolutionary biology. Indeed, standards were a reasonable, if not necessary, reaction. However, the way some scientists enforced these protocols had consequences for the community. As one scientist said, "There certainly was, in the '90s, a contamination issue. […] It was also a very convenient way, however, to beat other people. It was a nice stick to beat other people, and I saw [X] use that stick. Like [Y] said, it should have been titled, 'Do it With Me or Not at All.'" (22-00:49:50). A second said, "What it did seem to be – to some degree – was a means to control or limit access to the technique" (2-01:02:30). As a third researcher recalled, "[…] [F]or some time many people thought – and a few people inside the field tried to make people think – that ancient DNA was kind of something magic, and that only two or three labs in the world can do it. […] [A]ncient DNA is not magic. It's just careful science […]" (15-01:16:00). Research on DNA from ancient humans was particularly contentious. In 2010, the Leipzig lab published a paper with a method for distinguishing endogenous from contaminating DNA by identifying nucleotide fragmentation or misincorporations (Briggs et al. 2010). In reference to this research, one interviewee said, "They said, 'Oh, fuck. There [are] a lot of people doing this ancient human DNA, and somehow they get away with it [...].' So, then they invented this degradation pattern, which is a good hint that you are working with old DNA […]. But that was a *trick*. It was nothing else but a trick – a very useful approach – but it's a trick. And now they can say, 'Ah ha! Now that we have got this magic, *we* – and *only we* – can do ancient DNA of humans.'" For this interviewee, it was a way to control competition: "So, it was another way of defending resources. […] It was a last try to keep people out of the field" (14-01:29:00). For several scientists, criteria functioned to control access to and success in the field.

This form of boundary-work caused a schism in the community. This schism – between the "believers" and "non-believers" – was far from subtle. In an unpublished paper, initially intended as a chapter of a book that was also remains unpublished, two practitioners, Bernd Herrmann and Charles Greenblatt, described the field’s scientific and sociological structure: “The scientific community has not acted optimally in establishing a supporting and cooperative system, but has pretty early started in splitting up in schools of the ‘haves’ and the ‘have-nots’ instead. Whereas the ‘haves’ partially defined themselves more in terms of expert knowledge by self[-]allocation of scientific standards. Interestingly enough[,] basic epistemological standards of experimental design and falsification became tacitly a battlefield” (Herrmann and Greenblatt 2010, 2). One interviewee shared a similar sentiment: “[…] [I]t’s kind of been like a feudal system with lords battling each other – [laughs] – for thirty years trying to gain control [laughs] – setting forth on horses to destroy each other’s kingdoms. That would be my humorous version of it because that’s what it’s like sometimes. [Laughs]” (25-01:27:00). Boundaries, intended for disciplining the discipline, were more than a means to address concerns about contamination. For practitioners, these boundaries also functioned as a form of controlling competition, shaping ancient DNA’s disciplinary development, community culture, and even how scientists approached the writing of its history.

In the search for DNA from fossils, boundary-work is not only evident through this history of the technoscience, but it affects how scientists write their own histories and roles within it. For example, the first textbooks in the field, like Bernd Herrmann and Susanne Hummel’s textbook, cite several different studies than do research reviews written by Pääbo, Cooper, and students trained in their tradition (Herrmann and Hummel 1994; Hummel 2003; Donoghue et al. 2004; Pääbo et al. 2004; Willerslev and Cooper 2005; Stone et al. 2009; Shapiro and Hofreiter 2012). Some “non-believers” – like Pääbo and Copper – disregarded research by the “believers.” One scientist said, “I must admit I don’t even read these reviews. I think we would certainly have a feeling that there is a kind of body of work that we simply don’t believe, in the past, and that we don’t cite rather than spending a lot of time saying we don’t believe it” (36-00:50:00). Willerslev and Cooper made a similar statement in one of their research reviews: “Perhaps unsurprisingly, many of the most extravagant aDNA reports have since been either disproved or effectively disregarded. [...]. Many other claims remain in limbo, where a lack of appropriate methods or replication renders them effectively meaningless [...]” (Willerslev and Cooper 2005, 3). In certain cases, practitioners constructed their own history by dismissing other research they disagreed with but that was nonetheless a part of the history. Herrmann and Greenblatt argued that “different positions were not discussed in terms of scientific standards but were ignored rather by strategic behavior” like “citation cartels” and “self[-]referential structures" (Herrmann and Greenblatt 2010, 2). Another interviewee offered this opinion: "Ancient DNA has really been run by [X] and [Y] […] and [Z]. [Z's] role in steering things or pushing things in certain ways, criteria of authenticity, or 'Do it With Me or Not at All' school of ancient DNA […] – I find it very annoying because it's not a nice field to work in when everybody is saying, 'You can't!' […]" (11-01:29:15). This same scientist argued, "There is no 'one way' because science doesn't go the way 'one person' says it" (11-01:41:15). A second said, "[…] I have two ways to think about this history. Personally, […] I am so happy to have known this history of science that is completely crazy! And on the other hand, I could be very jealous […]. A lot of people know [X] and two or three other persons and they completely ignore the other ones" (48-01:58:00). Boundary-work through the presence or absence of certain citations, on both sides of the divide, was a way scientists tried to establish their version of history and place in it.

The divide between "believers" and "non-believers" was but one map of the community in terms of community conflict that researchers on both sides of the divide tried to draw. It was a divide characteristic of the time at the turn of the twenty-first century, and it is a divide that researchers recall when telling their memories of their history. While this schism was important then, as well as today in regards to the structuring of this history, the ancient DNA community was not a completely binary community. Gilbert and colleagues' response to Cooper, Poinar, and their tradition of "Do it Right or Not at All" was a counter claim that contested the criteria of ancient DNA authenticity. They argued that adherence to the criteria did not in every case guarantee authenticity. They argued that studies ought to be reviewed on a case-by-case basis with a flexibility that takes into account the uniqueness of each study. Contamination was a clear concern for this community, but individuals differed in the degree they employed methods to test for ancient DNA authenticity. The schism of "believers" and "non-believers" captures community conflict but does not encompass the entirety of this community culture, particularly in respect to the nuances of the relationships between individuals across time or space.

In its various forms, boundary-work was a critical component of disciplinary development (Gieryn 1983, 792). Scientists used it to help structure the technoscience in its early exploration and at a time when its future was far from certain. One interviewee presented this perspective: "The only thing I would say is in a sense unique about ancient DNA was that it came from nothing, as it were. Before those early experiments with Pääbo and his coworkers in the late '80s and into the '90s – before PCR – it was out of the question. How could you ever take a fossil and get anything out of it genetically? It was absolutely impossible. It came out of nowhere" (20-01:12:00). However, the technoscience did not come out of nowhere. Ancient DNA came from the interface of fields like paleontology, archeology, and paleobotany to molecular biology, microbiology, and genetics. But its development into a discipline in its own right was not inevitable. Indeed, practitioners had to make something from what seemed like nothing. It required a merging of various disciplinary values to answer questions about the investigation of DNA from fossils and its applications to a range of biological or historical questions. People interested in the technoscience brought different skills and different scientific or epistemic cultures to the table. For researchers, it may have seemed to come from nothing because they faced the task of building it from the ground up with consideration for, and conflict between, the different disciplinary scientific standards or expertise it attracted but also required. One interviewee noted this tension and explained it this way, "I think it's probably because there is a lack of traditions and culture" (14-01:19:15). Consequently, they had to create their own scientific and epistemic culture. Contamination became a defining part of that culture. In an interesting way, contamination divided the discipline, but at the same time, it provided a serious source of community cohesion for researchers on both sides of the schism. Celebrity played a part, too. Boundary-work on both fronts, in response to concerns about contamination and its populist past, helped scientists define their discipline. In other words, the very concerns that divided the community were the ones that defined it too, influencing the technoscience's identity and the individual identity of the researchers involved in it. This is why boundary building was so critical but so contested throughout ancient DNA's thirty-year history.

3.4.3 "The field of ancient DNA is dead"

Scientists built boundaries through more than their day-to-day activity. Indeed, their memories of their history are full of boundary-work. Reflecting on the thirty-year history, interviewees portrayed the practice in its early days as an answer looking for a question, rather than a question looking for an answer. As one researcher recalled, "It was kind of a technique, an answer looking for a question, as my old PhD supervisor used to put it" (2-01:03:00). For interviewees, there were several studies in which the answers seemed to supersede the questions. In reference to some scientists, one researcher remarked, "They may have a research question. Sometimes they've got it – the research question […]. But sometimes it's even *pre*-getting-a-research-question. It's like, 'Let's study these. Let's see if there's DNA in these fossils.' […]." This researcher shared this story as an example: "I remember one occasion when one of the well-known ancient DNA researchers said to me, 'What species should I study? Mammoths are being done. What species isn't being done?' […] I can remember two occasions when that happened. […] [I]t got to the point to where there would be PhD students and you could see the supervisor thinking, 'What species hasn't anyone done yet? Nobody's done musk ox. Ok. You do musk ox.' *Without a very clear question*. 'Do it. Do it. You can always come up with something interesting, you know.'" For this researcher, this seemed to be partly propelled by press and public interest in charismatic creatures that were also likely to led to high-impact publications: "[…] I've seen several examples of 'Let's blitz this species.' We give a PhD student this species. They collect fossils from all over […]. They do the DNA, they draw up trees, and *then* they start to ask questions […]. And then the supervisor is usually then looking for a high-impact angle […]. [Laughs]. It is a slightly odd way of doing science" (3-00:44:30). In reminiscing on the hunt for DNA from fossils, interviewees portrayed the practice as a technology and sample-driven, rather than a question-driven, sort of science.

With NGS, however, scientists seemed to think this had changed. In the introduction to the issue of "Ancient DNA: the first three decades," conference conveners argued that the field was no longer a curiosity but now a credible practice: "In the past, a large number of ancient DNA studies were either purely technical, or one-off historical puzzles but, as we can see from the contributions to this *Theme Issue*, this is no longer the case, and ancient DNA researchers are now addressing a growing number of important scientific questions" (Hagelberg, Hofreiter, and Keyser 2015). According to one interviewee, NGS freed researchers from PCR's constraints, allowing them to focus on the biological or historical questions rather than the technological limitations: "[…] [F]or the first time in history, I think we're not driven at all by the technology because the technology is permissive today. We are driven by the question we can answer with the technology. […] (Well, it's not really that yet, but it's *close* to it.) […]" (8-00:18:45). According to another interviewee, it was more than the technology-driven nature of the practice that had passed: "I think that we are question-driven rather than sample-driven." For this scientist, the early days were "sample-driven" but "now that all the low-hanging fruit have been picked it's more question-driven" (43-00:10:30). Both the technology and celebrity of the specific specimens had played a role in the technoscience's evolution, but in the minds of interviewees, being question-driven rather than technology or sample-driven was a mark of maturity.

This language of an "answer looking for a question," rather than a "question looking for an answer," was a way scientists tried to make sense of their professional past. This retrospective boundary-work by the later generation of researchers, and indeed by some scientists belonging to the earlier generation, deserves further analysis. This language can be considered as an extended episode of boundary-work. This language was a way to compose a narrative of their technoscience, intentionally or unintentionally, by drawing a line between its emergence and more or less established status today, its technology or sample-driven versus question-driven phases (Summerfield 2004). The sometimes derogatory or dismissive comments by some interviewees about earlier practitioners as mere as technicians chasing technology was an attempt to draw out their own achievements, thus drawing community distinctions between ancient DNA's past and present, and in the process aligning themselves within one scientific practice rather than another. According to interviewees, being question-driven rather than technology or sample-driven was a hallmark of scientific maturity.

Demarcation mattered because for scientists it signified growth and research relevance within evolutionary biology. In other words, scientific maturity signified legitimacy, and this mattered for researchers coming out of a thirty-year history of credibility contests. This was not unusual. In the philosophy of science, the demarcation debate has a long history of scholars discussing the more or less correct ways, even wrong ways, of doing science (Popper 1959; Kuhn 1962; Lakatos 1970). Both scientists and scholars have even debated that some sciences or ways of studying the sciences are more or less prestigious than others (Laudan 1981). Derek Turner, philosopher of paleontology, highlighted that in the physical sciences, researchers have drawn a line between two types of activity – theoretical work and day-to-day work of discovering, experimenting, or describing – and for some scientists, the theoretical work is the more prestigious of the two (Turner 2011, 8). This attitude in the physical sciences has spilled over into the life sciences, especially with debates about experimental versus historical science methodologies (Cleland 2001; Cleland 2002; Cleland 2011; Turner 2011; Turner 2014). According to Turner, as well as historian of science David Sepkoski, paleontologists in the 1970s tried to create a more prestigious position for themselves within evolutionary biology through a rereading of the fossil record in order to make theoretical contributions to understanding patterns or processes of evolution (Turner 2011; Sepkoski 2012). A similar story about prestige can be said of question-driven or hypothesis-driven work versus data-driven or exploratory research (Laudan 1981; Evelyn 2003; Leonelli 2012; Strasser 2012; Glass and Hall 2008;

Haufe 2013; Elliott et al. 2016; Leonelli 2016). Here, this extended episode of boundary-work through interviewees' memories figures into this broader background of historical and philosophical debates around the process of science.

However, the search for DNA from fossils, even in its early era as a technology and sample-driven sort of science, was in fact a question-driven one, too. Researchers were driven by questions regarding the theoretical preservation and potential extraction of DNA from ancient and extinct organisms (Chapter One). Further, answering these questions was no small feat. Throughout the 1990s, scientists faced extreme theoretical and technical challenges (Chapter Two). These questions were technical in nature as scientists sought to discover what was possible regarding the preservation and extraction of DNA from ancient skins, tissues, or bone. But for some studies, the questions also took on a biological bent. For example, the quagga study in 1984 was initiated as a theoretical and technical challenge, but the specimen was specifically selected in order to test a hypothesis about the evolutionary history of an extinct species (Chapter One Section 1.3.2) (Higuchi et al. 1984). Likewise, researchers working on the termite in amber study in 1992 selected this specimen to test hypotheses of insect evolution and extinction (Chapter Two Section 2.2.4) (DeSalle et al. 1992). One interviewee described the early days this way: "[…] Of course, there was a little bit of a biological question, but the biological question was very often secondary to the technical achievement" (15-00:44:00). In the early days, it was nearly necessary for the technical question to take precedence. The biological, archeological, or historical question could not be answered without the technical achievement. However, in interviewees' memories, drawing a line between the two helped define their disciplinary development. Some thought this shift from a technology to question-driven practice was evidence of a more mature practice.

While scientists were answering scientifically significant questions with NGS, the practice was still technology and sample driven. Indeed, according to several scientists, ancient DNA activity still seemed to be an answer looking for a question. With the shift from ancient genetics to ancient genomics, came a rush to test the technology on as many samples as possible: "I think whenever a new technology comes on board there's a lot of 'Ta-da!' Hey, we analyzed this stuff with this new technology.' And it's really driven by the labs that have access to the technology and the samples […]." For this scientist, this was a consequence of the technology: "I don't think that's necessarily unique to ancient DNA. If you look at genome sequencing, genome sequencing is very much, 'Ta-da! Here's a genome! Look at all these data.' And then they go off and ask questions" (30-01:27:50).

A recent report in *Nature* by Ewen Callaway specifically spotlighted this phenomenon (Callaway 2015b). In 2015, Morten Allentoft, Willerslev, and colleagues generated 101 genomes of humans from 700 AD to 3000 BC (Allentoft et al. 2015). Their goal was to test hypotheses about human evolution and migration during a time when new tools and traditions surfaced, then spread across Eurasia. While question driven, the project was also driven by the technology and samples. Researchers went over the top to generate more genomes than necessary. Callaway quoted Allentoft: "'We could have stopped at 80,' says Allentoft. But 'we thought, "Why the hell not? Let's go above 100." ' " (Callaway 2015b). With NGS, the issue was no longer too little data but rather too much data. On this particular point, Callaway quoted Greger Larson at Oxford University: "'It's an interesting time, because the technology is moving faster than our ability to ask questions of it,' says Larson, whose lab has also amassed around 4,000 samples from ancient dogs and wolves to chart the origins of domestic dogs. 'Let's just sequence everything and ask questions later.'" (Callaway 2015b). Another researcher remarked on a second but similar situation: "[…] We got some new genomes and it wasn't question driven anymore. We didn't have a look at those genomes because they were the key to a question, but [because] they were good samples and we could get whole genomes from it. […]" (13-00:42:00). Students were encouraged to adopt this approach, too: "[…] I knew I just needed phylogenetic markers, but I wanted to make them genome wide […]. So, my committee was like, 'Do next-generation sequencing.' So, I did. […] [W]e didn't know what to do with the data for like three years! […]. We had to develop […] a collaboration with bioinformaticians to learn how to take next-gen-data and get the phylogenomic data. […]" (GI-5-00:40:00). According to researchers, the availability of samples combined with the ability to sequence genomes superseded their aptitude to analyze the data. It sometimes superseded the question, too.

NGS propelled practitioners into an era of exploration as they searched for samples to test the technology on in pursuit of ancient genetic and genomic data. A consequence of this was the drive to be fast, in order to be first, and to ask questions of the data later: "People are going *over* the top because they can – just sequencing the living crap out of absolutely everything. So, we're in this kind of exploration phase again, where it's like, 'Grab as much data as you possibly can, hire a great bioinformaticist, and then start asking questions in the resulting data sets'" (22-01:18:00). The charisma or curiosity that surrounded some samples also continued to drive the discipline: "I think a lot of the time it is the kind of arms race to be the oldest or the weirdest or the most unusual – that it is a technique looking for something to do as opposed to always answering really important questions" (25-01:31:20). Yet both the technology and sample-driven nature of the practice made sense.

With NGS, the technology and sample-driven nature of ancient DNA activity was nearly necessary. One interviewee offered this opinion: "It kind of goes back to that adage like when you have a hammer everything looks like a nail. So, you have your technique and you just hit everything you can with it. I think that's true of all sciences" (27-01:29:45). NGS' potential power as applied to fossils needed to be demonstrated. The technology needed to be validated. PCR had faced a similar situation. In both cases, however, testing the technology required resources, like money. Reflecting on the field's past and even present, this same scientist said, "I think ancient DNA is changing now, but I think there was a long period where I think it could be characterized as an answer in search of a question – and it has to do with funding agencies and it has to do with publications. You need to somehow convince a granting agency to give you money to work on the problem and to go through that development phase." Further, "[…]. [T]hey would figure out what they could do and they would come up with potential reasons for why we might want to do it. They weren't really real and were never really the goal. [Laughs]. So, I think there is a sort of cat-and-mouse-game that's being played to get the funding to do the work you really want to do versus what you say you're going to do because it's fundable" (27-01:34:30). In an early or exploratory phase, it was pragmatic for practitioners to select species that appealed to the public, especially when funding came from the public. Some purposely played to the news value to fulfill media expectations, as well as their own research interests. The first studies to generate partial or whole genomes of ancient and extinct organisms were part of the process of validating new technologies and securing further research funding.

Further, the sample-driven nature of the technoscience, and the celebrity that surrounded specific samples, created competition. Speaking about the technoscience, one researcher recalled, "[…]. A colleague of mine said it was almost like Golem out of *Lord of the Rings*: you have this *precious*." According to this researcher "power resided with those who could persuade people with bones" (21-00:20:00). The necessity but rarity of fossils was a cause and consequence of community competition. As one scientist said, "[…] I know a couple of people who've said they've never come across anything quite as vicious and nasty as the ancient DNA field. […]" (22-00:33:00). When asked why competition was so severe, another added, "Oh, because it's a small niche area, I guess. There's only so many really big questions you can answer or you can tackle and there are – at least in the ancient DNA world – there are a lot of alpha-males kicking around who all want to basically not only own their territory but own the whole territory. […]" (25-00:28:15). Disputes often occurred as researchers raced for access to specific samples that held high news values: "[…] Actually, working in the ancient DNA field is often very difficult because there's so much competition over samples because the samples are very high-value – very sexy-type – items. There's actually a lot of competition in the field, a lot of bitchiness and fighting amongst individuals. […]" (32-00:37:20). One practitioner who entered the discipline at the height of the community competition in the late 1990s and early 2000s offered this opinion: "[…] [T]he generations before me – which I guess would be one to two generations of people before me – I always saw as being a very *nasty* community, […], extremely competitive, […] totally dominant. I think with my generation it has certainly improved" (7-00:18:15). The aggressiveness caused some scientists to leave the field, but those who stayed tried to change the community culture: "I remember that my generation […] had always talked about [how] we were really tired of […] the *aggressiveness* in the field, and from that perspective, I think we have had a different attitude. […] We're still competing with each other and sometimes we're collaborating with each other. […] [T]here's more collaboration between the third generation than there was between any of the other two generations" (7-00:22:00). Overall, competition for fossils, as well as other resources, influenced community culture. A generation of rising researchers who saw it decided to change it by building boundaries between past and present generations.

As the community expanded, the first generation of scientists in the 1980s and 1990s came into conflict with the second generation of scholars emerging at the turn of the century: "[…] [W]hat's happened is that the children have killed their parents. […] [W student] killed his supervisor who was […] [X supervisor]. So, [W student] killed [X supervisor]. And then [W's] students ganged up to kill [W]. And then you look at [Y supervisor], and [Z student] killed [Y supervisor]." This interviewee explained it as an "odd cycle of destruction" (9-01:17:00). For several students, their relationship with their supervisor was professionally or personally difficult. One practitioner who identified with the second generation said, "[…] [I]t's strange that the whole second generation is traumatized, somehow, by their PhD supervisor – or by someone. I mean, we're not suffering anymore but we – all of us – had a period in our life where we suffered" (14-00:37:30). This scientist said, "[T]hat's what ties us together" (14-00:36:30). Another made a similar statement: "[…] [I]f we think of that sort of generation then honestly I think the big change is that we are generally friends […]. […]. Of course, you can't be friends with the whole world, right? Talking about [W], [X], [Y], [Z], all those people – same generation – we get along pretty well together, maybe because we all had pretty weird supervisors. [Laughs]. So, yeah, that was a connection up front […]" (8-00:40:15). These researchers reacted by consciously creating a new atmosphere for themselves and their students: "[…] There are two reactions from psychology: when you're beaten, you beat back or you don't beat at all. So, I try not to beat at all […]. So, I try to treat my people better than my PhD supervisor treated me. […] So, I very consciously thought about the social structure and the relation between people in my group because of that" (14-00:38:15). Another added, "[…] It just taught me the value of relationships. […] The samples are hard to get, but you can get them. The money is hard to get, but you can get it. People though – as soon as you blow up relationships you have cut off access to money and samples and grants like you don't even know. […]" (22-00:42:00). These practitioners' reactions to and reflections on their predecessors is an instance of what historian of science Joe Cain termed "patricide": "In the context of using history to construct heritage, patricide is a systematic attempt to disconnect – to construct not relevance but *irrelevance*" (Cain 2009, 352–353). Their narratives of negation were systematic attempts to dissociate from the first generation, thus distinguishing themselves in order to create their own professional and personal identity within an already competitive community.

However, it was more than competition that complicated community culture. The interdisciplinary nature of the technoscience, formed from the interface of disciplines like paleontology and archeology to genetics, complicated how scientists saw their professional identity. When asked about professional identity, one researcher replied, "This is a very good question and hard to answer. I give different answers depending on who asks me" (12-00:20:45). Other comments were comparable: "I'm a population geneticist or an evolutionary biologist or someone who tries to study human history with DNA or I don't know. It depends on the audience I'm talking to how I describe myself differently" (48-00:03:00); "[…] I describe myself as a molecular evolutionist or a molecular archeologist. […] It depends on who I'm talking to and which project I'm working on at the time. […]" (32-00:36:00); "I'm a jack-of-all-trades and master of none, really – because I've done so many different things […]" (25-00:35:30); "I hate that question. Part of my problem is that I've moved around and haven't stuck with one thing. In the last fifteen plus years was more developmental biology" (24-00:57:15). Interdisciplinarity also affected the technoscience's institutional infrastructure. For example, one researcher with training in anthropology, genetics, ecology, and evolution, holds a professional position in anthropology but also teaches biology to science and social science undergraduates alike: “I feel firmly planted in both fields, really. Sometimes it makes me feel like I’m not up in either” (30-00:58:45). This is more than a personal problem. It is a professional problem for funding, particularly in the US. This researcher remarked, “NSF and the Biological Anthropology Subdivision and their cap for funding per year on a project is about $100,000 and that includes direct and indirect costs, so the *real* amount of money you get to spend is about $70,000. What that means is that you can either pay for the reagents to do the research or you can pay for postdocs to do the research, but you can’t pay for both. […]” (30-01:03:00). Despite the fact that the search for ancient DNA first attracted professional and popular attention in the US, governmental financial grants are low compared to Europe. A recent report in *Science* by Ann Gibbons presented this point: “While Europe forges ahead on a transformative technique, U.S. researchers struggle for funding” (Gibbons 2016). Gibbons pitted the problem on the field’s interdisciplinarity and the US’s inability to address it: “The interdisciplinary nature of the method is part of its power but also makes it prone to fall through the cracks in the U.S. system. And most human evolution research in the Unites States is considered social science, which has low priority” (Gibbons 2016, 1384).

Just as interviewees had difficulty defining their individual and institutional identity, they found it equally difficult to define the technoscience in which they worked. For example, some saw the search for DNA from fossils as a technique, others viewed it as a field, while a few found it to be a bit of both. In the introduction to the issue “Ancient DNA: the first three decades,” conference organizers commented on this, too. Despite its thirty-year history and recent theoretical or technological developments thanks to NGS, the technoscience felt like of a young science: “Despite these advances, ancient DNA research still has the feel of a young science. Some even doubt whether it is a field at all, or instead a collection of applications of molecular techniques to a variety of biological problems” (Hagelberg, Hofreiter, and Keyser 2015). But some interviewees argued it is a field: “I think it’s a field. It’s not a technique. You need lots of techniques in order to study it. It *is* a field of study” (23-02:20:00). Others argued it is a field but one dependent on technology: “It’s a field in itself, but it’s strongly dependent on the techniques. The techniques are shaping always the field, and constraining it, or expanding it. So, we will always be dependent on that” (49-01:20:00). A postdoctoral researcher said, “Ancient DNA – people develop methods to extract DNA and to preserve DNA or make it more efficient, cheaper. That’s a field in itself and it has its own questions. But then you can use that branch to ask lots of questions" (GI-3-00:11:30).

Others viewed the search for DNA from fossils as more of a technique, but said that if it is a field then it is at least an unusual one: "I don't think ancient DNA is a research field as such. I'm going to be bit – what's the word – provocative here. Or at least it's a very odd kind of research field. It's a technique. It's a technique." (3-00:46:45). This interviewee shared this story to explain this point: "[…] I can think of an ancient DNA researcher, but this person is by no means unique. (In fact, it's quite common to people in that field.) One minute they're working on the vegetational history of Siberia and the next minute they're working on what was the ethnicity of the first people to enter the New World, at the same time they're working on the phylogeny of camels." For this interviewee, this is distinctly different from other research agendas and approaches: "Now, you could not find that […] in this institution […]. There are people who devote their life to the phylogeny of algae. There are people who work on early mammal radiation. There are people who work on human evolution. This is because they've got some kind of drive because they're asking questions." But this point was quickly qualified: "I think the one thing that may be, might be said, in contradiction to what I've just said is that it *is* a specialist technique. You know, this […] leading researcher who is working in all those areas is doing that because they have a *great* deal of expertise in extracting DNA out of different sources. That in itself is a skill […]" (3-00:51:30).

These referrals to ancient DNA activity as a discipline dominated by the technology, as well as the structuring of this history by chronology and technology, requires further discussion. It is important to note that the structuring of this history by technology from chapter to chapter might give the impression of technological determinism. Crucially, technological determinism is not implied from structuring of the narrative in this way. While new technologies and techniques are critical to ancient DNA activity and the extent in which data can be made available and analyzed, this technoscience is much more than a user community of the machinery, be it PCR or NGS. Scientists, while drawing on developments in other fields, are active agents in adapting these innovations for their own purposes. They are quick to recognize and optimize the products of other fields to suit their search for DNA from ancient and extinct species. However, to be clear, these scientists, are also initiating innovations from within their specialty. Ancient DNA requires manipulation and management of data. For example, the nature of ancient DNA is not the same as that of modern DNA. Indeed, the extraction, sequencing, and analysis of degraded and damaged DNA requires a specialist skill set to understand the biochemistry of DNA damage in order to be able to correctly infer how differences between sequences relate differences among individuals and populations over time. Researchers are always adapting the technology of both PCR and NGS to problems that are unique to ancient samples and the sequences that can be recovered from them. Ancient DNA is not superficial and reducible to the technology that supports it.

With NGS, however, some scientists thought this specialized skill appeared to be becoming more mainstream. It seemed to be less of a field and more of a tool used across different fields: "I think one of the biggest changes that's happened with ancient DNA is that it's really not even a field. So, in the beginning, ancient DNA had its own thing, its own methods, it was all specialized […]. It was all it's *own separate world*." However, this changed as practitioners transitioned from PCR to NGS: "And *now* it's really just genomics – applied to ancient samples. We don't have to adapt our data sets or adapt our methods or adapt our statistics anymore. We can just use the same statistics that everyone else is using in the entire field of genomics […]. We can basically do anything that anyone else can do" (27-00:45:30). Ancient DNA, in the 1990s and early 2000s, was a discipline defined by criteria of authenticity. Indeed, practitioners developed these criteria around PCR and contamination concerns. But NGS lessened these concerns. Indeed, it made criteria almost, but certainly not entirely, irrelevant: "[…] 'Do it right or not at all' was 'Do it with me or not at all' […] – that day is gone. The voodoo is over and it's become technically now honest. You don't need a special lab now. You just need a separate lab. We still dress up in suits and all – just to say we did" (21-00:48:30). In the transition from PCR to NGS, scientists' boundaries were breaking down. As they did, some scientists predicted that the discipline would break down with it.

Interestingly, contamination, despite the division it caused the community, was a strong source of cohesion. As an emerging technoscience with multidisciplinary interests and interactions, scientists were united through common problems concerning the preservation, extraction, and sequencing of DNA from fossils (Chapter One). In the late 1980s and early 1990s, contamination was a source of community cohesion as researchers discussed it in newsletters and debated it at conferences (Chapter Two). In the 2000s, it became a subject of community conflict. But regardless of what side of the schism one was on, "believers" or "non-believers," criteria of authenticity defined the discipline and colored researchers' memories of their histories and contributed to the shaping of their identities (Chapter Three). Indeed, some argued that the technoscience was a method-based science defined around contamination concerns. Herrmann and Greenblatt remarked, "[…] [T]here is no common epistemological roof for aDNA research but only a methodological one. It appears to us that most of the scholars in the field are *not* really interested in methodological and epistemological developments but in getting their specific questions answered. But in trying to do so, they had to turn themselves into method developers first and thus met a couple of people who were working on the same limitations, drawbacks, and pitfalls as they did." Further, "This is the true background for the aDNA community, not a scientific program to solve a shared big question, and this may be one of our drawbacks" (Herrmann and Greenblatt 2010, 3). Contamination was a core component of ancient DNA's social structure.[38]

But with changing concerns regarding contamination, some scientists questioned what would hold the community together or whether it would unravel. For most of its history, researchers involved in ancient DNA activity had been focused on avoiding or detecting contamination. It was a shared struggle across the community. One scientist said "[t]here was definitely an ancient DNA community" defined by "[p]eople with the same sorts of struggles who were applying it to a number of different questions" (30-00:31:00). Reflecting on the emergence of ancient DNA research, another researcher remarked, "[…] [W]e're working in *completely* different areas and almost have no common ground of communication other than how difficult it was to get ancient DNA out of the sample and then whether PCR worked or not." However, as criteria of authenticity, specifically regarding contamination concerns, became less of an issue, the community started to shift in different directions. This researcher recalled, "After that we just completely diverged. It was very much a tool. This is one of Svante's comments, I think. It's a tool rather than a field per se, and there's a gray area between the two […]" (32-00:52:30). In this gray space, the technoscience's future was wide open but uncertain.

Some practitioners predicted that without contamination, the community would dissolve. One scientist said, "I think it's matured so much that there no longer is a community. [Laughs]. You've got all these people using this technique – technologies – to answer questions and it's matured to the point where people from all these different areas don't really need to talk to one another" (24-01:06:00). From Nottingham in 1991 where researchers first met to investigate the idea of DNA from fossils, this scientist thought there was no need for those same scientists to meet again. Herrmann and Greenblatt offered this opinion of the future of the field: "It might be sad, but our community faces

[38] Elsbeth Bösl makes an argument supporting this statement in her unpublished paper. See Bösl (2016).

the fate of all pioneering communities. To a certain extent in our opinion the task is done. What now comes is the routinization within the subjects, wherever aDNA technology is suitable and helpful in solving the problems in associated fields. The aDNA research has been described as an interdisciplinary science. Here an uncertain future awaits us" (Herrmann and Greenblatt 2010, 4). For one interviewee, the technoscience's future was to move mainstream as a tool used in other fields rather than existing as a field itself: "I think – this again is even more provocative – I think it's possible that ancient DNA as a field might actually, sort of, *evaporate*. Now, what I mean by that is not that people won't carry on doing ancient DNA research. On the contrary, I think it's going to grow and grow, but I think it's just going to be seen as a tool, […] by people working in what I would describe as 'real research' areas. [Laughs]" (3-00:46:45). Others, however, put the point more bluntly.

From the 1980s to today, ancient DNA activity developed into what at least appeared to be a discipline, but one interviewee, contemplating its future, declared the discipline would die. For this interviewee, its death had already arrived: "The death of ancient DNA has come about because no longer can you have a career as being someone who is good at getting ancient DNA out of old fossils. Now you have to actually understand that data […]" (9-00:59:00). For this interviewee, the technoscience was becoming more mainstream as a technological piece of the puzzle for answering biological, archeological, or historical questions: "The death is when you've got groups like David Reich's group or Svante's group starting to just have ancient DNA as part of their group. […] [S]uddenly, it's just part of population genetics. There is no longer a discipline. Ancient DNA was a discipline where you had to be specifically trained because levels of contamination were so high and the techniques were so poor that you needed specialist skills […]." As contamination became less of an issue, researchers turned to the expertise of statisticians and bioinformaticians: "[…] [Y]ou need to be someone whose head is in population genetics, and those people can now sweep up the ancient DNA data and interrogate it with modern data and do meaningful things with it. Consequently, the field of ancient DNA is dead" (9-00:49:40). Crucially, this did not indicate its failure. Rather, some saw its death as its success: "[…] It's not a death because it's done. It's because [it's] […] taken over. It moves into mainstream" (9-01:26:45). Another added, "The end of the ancient DNA society, as such, was because of its own success" (28-00:03:00). Here, new opportunities, as well as new obstacles, were ahead.

According to scientists searching for ancient genomes, population genetics seemed to be the new name of the game. One interviewee put it this way: "I've been waiting for the field to die out for twelve years now. I always considered ancient DNA to be mature when we wouldn't need an ancient DNA community anymore, but we would have the normal communities – ecology, evolution, archeology, whatever – and this would be applied as tools." This researcher quickly qualified this, explaining there was a need for a community, just a different type of community: "[…] [S]ince we're having a technical development, since we're having next-generation sequencing, since we're now working with analytical methods and software connected to next-generation sequencing, we're still having specific, or at least semi-specific, development in ancient DNA which is still to some extent legalizing an ancient DNA community" (46-00:25:00). Expertise in mathematics, statistics, and bioinformatics was one way forward. As one scientist said: "[…] So, the future belongs to the geeks, not the Greeks, I suppose. [Laughs]. It really does" (21-00:20:00). This new skill set would require researchers, particularly archeologists, to not necessarily become population geneticists in their own right but to understand what population geneticists are capable of, as well as their limitations, when reconstructing past population origins, migrations, and evolution. Matthew Collins, molecular archeologist at York University, presented this point in "Archaeology and the biomolecular 'revolution'; too much of the wrong kind of data" (Collins 2006). The transition from PCR to NGS was not just a transition from one technology to another. For researchers, it represented a fundamental restructuring of the technoscience within wider disciplinary discourses.

Although contamination provided cohesion for the technoscience, so did its status as a celebrity science. Indeed, this thesis has argued that it was a fundamental part of ancient DNA's disciplinary development. Since the start, the investigation of DNA from fossils captivated press and public audiences (Chapter One). *Jurassic Park* and the media provided the momentum behind the emerging practice, helping to marshal interest in terms of financial and organizational initiatives for further researching the recovery of DNA from fossils (Chapter Two). Overall, the interplay between scientists and media contributed to the co-construction of its professional and popular identity. "Ancient DNA" as a term – and the conferences, collaborations, publications, grants, and newsletters that broadcasted this name – served to single out the young practice as something exciting and worthy of investigation. One practitioner presented this point: "The term has certainly also been coined more by people who are trying to sell their science to the media as well. […] It is good to have a brand that people can recognize and to show that what you do is different from other fields, so I think that is why people have used it for the media as well. […]" (12-01:43:15). In other words, celebrity was a survival strategy for practitioners at a time when success was not necessarily a given. The preservation and extraction of DNA from ancient and extinct organisms had to be demonstrated. Obtaining press, public, and political support was important for this. In the 1980s and 1990s, media was welcomed in order to market ancient DNA activity and marshal professional, public, and political interest. "Ancient DNA" was a way of branding a novel and controversial practice, certainly considering that this research practice required expertise and experience across scientific specialties not traditionally in contact. Sociologist Elisabeth Clemmens also noted the way in which media facilitated research by bringing different disciplines together to investigate the question of the death of the dinosaurs: "Our usual image of the sciences is of a congeries of institutionally separate disciplines, each governed by a particular set of practices, professional norms, and cognitive orientations. But, as the impact debates graphically demonstrate, popular culture can serve as a matrix which fosters connections among disciplines that otherwise protect their institutional and intellectual autonomy" (Clemmens 1994, 119). Celebrity, like concerns for contamination, was a critical component in constructing the technoscience's identity as a distinct discipline within evolutionary biology.

With NGS, several scientists seemed to think the technology and sample-driven nature of the practice had passed, but had the technoscience's celebrity-driven nature come and gone, too? Regarding *Jurassic Park* and its role in the development of the discipline, a postdoctoral researcher suggested the celebrity of the technoscience was no longer needed to sustain their studies: "Again, I was not there so I don't remember the influence, but I imagine it was massive because suddenly so many people were interested about something that wasn't there before. That's how the field started. But since the field is established, and especially because of NGS, the field has basically gone off in its own direction and it's supporting itself and it does not need *Jurassic Park* or any such thing to get it off and get the funding because the funding is already there and in plenty – it's just the distribution […]" (34-01:03:00). The Neanderthal Genome Project, however, seems to stand in contrast to this comment, and so does the race to sequence to the first genomes from ancient and extinct organisms. While there is continuity regarding the news values, as well as the interplay between science and media, from the PCR to the NGS era, there does seem to be a distinct difference. Today, it seems that scientists are so aware of and accustomed to the media that they know how to cultivate and control the spotlight in order to pursue, then promote, their research. Today, scientists draw on ancient DNA's populist past to inform its future, and by extension their own scientific success.

3.4.4 Conclusion

The process of boundary building was crucial to the evolution of ancient DNA research from an emergent to more or less established practice. Following years of credibility concerns under the media spotlight, some scientists felt the need to respond to both contamination concerns and celebrity in order to demonstrate that the technoscience was a rigorous and reliable approach for studying evolutionary history. Cooper and Poinar's "Ancient DNA: Do it Right or Not at All" and criteria of authenticity drew a hard line between what they thought was credible versus less credible work. The "non-believers" enforced boundaries to insulate the technoscience from the work of the "believers" whose work, in their opinion, was not to the same scientific standards. Cooper and Pääbo, primary proponents of these criteria, also reacted to celebrity by drawing a line between the scientific significance of a project versus press or public interest as a hallmark of worthwhile research. Here, some argued that investigations of public personalities or speculations about bringing back extinct creatures was sensation, not serious science. This boundary building on both fronts appeared to send a message to internal and external researchers that the search for DNA from fossils was serious business. Researchers tried to standardize the practice in order to stabilize it. Yet boundaries were more than a means to control contamination concerns or celebrity. For several scientists, it was a way to control access to and success in the field. Boundaries were a way to structure a practice that emerged from the interface of disparate disciplines with different expertise and epistemic cultures. However, the divide between "believers" and "non-believers" was but one map of the community in terms of community conflict that researchers on both sides of the divide tried to draw. Some challenged the dogmatic dedication to the criteria in a way that pushed against the seemingly hard and fast divide. This is why boundary-work was so critical, complicated, and contested.

Ultimately, boundary-work was a part of this process. It existed through scientists' day-to-day research, was written into their own histories via inclusion or exclusion of certain citations, and expressed in their memories of their discipline's development. It affected practitioners' individual and institutional identity, especially how they saw their technoscience's identity after its thirty-year history. In reflection, interviewees described the discipline in its early days as an answer looking for a question, rather than a question looking for an answer. They felt that in the early days of PCR, researchers were driven by the technology and samples. With NGS, however, some scientists said the practice was more question driven and therefore, more mature. In an extended episode of boundary-work, interviewees tried to make sense of their history by drawing a line between the technoscience's emergent and more or less established status today. They did this by drawing on larger historical and philosophical conversations about the proper process of science. They were seeking legitimacy following years of contests for credibility. But NGS encouraged exploration again as scientists were naturally driven by the technologies' capabilities and the celebrity surrounding specific samples. The Neanderthal Genome Project, and race for the first genomes of ancient and extinct organisms, hit headlines in both the media and journals like *Nature* and *Science*. The spotlight was not lost. Indeed, some scientists like Pääbo cultivated celebrity. While the star status of the technoscience – a crucial characteristic of ancient DNA's identity – remained, some scientists questioned the future of the field now that contamination, a dominant and defining feature of the field, was less of one. Some argued it would still be a field but one defined by the technology. Others argued the field would dissolve, and ancient DNA techniques would become routine techniques in other fields. However, several said the discipline would die, but that its death was not because of its failure; its death was its success. Without this problem that defined the discipline for most of its history, interviewees questioned the future of the field. For the most part, its future, although uncertain, was open to new developments and directions within evolutionary biology.

3.5 CONCLUSION

In this chapter, I argued that researchers reacted to credibility concerns by building boundaries around the practice to try to demonstrate the technoscience's legitimacy with evolutionary biology. For some scientists, this was incredibly important if they were to transform the technoscience from an emergent into an established practice. By the 2000s, practitioners were engaged in a contest for credibility. For several scientists, this required a response to issues around contamination and celebrity. "Ancient DNA: Do it Right or Not at All" was a response to these issues, drawing a line between what some saw as credible versus less credible work. Criteria emphasized standardization and replication. Criteria was intended to ensure experimental expertise. Standards introduced by this particular publication, initially intended for disciplining the discipline, ultimately divided the community into self-subscribed "believers" and "non-believers" concerning ancient DNA authenticity. Further, some researchers saw criteria as a means to control community competition. By the mid-2000s, however, PCR posed considerable technical constraints, and as a technoscience both defined by and dependent on this technology, scientists struggled to develop the discipline into a credible practice.

Yet NGS offered new opportunities for researchers to overcome many of the contamination concerns associated with PCR. According to several scientists, NGS rescued the technoscience. NGS did not remove the problem of contamination, but it did make it manageable. Now, practitioners faced challenges of too much data. The search for ancient DNA, now the search for ancient genomes, necessitated more than a technical transition. It required researchers to change their infrastructure in terms of finances and expertise. But through it all, celebrity followed the field. Indeed, scientists cultivated and controlled the media spotlight. As NGS rescued the technoscience, it also rejuvenated professional and popular interest in bringing back extinct creatures. Technology, such as NGS and CRISPR, played a part, but discussions of de-extinction were further facilitated by respected researchers like Church, Shapiro, and Hendrik Poinar. The search for DNA from fossils seemed to have come full circle with speculation about resurrection.

Following a thirty-year history of technical transition in the limelight, boundary-work helped structure, then specialize, the discipline as researchers tried to separate issues of contamination from celebrity and competition. Yet boundary-work extended beyond scientists' daily activities. This boundary-work existed in their memories and shaped their identities, especially as they tried to make sense of their history by drawing a line between the technoscience's emergent and established status today. Here, boundary building affected practitioners' individual and institutional identity. It also affected how they saw their technoscience's identity and the future of the field at a time of technological transition. Crucially, the structuring of this history by the technology of the time might give the impression of technological determinism but this is not intentionally implied. Although the technologies of PCR and NGS has been critical to ancient DNA activity, the technoscience was much more than a user community of these techniques. Technology shapes what scientists can do, but it does not suggest an inevitability of success or even change. Ultimately, this chapter argued that boundary-work, in response to contamination concerns and celebrity, was a crucial component to ancient DNA's disciplinary development.

CHAPTER FOUR

THE SPOTLIGHT MOVES ON: ANCIENT DNA AS A CELEBRITY SCIENCE

4.1 INTRODUCTION

Ancient DNA has historically walked a fine line between science and science fiction. This is stressed by its short but sensational history. Its beginnings are a story of science, speculation, and spectacle as the press and public, as well as some scientists, wondered whether DNA from fossils could be used to bring charismatic creatures, such as dinosaurs, back to life. Michael Crichton picked up on the wonder of the new technoscience and played out its fantastic but catastrophic consequences in his book *Jurassic Park*. Steven Spielberg placed the science fiction story in an international spotlight through the movie *Jurassic Park*. Its sequels released over the next twenty years continued to capture our curiosity. Today the franchise continues to be closely connected to the technoscience that inspired it. However, this thesis has also argued that the technoscience was in turn influenced by it. To some extent, *Jurassic Park* did actually drive and develop the hunt for DNA from fossils. Throughout the technoscience's thirty-year history, the rhetoric of resurrection has followed the field closely, and scientists with any association to the field are commonly asked, "Can we resurrect a dinosaur?"

In this chapter, I outline researchers' initial reactions to the idea of de-extinction to try to understand why, and in what ways, the search for DNA from fossils is linked to resurrection research. I argue that the link between the two is not necessarily a link made in the technoscience itself, but one made through the media and through the legacy of *Jurassic Park*. However, scientists are part of this, too. They engage with de-extinction, or at least entertain press and public interest in it, not because it represents their research, but because they understand its advantages when communicating to public and political audiences for support.

In this chapter, I also outline the celebrity science concept; a new concept formed from my synthesis of professional and popular literature on the search for DNA from fossils and from my analysis of oral history interviews with over fifty scientists who work in and around the technoscience. Here, I argue that the evolution of the technoscience into a celebrity science was the result of a relationship actively pursued, then produced, by both scientists and the media. Ultimately, this chapter argues that the search for ancient DNA in the media limelight is a case study of celebrity science with implications for contemporary science communication, and I offer it as a model for other scholars interested in studying other sciences in the limelight.

4.2 BRINGING BACK EXTINCT SPECIES

4.2.1 Introduction

In this section, I investigate ancient DNA practitioners' perspectives of de-extinction in relation to the search for ancient DNA to understand why the two remain tightly coupled today. Researchers with any association with the search for DNA from fossils are frequently asked if it is possible to bring back extinct species. Ancient DNA, however, is only one possible step in a series of steps necessary for achieving some level of de-extinction. Interestingly, ancient DNA researchers have very little to do with de-extinction. Indeed, some scientists, although aware of *Jurassic Park*, have little or no awareness of de-extinction and the debates around it today. But those who do have serious biological, technological, and philosophical concerns. They also argue about the ethics of it. Researchers involved in ancient DNA activity have concerns about the motivations behind de-extinction. They worry whether de-extinction is more about sensation than science. The link between ancient DNA activity and de-extinction is not a link made in the technoscience itself, but a link made through the media and repeated references to *Jurassic Park* in an attempt to link professional research to a popular reference. But some scientists have also encouraged this link by engaging in de-extinction, or at least entertaining press and public interest in it, because they understand the significance of news values for communicating to public and political audiences (Chapter Two). However, for the first time in its thirty-year history, resurrection, once simply speculation, is now a possibility for some scientists thanks to technology like NGS and CRISPR (Chapter Three). For both professional and popular audiences, the narrative of bringing back extinct species has not necessarily evolved beyond *Jurassic Park*, but now extends to speculation about Pleistocene Park with the potential to bring back the mammoth. Overall, interviewees' past reflections, as well as present reactions, to de-extinction represent a timeline of evolving expectations regarding the search for DNA from fossils and its complex relationship with speculations about resurrection.

4.2.2 Debating De-Extinction

In the ancient DNA community, attitudes towards de-extinction are varied. In conversations about de-extinction, some interviewees said this research requires incredible technological and biological improvements, as well as philosophical, ethical, political, or environmental considerations.[39] Yet not all interviewees had something to say. There are several scientists who are unaware of de-extinction, primarily because it is not a part of their research (1; 14; 21). There are some who are aware of it but avoid association with it (2; 48). Others, however, oppose the idea for a range of reasons. One described de-extinction as a "freak idea" (37-01:27:30). Another called it "ludicrous" (2-01:41:50). Others referred to researchers involved in it as "weird," "crazy," or "mad" (30-01:57:50; 18-00:33:35; 5-01:39:30). But some scientists more closely connected to recent de-extinction research had a more confident, yet still circumspect, view of it (6; 15). New technologies and techniques of the last five to ten years have made several scientists from the ancient DNA community reconsider resurrection as a technoscientific possibility, even if their opinion is a critical one. Throughout ancient DNA's disciplinary development, the idea of bringing back extinct species has followed the field closely. Recently, however, de-extinction has evolved from more than an idea and into an emerging endeavor. While most of the ancient DNA community is not involved in de-extinction research, some scientists from the community are responding to it. De-extinction seems to be an emerging but contested technoscience.

In the late 1970s and early 1980s, the lack of technology was a reason practitioners rejected Charles Pellegrino's idea of bringing back dinosaurs via DNA from insects in ancient amber (Chapter One). Although some scientific studies lent credibility to the idea in the 1990s, especially as it gained international interest in the spotlight of *Jurassic Park*, researchers still resisted the idea because the technoscientific hurdles to go from DNA to de-extinction remained high or even far-fetched (Chapter Two). In regards to resurrection, one scientist said, "Oh, I've been ranting about this for a long time, and stop me if I *really* start ranting. […] It's a little like an old cartoon from a science magazine: 'Equations! Equations! Equations!' all over the board and in the middle – 'And then a miracle happened!' – 'Equations! Equations! Equations!' […]" (24-01:46:40). Today, however, there are multiple methods that scientists suggest for bringing back extinct creatures, from back-breeding and cloning to genetic engineering (Sherkow and Greely 2013). Reinhold Rau's project to bring back the quagga is one example of back-breeding. He died in 2006,

[39] This section is a synopsis of researcher reactions to de-extinction based on my interviews. It does not represent de-extinction or the debates about de-extinction in its entirety. See Jacob Sherkow and Hank Greely's "What If Extinction Is Not Forever?" (2013). See Shapiro (2015), Pilcher (2016), and Wray (2017) for works that discuss de-extinction.

but before his death he witnessed the birth of Henry, the result of three generations of back-breeding and the first phenotypically representative relative of the quagga in over a century since its extinction (Max 2006). Cloning offers another option for resurrection research. In 2000, the Pyrenean ibex, commonly called the bucardo, went extinct. In 2009, a team tried to bring it back via cloning, and out of hundreds of embryos, one baby bucardo was born but died ten minutes later because of developmental lung deficiencies (Folch et al. 2009). Additionally, Long Now Foundation's Revive & Restore has ongoing opportunities for genetic engineering to make extant organisms more like extinct ones. One project, conducted by Ben Novak and in collaboration with Beth Shapiro at University of California, Santa Cruz, is trying to bring back the passenger pigeon ("The Great Passenger Pigeon Comeback," 2016). A second study headed by George Church at Harvard University is also experimenting with genetic engineering through genome editing to revive and restore the woolly mammoth ("Woolly Mammoth Revival" 2016). Other studies, like Jack Horner's study to make "chicken-o-saurus," are experimenting with reverse genetic engineering research to make a dinosaur-look-a-like out of a chicken (Horner and Gorman 2009; Harris-Lovett 2015). While there are technoscientific developments, several scientists from the ancient DNA community argue there are biological and philosophical concerns to consider also.

The question of what makes a species a "species" is a long debated one in the history and philosophy of biology (Darwin 1859; Mayr 1963; Hull 1965; Beatty 1985; Dupré 1993; Hey 2001). De-extinction raises the question again but from a very different view. In 2013, Stanford Law School hosted "De-Extinction: Ethics, Law, and Politics" and "defined true de-extinction" as "*when a complete organism from an extinct species is brought back to life"* (Greenberg 2013, 1). However, researchers realized this definition was far from perfect as it begged the question of species continuity or authenticity (Greenberg 2013, 3; Sherkow and Greely 2013, 32). Interviewees say epigenetics, the external and environmental factors that play a part in organism development, are incredibly important. One researcher remarked, "If you were to recreate a mammoth, and you were to release a mammoth – from an elephant – then it may not have any of the mammoth epigenetic changes because it's coming out of an elephant, and that could be massively important because we know epigenetics are so important now" (6-00:26:30). Interviewees say these biological complications have philosophical implications regarding the difference between a mammoth versus what some might say is a "pseudo-mammoth" (Nicholls 2008). A second scientist said "you can mammoth-ize it" but "it will never become *the* mammoth"

(37-01:26:00). A second said, "[…] [I]t's not a de-extinction. It's a sort of 'frankensteinization' of life […]" (8-02:03:00). A postdoctoral researcher, however, said the definition of de-extinction requires a much more nuanced perspective: "It's a *very* interesting problem to start with, and again it comes down to semantics. How do you define passenger pigeon? Is it once you've changed enough genes? […] Do you need to change everything so that you have a completely identical genome to the bird that you got the genome from? Would you call that a passenger pigeon? […]. […] [E]ven if it's genetically identical to a passenger pigeon it's probably not going to be environmentally and ecologically […]" (GI5-01:08:30). In the case of the passenger pigeon project, authenticity is determined by functionality: "[W]e don't necessarily want to duplicate a passenger pigeon, but we want to duplicate its ecology. […] [W]e're just focused on the traits that make passenger pigeon ecology, not necessarily what makes the organism 100% 'it.' […]" (45-00:06:15). Revive & Restore, home to the passenger pigeon project, say the same for their project to bring back the mammoth ("The Great Passenger Pigeon Comeback," 2016; "Woolly Mammoth Revival" 2016). But technology, biology, and philosophy aside, practitioners find ethics a cause for concern.

Interestingly, ethics also dominate interviewee debates about de-extinction. It is not just the immediate issues that concern some scientists, but also the implications of resurrection research for the future. As one researcher remarked, "By the way, if we could 'de-extinct-ify' a mammoth, we could 'de-extinct-ify' a Neanderthal. And then you could multiply the ethical issues by a matter of ten-fold" (3-01:56:00). In 2013, the prospect of bringing back a Neanderthal made international news. In *Der Spiegel*'s "Can Neanderthals Be Brought Back from the Dead?" Church was interviewed and quoted suggesting it could be technically possible to bring back a Neanderthal (Bethge and Grolle 2013). The news went viral. *The MIT Technology Review* ran a report titled "Wanted: Surrogate for Neanderthal Baby" (Rojahn 2013). *Daily Mail* announced, "Wanted: 'Adventurous woman' to give birth to Neanderthal man – Harvard professor seeks mother for cloned cave baby" (Macrae 2013). *The Huffington Post* in Canada shared the story, too: "Dream of giving birth to a bouncing Neanderthal baby? One of the world's leading geneticists believes he can make it happen. George Church, a professor at Harvard Medical School, told Der Spiegel we have the technology to not only reconstruct the DNA of our long-extinct relative, but actually resurrect the species" ("Neanderthal Baby Clone: George Church, Harvard Geneticist, Looks To Resurrect Extinct Species" 2013). Some researchers reacted more critically. Svante Pääbo, for example, responded with a personal piece in *The New York*

Times condemning Church's claim as technically impossible and ethically inappropriate (Pääbo 2014b). One interviewee shared similar sentiments: "George Church and his suggestion that you can clone a Neanderthal is just idiotic. First of all, the technology – and you never want to say *never* – but the technology is nowhere near it. And secondly, why would you want to? Just to see it?" (18-00:28:25). Church, however, felt his message was misunderstood and mistranslated by other media outlets. In response, *Der Spiegel* tried to clear the confusion by publishing another piece titled "Surrogate Mother (Not Yet) Sought for Neanderthal" to set the record straight ("Surrogate Mother (Not Yet) Sought for Neanderthal" 2013). Nonetheless, this controversy highlighted the ethics of research responsibility and that scientists have something to say about the bioethics of de-extinction.

It appears that for some scientists, de-extinction is a moral matter. Some say de-extinction is a reasonable response to extinction. Paleontologist Michael Archer, for example, says we have moral obligation to bring back extinct species like the thylacine, because we – through human population and predation increase – were the cause of their demise (Archer 2013). However, some say de-extinction is an inappropriate use of research or resources arguing it is morally wrong to resurrect a mammoth or any other animal for entertainment: "Some people clearly just want to have zoos. […] All they want to do is put a mammoth in a zoo or a wild life park […] [A]re you doing that to make money or are you doing that to inspire people to love nature?" (6-01:55:00). Even if inspiring people to care for or conserve nature is the motivation, others think conservation efforts are better invested in preserving the current environment instead of resurrecting what is already lost: "[I]f you're going to spend the money, it is morally wrong to focus on bringing back an animal that is extinct to the exclusion of an animal that isn't extinct but will be extinct." This same scientist said that if "aliens landed and looked around" then "they'd be pretty surprised to see that we decided to piss the last of our resources on trying to bring back the mammoth" (2-01:39:15). In other words, motivation matters, and when it comes to de-extinction, entertainment potential should never supersede resource or research value. Indeed, there are interviewees that think de-extinction seems to be more spectacle than science, a gimmick to allure media attention: "It's more hype than science" (35-01:04:30); It's cheap, it's sleazy, it's the Sunday news. And that's very rarely right, in fact, meaning always wrong" (32-01:27:30); "[T]he only reason to do this is to get an article in *National Geographic*" (14-P2-00:25:00); "The only reason to do it is to create a kind of splash and get attention. And that's not a good reason to do science" (47-00:25:30). For interviewees, research relevance matters. Consequently, they view practitioners whose projects appear to prioritize the media spotlight over scientific significance with suspicion, even disdain.

Archer, the main mastermind behind the project to bring back the thylacine, has been a target of criticism from colleagues. In 2000, at the height of hype but skepticism for the search for DNA from fossils, the Australian Museum broadcasted the Thylacine Cloning Project (Colgan and Archer 2000). Initiated by Archer, the project announced a twenty-year trajectory for resurrecting the thylacine via DNA from a pickled pup. This broadcast made national and international news (Fletcher 2008; Fletcher 2010). However, some scientists were less than amused, protesting the project on grounds that it was more spectacle than science. Some argued it was a strategy to attract press and public attention. One practitioner offered this opinion: "I've just said bollocks all the way through. I get very upset. I could talk myself into a bit of a frenzy over it because I think it's so misguided. It isn't going to happen, in my opinion. It's cheap and sleazy in terms of the way that it attracts media attention and the public." Specifically, "I think […] the thylacine is a perfect example. 'We're doing it! It's almost there! It's going to be here in a year or two!' It's a fucking snake oil salesman. […]" (32-01:17:30). For this practitioner and others, the technological and biological hurdles were high, but researchers also took issue with the way the project was presented. For example, ancient DNA researchers like Alan Cooper and Jeremy Austin, openly opposed the project, arguing its achievement was far from possible; they took issue with what they thought was premature publicity for a project that had little to no technoscientific evidence for its feasibility (Fletcher 2008; Fletcher 2010). Scholar Amy Fletcher, University of Canterbury in New Zealand, described the debate around the thylacine's resurrection as a controversy over control for how the project should be framed and interpreted, as either science or spectacle, across the mass media (Fletcher 2008; Fletcher 2010). As she argued, "When it launched the thylacine project, the Australian Museum walked out on the unstable precipice of 'paleogenomics as science' versus 'paleogenomics as spectacle.'" (Fletcher 2010, 51–52). Researchers use of the media spotlight, intended to promote the project, simultaneously destabilized it. For Cooper and Austin, the project was problematic, and as it involved multiple interests, scientists on both sides of the debate found it difficult to control the story – whether the museum wanted to tell a story of science or whether opponents wanted to shame it as an act of spectacle.

Struggles to define de-extinction as science or spectacle is a consequence of a long legacy of debates in professional and popular circles tracing back to *Jurassic Park* (Chapter One, Two, and Three). This legacy makes some scientists question the motivation behind de-extinction. They ask about de-extinction's scientific purpose, potential, and pay-off: "[…] 'What's the point of doing it? Is it worth it?'" (38-00:23:00); "[…] [W]hy would you want to? Just to see it?" (18-00:28:25); "But why the hell are we doing it?" (24-01:46:40). Some oppose bringing back extinct creatures because they think these projects prioritize the sensation of de-extinction over the scientific significance of it: "There's no scientific question that could possibly benefit us. […] You don't do science unless it's hypothesis-driven. You want to bring back a dinosaur? What are the questions?" (39-00:29:00). Noting the professional and popular shift in attention from bringing back dinosaurs to mammoth revival, one researcher remarked, "I see the reel being replayed now with […] how we're going to resurrect mammoths […]. […] I know this sounds cynical, but they're exploiting the journalists – the media – for attention" (17-01:19:00). However, there are arguments for de-extinction's scientific significance as several scientists in the ancient DNA community are taking the time to educate themselves about its potential. In *How to Clone a Mammoth*, Shapiro presents the first concise and comprehensive account of the science and science-fiction of de-extinction (Shapiro 2015). Here, she explores the technoscientific feasibility of de-extinction by outlining the series of steps and obstacles to overcome to make it a reality. She also explores the possible payoff of de-extinction, arguing that the ultimate goal is not necessarily the resurrection of a long-lost species. Rather, the goal is the revitalization and stabilization of ecosystems. For example, reviving the mammoth, or an elephant with mammoth-like traits, and restoring it to colder climates like Siberia, particularly places like Pleistocene Park, could reintroduce the growth of the grasslands that were once an essential element of the tundra ecosystem before the mammoth's extinction (Shapiro 2015). For some scientists, de-extinction is a holistic rather than individualistic effort.

As researchers debate the science or spectacle of de-extinction, there are some that say there is a place for spectacle in science. In discussing de-extinction, another interviewee added, "I think it's a mix of both. It's certainly sensation, but you have to have a lot of science to make it happen" (30-01:53:25). Indeed, in the history of ancient DNA research, speculation and spectacle propelled the technoscience forward. In the 1980s and 1990s, imagination led to innovation (Chapter One and Chapter Two). In outlining oppositions to de-extinction, Jacob Sherkow and Henry Greely of Stanford Law School and Stanford University also acknowledged its benefits. Sherkow and Greely argued that the "wonder" associated with bringing back extinct species was a positive, not a negative, point: "The last benefit might be called ‘wonder,’ or, more colloquially ‘coolness.’ This may be the biggest attraction, and possibly the biggest benefit, of de-extinction. It would surely be very cool to see a living wool[l]y mammoth. And while this is rarely viewed as a substantial benefit, much of what we do as individuals – even many aspects of science – we do because it’s ‘cool.’” (Sherkow and Greely 2013, 33). One researcher, in response to the question about de-extinction as science or sensation, remarked, “Well, the de-extinction is definitely science. Hyperbole is the word that has often been used. […]. And so in a sense this comes across to people as sensationalism or hyperbole: ‘You haven’t done it yet and yet you’re already saying how it’s going to fix up the world.’” For this researcher, this is part of the innovation process: “[…] [B]ut that’s about putting a vision in front of what you’re doing. […]. The public want to know why you’re doing it. What’s the point of this? Where’s it going? If it succeeds, what will it mean? If it doesn’t succeed, what will you do? They need context and I think it’s up to us as scientists to not only say what we’re doing but to put it in a broader context” (40-01:40:45). After a thirty-year history of disciplinary development under intense public interest and extreme media exposure, researchers were accustomed to the spotlight but wary of the difficult balance between celebrity and credibility.

4.2.3 Evolving Expectations

In the 1990s, *Jurassic Park* introduced the search for DNA from fossils into the popular and professional consciousness. The book and movie raised awareness, attention, and even expectations. For this already public-facing practice, *Jurassic Park* became the ultimate idea and image in the minds of the media of what the technoscience may one day accomplish. One scientist said, “I think if *Jurassic Park* had come out twenty years later I don’t think necessarily there would be quite the association that there is with it today, but since it was at its onset and since there’s this link […] it sort of followed the field, again, for better or for worse.” However, this interviewee was wary of inflating its influence: “It is a story of fiction and I think people on both sides in the media get its importance and significance wrong, and the scientists sometimes overstate its importance in driving our field in a particular direction as well.” Nonetheless, the link between the two did have an influence. Here, the media’s repeated references to the technoscience and *Jurassic Park*, attempts to link professional research to a popular reference, reinforced an association between the two. This same scientist said, “I think people on both sides suffered from its tremendous impact. […]. It will always be tightly associated […] and you can see that in the collective memory of the journalists. The journalists always end, certainly when you talk about de-extinction, it's *always* back to *Jurassic Park*. […]" (33-00:04:15). Another interviewee put the point more bluntly: "I think to some extent there was a cultural feedback between *Jurassic Park* and the field" (15-00:56:00). *Jurassic Park* encouraged public expectations that researchers may one day bring back extinct creatures, while a series of studies in the 1990s claiming the recovery of DNA from insects in ancient amber seemed to suggest that at least the first step of the process was possible. One practitioner put it this way: "It all comes from ancient DNA, but that doesn't mean that the ancient DNA researchers are engaged" (41-00:26:00). For better or worse, the interplay between the technoscience and the idea of bringing back extinct species reinforced press and public expectations that *Jurassic Park* might be more than science fiction.

When researchers recall the 1990s, they remember it as a decade colored by hype for *Jurassic Park*. As one researcher recalled, "[…] [T]here was a big hype and big wild dreams about resurrecting dinosaurs – […] *Jurassic Park* like ideas. […]" (37-01:02:30). Another researcher also recalled "a lot of really ambitious speculation" but that "amongst the scientific community" there was "a lot of realism about what could be done and what couldn't be done" (4-45:35:00). As scientists searched for multi-million-year-old DNA, particularly DNA from insects in ancient amber, their goal was not to bring back a dinosaur. Rather, their goal was to test and define the limits of DNA preservation from ancient and extinct specimens. Nonetheless, the series of studies that reported the recovery of multi-million-year-old DNA – published in prestigious journals like *Nature* and *Science*, then publicized across the mass media – appeared authentic particularly behind the backdrop of *Jurassic Park* (Golenberg et al. 1990; Johnson 1990; Cano, Poinar, and Poinar Jr 1992; DeSalle et al. 1992; Cano et al. 1993; Begley 1993; Woodward, Weyand, and Bunnell 1994; Monastersky 1994). One practitioner offered this opinion: "[…] [W]hen *Jurassic Park* came out the […] public expectations became that you can get DNA 50 or 60 million years old, or even older, and so that meant that some scientists who had obtained such evidence found that their experiments were then kind of more accepted by the public as being genuine because the public believes that that was the way things ought to be. And the public, of course, included a lot of other scientists" (4-45:35:00). However, skepticism followed optimism as researchers tried to replicate results but failed to do so: "[…] [A]t the Natural History Museum in London where I was trying to get DNA out of insects preserved in amber – you know, the whole sort of *Jurassic Park* concept […]. I honestly thought that was going to work and it clearly doesn't, and so we do know there is an upper bound on the amount of time we can go back in time to get real ancient DNA"

(25-00:43:45). The debunking of DNA from insects in ancient amber had an important impact on the field. It threw the field into a state of disrepute because the research reality contradicted public expectations of it, expectations that had been based on the science fiction story of *Jurassic Park* but ultimately grounded in the idea of extracting and sequencing DNA from fossils.

Jurassic Park posed problems for practitioners because it raised expectations among the press and public that did not coincide with the reality of the research. This challenged their credibility. For some scientists, *Jurassic Park* had a negative impact that cost the field its credibility: "[…] [I]t was a big impediment because people put too high hopes into ancient DNA research connected to things like *Jurassic Park*, and I don't know if it was just coincidence or incompetence (or I shouldn't say incompetence but lack of experience with the technology in the early days), but many people actually published million-year-old sequences which later turned out to be rubbish, and that has damaged the credibility of the field *incredibly*." Overall, "[I]t has damaged the field more than it has helped it" (15-00:50:30). Another interviewee shared similar sentiments: "I think it has had a bad influence […] because it raised […] levels of expectations about DNA and what […] ancient DNA could do. […] [U]nfortunately, because it was made by a great director – Steven Spielberg – it's a film that sticks in people's minds. […] And it gets shown again, and again, and again." (5-01:20:00). For this scientist, *Jurassic Park*'s link as a popular reference to professional research (despite studies demonstrating that multi-million-year-old DNA is highly unlikely) continues to influence the public perception of the technoscience: "[W]hen I give a talk about ancient DNA, they put up a poster and it has a *dinosaur* in it. I've […] objected […]. I've said, 'There's no dinosaur DNA. You should *not* show the dinosaur.' [I]t's had a bad influence. We're still living it down" (5-01:22:00). According to this interviewee, it also diverted attention from other research opportunities and damaged reputations: "[…] [D]inosaur DNA […] diverted a lot of time and energy and ruined several scientists' reputations. There was a load of media hype about their work and then it was just shown to be contamination, and I think they're still living that down" (5-01:23:00). Another researcher remarked, "The media think about *Jurassic Park* when they think about ancient DNA. That's the trouble" (23-02:11:00). However, others have a more positive perspective.

Interviewees also admit *Jurassic Park*'s positive influences. One scientist said, "On balance, if I could go back and say, 'Are we going to have *Jurassic Park* or not?' I would have it. […] I think its benefits outweighed its negative influences. The benefits were by making the whole research field in the public eye." This same scientist said, "I think the negatives could have been very bad, but I think that because the community did deal with it in the late '90s, the negatives, in the long term, have not been damaging. And so, in that regard, *Jurassic Park* had a positive effect and that has been long lasting" (4-01:20:00). *Jurassic Park* coincided with, then catalyzed the search for ancient DNA into the media spotlight which helped generate interest in the novel and controversial practice (Chapter Two). It was a crucial component to its disciplinary development. However, by the mid-to-late 1990s, increasing issues of contamination and even celebrity put the technoscience's credibility on the line (Chapter Three). The public interest in the legacy of *Jurassic Park*, once fundamental to its formation, posed a problem as researchers tried to transform the evolving practice into an established one. In retrospect, however, another scientist added that this was a normal part of testing and imposing limits: "[…] *Jurassic Park* was […] beneficial to the field of ancient DNA research […]. [T]he early age of ancient DNA research seemed like the motif was shooting for older and older DNA, and everyone wanted to see if you could find dinosaur DNA. There was a major push for that in *Jurassic Park*, and because it was such a sensational provocation to get it, people tried, and they *failed*. And they learned something *valuable*. You can't get dinosaur DNA" (45-00:54:30). As a result, researchers started investigating the nature of DNA degradation, how long it lasted, and under what causes or conditions it survived: "[…] [I]t really brought the hammer down on the field that there had to be protocols that ensured quality of data and quality of the process. […]" (45-00:54:30). In other words, exploration – success and failure included – was part of the process. Celebrity did not necessarily compromise the technoscience's credibility, but it did complicate it.

In reflecting on the technoscience's history, researchers rationalized its successes and failures through analogies of development from an immature practice to a more mature one today: "The problems that paleogenetics had in the '80s is a bit like a child growing up: you fall down and you get up again. And I think paleogenetics is way more mature now" (13-00:17:00); "[…] I would say we went through this long period of birth and adolescence and we're finally a mature discipline […]. We've finally become the researchers we always wanted to be […]" (27-02:45:00). One researcher recognized that though the field had grown, it was still a young field: "It's still a science in diapers, but at least the baby's walking" (31-00:37:20). For a fourth, this process of disciplinary development, especially with the introduction of new technologies and techniques like NGS, was not necessarily unique to ancient DNA research, but rather a frequent feature of emerging research: “I’d say this research discipline has developed the way that all science – new scientific disciplines – develop in that you have an initial wonderful discovery, you have lots of hype and high expectations, and then you come down to it with a bump, and then you do the hard work of working out what it all means and what you can really do: what is realistic and what isn’t.” This same scientist said, “And that may take the next ten to twenty years of that research discipline, and I think we have got through that phase now of sorting ourselves out, and I think with these next-generation sequencing techniques we have to do it all again; come down to it with a bump, and sort out what we can and can’t do. So, I think it’s cyclical” (5-01:52:50). New technology, like NGS, opened up the technoscience to the population genetics, synthetic biology, and conservation biology communities, particularly in regards to de-extinction. Yet as they entered an era of exploration with this new technology, their memories of ancient DNA research’s past successes and failures shaped their expectations of the future of the field.

Today, the rhetoric of resurrection is still closely connected to the search for ancient genes and genomes, but there has been a subtle shift in focus. Today, questions are less about dinosaur resurrection and more about mammoth de-extinction. New technologies and techniques, like NGS and CRISPR, are partly responsible for this and the seriousness in which some, although not all, scientists are reconsidering resurrection. One researcher reflected, “In the good old days, you could conveniently say, ‘You can’t clone a mammoth.’ And that was the end of it. But with NGS, it’s got a bit difficult now […]” (2-01:37:30). Another added: “[…] [C]an we clone dodos and thylacines and mammoths? […] [A]t the time I said, ‘Oh no, that’s never going to happen and the first reason is because we’re never going to be able to sequence whole genomes of extinct species.’ And of course, five years later, the field starts sequencing genomes of extinct species. That’s completely changed the way people view what you can and can’t do in the ancient DNA field. […]” (25-00:43:45). In 2006, scientists sequenced 13-million-base-pairs of DNA from a 28,000-year-old woolly mammoth, demonstrating the powerful potential of NGS as applied to fossils (Poinar et al. 2006). According to the press, this was a step closer to generating whole genomes which also meant a step closer to bringing long-lost creatures, such as the mammoth, back to life. *Newsweek* reported this research, explaining, “The scientists, in other words, had managed to assemble half the woolly-mammoth genome; they claimed that in three years they could finish the job. That would put scientists within striking distance of an even greater feat: repopulating the earth with creatures that vanished ages ago" (Margolis 2006). NGS revived press and public interest in resurrection, but it also inspired some scientists to reconsider resurrection, too.

In 2008, for example, scientists sequenced the nuclear genome of the woolly mammoth (Miller et al. 2008). *The New York Times* announced, "Scientists are talking for the first time about the old idea of resurrecting extinct species as if this staple of science fiction is a realistic possibility, saying that a living mammoth could perhaps be regenerated for as little as $10 million." Reporter Nicolas Wade explained that the feat was not so simple, but one practitioner, Stephen Schuster at Pennsylvania State University, made it seem within reach: "There is no present way to synthesize a genome-size chunk of mammoth DNA, let alone to develop it into a whole animal. But Dr. Schuster said a shortcut would be to modify the genome of an elephant's cell at the 400,000 or more sites necessary to make it resemble a mammoth's genome. The cell could be converted into an embryo and brought to term by an elephant, a project he estimated would cost some $10 million." Wade quoted Schuster: "'This is something that could work, though it will be tedious and expensive,' he said" (Wade 2008). One researcher recalled this comment: "[…] The first mammoth genome […] – Stephen Schuster […] provided one of those quotes to the media that they love. He said something like, 'Give me a few million euros or dollars and I will recreate the mammoth for you.'" (37-01:24:50). Yet Schuster was not just appealing to press and public curiosity. According to some scientists, this may have sounded like hype reminiscent of the 1990s and early 2000s, but for others de-extinction could be a research reality.

TEDxDeExtinction, an event co-hosted by National Geographic Society, TED, and Revive & Restore in 2013, positioned the idea of bringing back extinct species, for the first time, in a professional context (Chapter Three) ("De-Extinction Projects, Techniques, and Ethics" 2012; "TEDxDeExtinction" 2013). The event featured over twenty talks on de-extinction, with Church and Shapiro, as well as Hendrik Poinar, speaking specifically on the potential to bring back the mammoth ("Woolly Mammoth Revival" 2016; "The Great Passenger Pigeon Comeback" 2016; Gewin 2015; Shapiro 2013; Poinar 2013; Church 2013). Although they highlighted the considerable technological and biological challenges associated with such a task, their positions as respected researchers have caused some scientists in the ancient DNA community to reconsider the idea of resurrection. One interviewee put it this way: "It's dangerous to say that the de-extinction people are crazy people. It *used* to be crazy people. The reason I got interested is because I suddenly saw serious people in it. So, there are certainly crazy de-extinction people, but there appear to be very credible de-extinction people" (6-02:00:00). As expectations evolved, so did the spotlight. While *Jurassic Park* may be on much of the press's and public's minds, some researchers and reporters are realizing that Pleistocene Park could be the new technoscientific trend. TEDxDeExtinction, as well as Carl Zimmer's 16-page print in *National Geographic*, placed this technoscience in a media spotlight (Zimmer 2013). From this view, the emergence of real de-extinction research today, with the enthusiasm but criticism that surrounds it, shares parallels with the emergence of ancient DNA activity in the 1980s and 1990s (Chapter One and Chapter Two). It could be that de-extinction, with some serious science behind it, is in a prime position to become a technoscience and celebrity science in its own right.

4.2.4 Conclusion

This section examined ancient DNA practitioners' perspectives of de-extinction in relation to the search for DNA from fossils in order to understand the press and public fascination with resurrection that has followed the field since its start. Pellegrino discussed de-extinction as early as the 1980s, but his ideas gathered little to no traction due to a lack of technology, as well as other reasons (Chapter One). In the 1990s, Crichton's and Spielberg's *Jurassic Park* introduced the idea and its implications to public and professional audiences worldwide (Chapter Two), but TEDxDeExtinction, in 2013, represented the first formal forum to discuss de-extinction in light of new technologies like NGS and CRISPR (Chapter Three). Despite this close connection between the technoscience and de-extinction, interviewees involved in the search for DNA from fossils are far from involved with resurrection research. This link between the technoscience and speculations of resurrection is not a link made in the technoscience itself, but one made through the popular press and legacy of *Jurassic Park.* Ancient DNA is only the first possible step in a series of steps necessary to make it happen, but both journalists and scientists entertain de-extinction, and *Jurassic Park* specifically, in an attempt to link professional research to a popular reference. Researchers, like reporters, acknowledge the advantages of this news value. In many ways, its news value helped promote the practice. But *Jurassic Park*'s legacy, according to interviewees, has had negative influences on the field, specifically as public expectations for multi-million-year-old DNA were not met by the research reality. With credibility on the line, some scientists saw *Jurassic Park*, as well as the press and professionals who played to it, to blame. Interviewees' wariness about de-extinction as more spectacle than science, like in the case of the thylacine or Neanderthal, should also be understood in this context. However, ancient DNA researchers' interest in de-extinction is changing as technology is changing. In light of innovations like NGS and CRISPR, influential individuals like Church, Shapiro, and Hendrik Poinar are addressing de-extinction's potential and its challenges. These interviewees' past reflections and present reactions to de-extinction embody a timeline of evolving expectations regarding the technoscience and its relationship to ideas of resurrection. Even in a celebrity science, the spotlight moves on. As expectations evolve, some scientists move with it.

4.3 LIFE IN THE MEDIA LIMELIGHT

4.3.1 Introduction

In this section, I outline the celebrity science concept and its implications for understanding contemporary science communication by using the search for DNA from fossils as a case study of celebrity science. Ancient DNA offers an opportunity to trace the development of a discipline from the 1980s to today, at a time when expectations in science communication were evolving. First, I offer an outline of the celebrity science concept in terms of its definition, essence, and process. This is a new theoretical concept formed from my synthesis of professional and popular publications surrounding ancient DNA activity, and from my analysis of interviews with scientists. Here, I argue that the evolution of the technoscience into a celebrity science was the result of a relationship actively pursued, then produced, by both scientists and the media. Second, I outline the implications of the celebrity science concept for contemporary science communication. I argue that this phenomenon is best understood in the context of science communication movements in the 1980s, as well as other movements like the rise of modern media and celebrity culture. I also argue that researchers, scientific institutions, and scientific journals consider the value of news when conducting or reporting research and that this affects scientific publishing and publicity practices. Drawing on science studies scholarship as extensive evidence of the interplay between science and media across time and different subjects of science, I highlight how and when researchers use celebrity to accomplished their goals. I suggest the celebrity science concept as a model for other scholars interested in studying other sciences in the spotlight.

4.3.2 The Celebrity Science Concept

I argue that a celebrity science is a subject of science that exists and evolves under intense public interest and extreme media exposure. The mass media are crucial in the making of a celebrity science. They seek the science and its scientists for its news values, its potential to attract public attention. They craft opportunities for publicity. But scientists participate in the making of a celebrity science, too. The mass media influences are so substantial that researchers respond, positively and negatively, to the attention and even reinvent their reputation and the technoscience's reputation accordingly. Ultimately, a celebrity science is the outcome of prolonged publicity advanced by a relationship that is actively pursued and produced by both scientists and members of the media.

The term celebrity science was chosen as an extension of the term celebrity scientist, an idea introduced by communication studies scholar Declan Fahy (Fahy 2015). Though the term celebrity scientist is relatively recent (specific to the modern movement of celebrity culture), it feels familiar because scientists have had a public presence in the past (Goodell 1977; Gregory and Miller 1998; Broks 2006; Fahy 2015). Thomas Edison, for example, was the late-nineteenth-century image of an iconic inventor, while Albert Einstein personified early-twentieth-century physics (Pretzer 1989; Barrow 2005). Fred Hoyle was the voice of astronomy on the radio, while Carl Sagan became a celebrity in cosmology thanks to television (Davidson 1999; Gregory 2005). Recently, Janet Browne, historian of science, also argued for Charles Darwin as a "scientific celebrity" of late-nineteenth-century natural history (Browne 2003). However, the rise of the mass media in the mid-to-late twentieth century, and journalists' increased interest in science, offered scientists opportunities to become public-facing practitioners on a new level (Friedman, Dunwoody, and Rogers 1986; Gregory and Miller 1998; Broks 2006). Science in the news became-routine. In the 1970s, science communication scholar Rae Goodell highlighted the effects of this, profiling a range of researchers from anthropologist Margaret Mead and biologist Paul Ehrlich to chemist Linus Pauling and astronomer Carl Sagan. According to Goodell, these scientists were "visible scientists" (Goodell 1977). For Goodell, these visible scientists shared personal and professional characteristics, media-oriented characteristics, that helped them attain press and public visibility.[40] These visible scientists then used their authority and access to the media as a platform from which to speak to the public not just about science, but also about science policy. Fahy's celebrity scientist concept builds on this. For Fahy, the celebrity scientist is a new breed of scientist that has emerged in light of the rise of a new celebrity culture.

[40] Rae Goodell argues that visible scientists embody personal and professional traits that make them attractive to the public. She argues that visible scientists are articulate, have a colorful image as well as a credible reputation, and speak on hot or controversial topics. See Goodell (1977).

Fahy argued that from the 1980s onward, media started treating scientists as celebrities. These references became apparent at the turn of the century. For example, *The Independent* said science was full of "media superstars," *New York Times* called Neil deGrasse Tyson a "space-savvy celebrity," while *Nature* called Susan Greenfield a "celebrity neuroscientist" and *Science* called her a "rock star" (Connor 2001, 11; Martel 2004, E5; *Nature* 2004, 9; Bohannon 2005, 962; Fahy and Lewenstein 2014, 87). For Fahy, there was a qualitative difference between the visible scientists of the past and the celebrity scientists of today, and Carl Sagan embodied that difference. Fahy, in a co-authored article with science communication scholar Bruce Lewenstein, argued that in the 1960s and 1970s, Sagan was what Goodell called a visible scientist. According to Fahy and Lewenstein, however, his visibility turned to celebrity after his big television break with his personal but professional show *Cosmos*: "Sagan marked the shift from visible scientist to celebrity scientist; he was a celebrity within a general culture that increasingly valued celebrity for its own sake […]" (Fahy and Lewenstein 2014, 86). This rise of celebrity culture, the increasing value of celebrity for its own sake, was a new social phenomenon introducing a new view of "scientific stardom" (Fahy and Lewenstein 2014, 93). In this celebrity culture, a scientist could achieve visibility while not necessarily achieving celebrity. Under this influence of celebrity culture, Fahy profiled a series of scientists who qualified as celebrity scientists. These celebrity scientists, like the cosmologist Stephen Hawking and late paleontologist Stephen Jay Gould, were credentialed experts in their professional sphere but also had attained fame, fortune, and influence in the public sphere. As celebrity scientists, they used the media as a public platform to popularize science and influence public attitudes towards science.[41] For Fahy, however, stardom's influence cuts both ways. As celebrity scientists, their stardom affords them influence within science (Fahy 2015, 3). In other words, stardom filters back into science. Celebrity affects the process of science.

To understand celebrity's complexity, I suggest that a definition of celebrity science, a concept that operates on the group level, is best understood with attention to two things;

[41] Declan Fahy and Bruce Lewenstein suggest additional attributes of the celebrity scientist. See Fahy and Lewenstein (2014).

its “essence” and “process.”[42] In exploring the essence of a specific celebrity science, we can ask questions like, “Why or to what extent does a subject of science or technoscience attract press and public interest?” For example, why does the search for DNA from fossils attract so much public interest and media exposure, and why has that attention followed the field so closely over its thirty-year history? Here, I only explore, not explain, the essence of a celebrity science, and I use Fahy’s celebrity scientist concept as a starting point. According to Fahy, the making of a celebrity scientist is a four-step process. The celebrity scientist must have one) scientific expertise, two) access to and adoption of alternative outlets, like media outlets, for communicating that expertise, and three) the ability to engage with a wide spread audience on a wide range of issues. For Fahy, however, the fourth step is the most important step. Through media efforts, as well as their own attempts, an individual comes to represent the iconic image of a scientist, embodying the ideas, ideologies, and issues unique to their time. For Fahy, a celebrity scientist is the public face of science (Fahy 2015, 205-206). In reference to the researchers he profiled, he explained, “Their words and images became valuable to publishers and broadcasters: the scientists became cultural commodities” (Fahy 2015, 205). This last step, as Fahy argued, is the “most elusive, most abstract, yet most important feature of fame” (Fahy 2015, 206). To illustrate this, he provided this point: “As cultural critic Louis Menand explained, this is the way the star’s personality intersects with history, the way his or her stardom coincides with the Zeitgeist, the spirit of the age, so that there is a perfect correspondence between ‘the way the world happens to be and the way the star is.’” (Fahy 2015, 206; Menand 1997). I suggest that this last step in the making of a celebrity scientist is the first step in the making of a celebrity science. In other words, where the celebrity scientist ends is where a celebrity science begins. Here, I suggest that this “perfect correspondence between ‘the way the world happens to be and the way the star is’” says something about the essence of a subject of science that lends itself to news value, intense

[42] Scholars have debated the definition of celebrity with attention to ideas of one) celebrity as a quality immanent to the individual or two) celebrity as an industry, something made by the mass media (Dyer 1979, 1986; Wernick, 1991; Gamson 1994; Marshall 1997; Giles 2000; Turner, Marshall, and Bonner 2000; Rojek 2001). See Turner (2004) and Evans and Hesmondhalgh (2005). In this thesis, I interpret these discussions and their applications to the celebrity science concept in terms of trying to understand a celebrity science with regards to its “essence” and celebrity as a “process.” In this history, I take the technoscience’s news values, or “essence,” for granted in exchange for exploring its disciplinary development into a celebrity science as a “process.” However, it is critical to mention this idea of “essence,” even though the term carries historical and philosophical baggage, because it speaks to the question of why a specific science subject elicits so much press and public enthusiasm in the first place. Indeed, Rae Goodell and Declan Fahy acknowledged that the making of a visible scientist or celebrity scientist is a process, but they also argued that the individuals who became visible or celebrity scientists possessed personal and professional traits that made them attractive to the media in the first place. Understanding the power of celebrity requires recognizing both these considerations.

public interest, and extreme media exposure. Perhaps the fascination with the search for DNA from fossils is a consequence of a long line of popularization in public-facing fields from paleontology and archeology to molecular biology (Judson 1979; Clemens 1986; Rainger 1991; Glen 1994; Nelkin and Lindee 1995; Mitchell 1998; Keller 2000; Cohen 2002; Sommer 2007; Schmalzer 2008; O'Connor 2008; Bowler 2009; Brinkman 2010; Manias 2013; Rudwick 2014; Hochadel 2016). But interest is not enough to make a subject of science into a celebrity science. A celebrity science is also a process.

The most important part of the celebrity science concept is understanding that it is a process and that celebrity can operate on a group level, not just an individual level.[43] Here, we can ask questions such as, "How does the mass media represent the science or technoscience to the public?" "What are the effects of press and public attention on the science or technoscience?" "Do researchers respond to the attention?" "If so, how, why, and does this make a difference to the process of science and science communication?" I suggest there are specific signals that indicate the presence of, or the process of making, a celebrity science.

First, the mass media is a crucial component in the making of a celebrity science. Members of the media seek the science and its scientists for the news value and potential to attract public attention. The media offers occasions for publicity. However, press and public interest is not enough. The second and most important part is that scientists participate in the process, too. In a celebrity science, mass media influences are so substantial that researchers respond, positively and negatively, to the attention. The making of a celebrity science is an active process involving both scientists and the media.

Chapter One and Chapter Two are evidence of a difference regarding a passive or active relationship between scientists and the media. In Chapter One, scientists did not necessarily capitalize on press and public interest in the idea of using DNA from fossils to

[43] A challenge to my celebrity science concept is how to conceptualize celebrity on the group level. In celebrity studies, the focus is largely linked to the individual. My goal is to extend the concept of celebrity from the individual to the group level. My goal is not to discuss the celebrity scientist, but to discuss celebrity science and how intense public interest and extreme media exposure affects the group. Of course, the group includes the individual, and the role of the individual – particularly regarding personality – is important. This was discussed in Chapter Two (Section 2.4.3) and will be discussed in detail in Chapter Four (Section 4.3.3). Here, agency is an issue when conceptualizing celebrity on the group level. For example, who or what functions as the celebrity within a celebrity science? In the technoscience of ancient DNA research, is it the fossils? Is it the cultural conception of DNA? Is it speculation about what can be achieved using DNA from the fossils? Is it the idea of and image of *Jurassic Park* that carries celebrity? Is it a combination of these features? Or is it the type of researchers this research attracts, and is it these researchers' ability to adapt to the technoscience's news values? These questions about agency share parallels with questions about the essence of a technoscience that attracts press and public interest.

bring back extinct organisms such as dinosaurs or mammoths. While this sort of speculation played a part in the emergence of the technoscience, researchers were cautious to capitalize on it publicly. In Chapter Two, however, the interplay between science and the media intensified as researchers recognized that media exposure, often tied to the idea of bringing back extinct species, was a way to communicate to popular or political audiences for support of the emerging practice. Scientists acknowledged the advantage of news values and responded to it not just by making constant contact with the press but by adopting, then adapting the language of news values when communicating their research. Scientists responded to the media by reinventing their reputation and that of the technoscience according to this attention. In Chapter Three, researchers' responses to contamination concerns and celebrity through boundary-work are evidence of this. They learned how to simultaneously cultivate and control celebrity. Chapter Three, again, demonstrated how scientists sought the media spotlight through the search for the first and oldest genomes, packaging their research in a way that hit headlines. Ultimately, a celebrity science is the outcome of prolonged publicity advanced by a relationship that is actively pursued and produced by both scientists and members of the media. It is an active process. It is a dialectical process. A celebrity science is a co-constructed and co-evolving relationship between scientists, the media, and the public.

This definition of the celebrity science concept also requires a reference to celebrity studies. In this literature, however, there is no clear consensus on what constitutes a celebrity. Indeed, the definitions of both celebrity and celebrity culture are debatable (Turner 2004; Evans and Hesmondhalgh 2005). Yet Fahy, in forming his celebrity scientist concept, selected a definition developed by cultural studies scholar Graeme Turner that captured celebrity's complexity (Fahy and Lewenstein 2014, 88). Turner considered celebrity "*a genre of representation*" and *"a commodity traded by the promotions, publicity and media industries that produce these representations and their effects"* (Turner 2004, 9). For Turner, celebrity is both a process and a product of media representation, a "*cultural formation*" with a "*social function*" (Turner 2004, 9). Celebrity, as a process and a product, connotes specific characteristics, too. Sociologist Chris Rojek considered celebrity "as the attribution of glamorous or notorious status to an individual within the public sphere" (Rojek 2001, 10). For Rojek, glamour and notoriety captured both the "favourable" and "unfavourable" forms of "public recognition" that are often, and sometimes simultaneously, connected with celebrity (Rojek 2001, 10). As Rojek argued, celebrity, whatever its attributions and affect, is something that is carefully constructed by the mass media, and it can come in different forms, too (Rojek 2001, 10–13). On the one hand, celebrity can be "ascribed" or "achieved," meaning that fame can come from lineage (i.e. Prince William and Prince Harry) or from accomplishments (i.e. Venus Williams and Serena Williams). However, celebrity can also be "attributed," and this happens when the media repeatedly represents an individual as noteworthy or outstanding (Rojek 2001, 17–18). In whatever way stardom manifests itself, scholars agree that the mass media is a crucial component in making the ordinary extraordinary.

While celebrity is associated with positives, like fame or fortune, it also carries negative connotations. Historically, celebrity has been associated with inauthenticity. Historian Daniel Boorstin, for example, offered one opinion of celebrity that, as Turner argued, continues to be one of the most well-known adages of celebrity today (Turner 2004, 5). In the 1960s, Boorstin described "the celebrity" as "someone who is well-known for their well-knownness" (quoted in Turner 2004, 5). According to Boorstin, the celebrity is not necessarily known for their achievements, but by their ability to publicly differentiate themselves from others via their personality (referenced from Turner 2004, 5). Boorstin argues that this differentiation is trivial and attributes the rise of the celebrity as a consequence of the inauthenticity of contemporary American culture (referenced from Turner 2004, 5). Here, celebrity is seen as superficial. It depicts the celebrity as a victim of trends going in and out of style, someone or something with temporary, rather than lasting, value. Indeed, celebrity in this sense of the term is a pejorative term. One researcher remarked on this tension between science and the limelight, explaining, "[…] [C]olleagues (especially outside, at least, the field, and maybe even inside, you know), kind of also look at you like that you're kind of a media whore" (7-00:43:45). However, Fahy's celebrity scientists, as well as Goodell's visible scientists, demonstrate that though this tension exists, celebrity is a much more complex concept than the adage of being well-known for being well-known allows. The search for DNA from fossils as a case study of a celebrity science demonstrates this. Here, individuals working in or around a celebrity science are not necessarily victims of stardom. Rather, they cultivate it and try to control it. They try to strike a balance between the expectations of their professional and public persona. In other words, celebrity does not have to be an exclusively dismissive or derogatory term. A celebrity science can make a virtue of this tension between authenticity and inauthenticity. But if there is this tension between science and the media, as highlighted in Chapter Two and Chapter Three, then why do scientists still seek the spotlight?

As argued in this thesis, ancient DNA activity surfaced from the interplay between paleontology, archeology, and molecular biology, but it also emerged, then evolved, under the influence of press and public enthusiasm. Ultimately, scientists from these disparate disciplines were interested in the theoretical preservation of DNA from fossils and its potential extraction, then analysis and application within evolutionary biology. This common interest, as well as common problem with concerns about contamination, brought these scientists together in discussion and towards the development of a discipline. Cooperation, initiated by common interests and problems, caused researchers to gather together, overcoming disciplinary boundaries in exchange for the founding a novel research program (Cain 1993). Yet the media also united and mobilized the field forward. While the public was interested in the idea of recovering DNA from fossils for the light it might shed on evolutionary history, they were also invested in the technoscience because of speculation that DNA one day may be used to bring back extinct creatures such as mammoths or dinosaurs.

In its emergence, scientists not only reached across disciplinary boundaries within the sciences but reached out to the mass media, too, enlisting their interest and influence to aid the development of the discipline. In this history, we see that scientists enlisted press and public interest specifically in the early and exploratory phase of their research program. In the late 1980s and early 1990s, scientists were motivated to seek the media to raise awareness of and investment in their research practice. Publicity was desired for legitimacy. Publicity was desired for its potential to attract further funding necessary for testing novel hypothesis about the preservation and extraction of DNA from fossils. The possibility and utility of DNA from ancient and extinct organisms was not a given, as evidenced by the US National Science Foundation's (NSF) rejection of Allan Wilson's proposal in the early-to-mid 1980s (Chapter One). In the late 1980s and throughout the 1990s, scientists enrolled the media to raise awareness and appreciation of their emerging practice. Crucially, this publicity helped gather researchers together across several scientific specialties, either in enthusiasm for or criticism against claims being made concerning ancient DNA authenticity. Publicity was also sought by scientists in light of controversial claims, namely the preservation of multi-million-year-old DNA (Chapter Two). The hype around *Jurassic Park* and practitioners' attempts to test the *Jurassic Park* concept, be it DNA from dinosaurs or insects in ancient amber, is an excellent example of this. Jack Horner and the NSF capitalized on the hype, aligning Horner's research and NSF's patronage of it within the *Jurassic Park* spotlight. Further, the UK Ancient

Biomolecules Initiates (ABI) consciously catered to popular interest through funding descriptions and decisions to support a study to test the viability of extracting DNA from insects in ancient amber (Chapter Two). Additionally, scientists sought the media to establish or reinforce partnerships. Pääbo's partnership between with Max Planck Institute of Evolutionary Anthropology and the commercial biotechnology company 454 Life Sciences Corporation is an example where publicity via press release and press conference established collaborations as well as resources to initiate the Neanderthal Genome Project (Chapter Three). In other words, big visions require big funds and sometimes aligning those visions with popular interests was one way to achieve or assure those funds. The media was critical in developing and defining ancient DNA activity as an autonomous area of research via branding and prolonged publicity. These are some of the moments when and reasons why scientists sought, and continue to seek, the media.

Yet the ways in which scientists seek the press is shared across the sciences. Literature in science studies has exhibited evidence of key moments and motivations of researchers to draw on press and public attention. Literature also suggests how media mobilizes researchers into action. Indeed, much of this literature suggests that the interactions between science and the media exist in or are exaggerated by times of controversy. Sociologist Elisabeth Clemmens outlined the debate around the death of the dinosaurs in 1980s, specifically debate surrounding the asteroid impact hypothesis, and how communication through the media facilitated, then furthered, scientific investigation into the hypothesis (Clemmens 1986; Clemmens 1994). Clemmens argued that popular interest in this topic catalyzed communication between different disciplines and led to increased research initiatives: "For astrophysicists, geologists, and geochemists, however, the link with a compelling question such as the death of the dinosaurs brought the promise of a new source of publicity, celebrity, and, perhaps, even greater funding (Clemmens 1994, 111). The fact that the press and public found the hypothesis plausible, or at least appealing, required researchers across distinct disciplines to address the evidence as professionals but within the media spotlight. Clemmens argued that because the debate was embedded in popular interest from the onset that this interest facilitated, even accelerated, research into the debate: "Our usual image of the sciences is of a congeries of institutionally separate disciplines, each governed by a particular set of practices, professional norms, and cognitive orientations. But, as the impact debate graphically demonstrate, popular culture can serve as a matric which fosters connections among disciplines that otherwise protect their institutional and intellectual autonomy" (Clemmens

1994, 119). Clemmens' study, like this history, demonstrates how interdisciplinary research around a scientific problem or even a new scientific program can be initiated, then sustained, by popular interest and investment. Attention from outside the boundaries of what scientists see as standard science, such as the media, can in fact bridge disciplinary boundaries especially in light of controversy.

Further, scholars have suggested that practitioners pursue alternative avenues of communication, such as the media, when legitimacy is at stake. Literature, for example, has argued that scientists "turn to the public" for legitimacy at the moment of making a controversial claim, at times of crisis, when there is competition or a desire for cooperation, and when in need of defining and negotiating boundaries of science (Bucchi 1996). There are several studies in which scholars have tried to identify when, how, and why scientists reach out to the public. Lewenstein's seminal study on the cold fusion controversy in the late 1980s and early 1990s, for example, highlights how scientists bypass conventional research and review norms to advance a controversial claim (Lewenstein 1995; Simon 2002). The discovery of cold fusion was not only announced in the press, but the controversy over its reality and means of replication played out in the press, too. Further, Angela Cassidy made a compelling case with evolutionary psychology for how popular science provided a special space outside the norms of academia for debate across disciplines as researchers tried to claim their expertise in this area of research (Cassidy 2005; Cassidy 2006). Felicity Mellor outlined how a select group of planetary scientists and astronomers actively advocated for the threat of an asteroid colliding with earth in the near future (Mellor 2007; Mellor 2010). She argued that they promoted the asteroid impact threat via evidence, narratives of technology-to-the-rescue, and appeals to the media in order to to confirm the legitimacy of their concerns as an important scientific issue. Research by Goodell, Fahy, and science communication scholar Jane Gregory further demonstrated the role of the individual in shaping public science and how stardom feedbacks into the shaping of science itself (Goodell 1977; Gregory 2005; Fahy 2015). In the case of genomics research, a case close to the history of ancient genetics and genomics, Stephen Hilgartner argued for the increasingly intense "media-orientation" of genome researchers during the days of the Human Genome Project (HGP) (Hilgartner 2012). For Hilgartner, "science-media coupling" was "strategic interaction" (Hilgartner 2012, 190). He argued that these genome researchers turned to the media in the face of competition to race to sequence the human genome: "The genome scientists were conscious of their behavior and orientation towards the media. They enlisted in the media at a time when competition was fierce. HGP leaders, for their part, arguably did what the managers of any enterprise would: they tried to react strategically to emerging events, seeking to acquire information, develop a short- and longer-term plans for responding, and tailor media messages that would defend their legitimacy" (Hilgartner 2012, 212). These studies showcase the science-media connection as a conscious, calculated, and growing phenomenon (Rödder, Franzen, and Weingart 2012). The connections between this history and evidence for the science-media connection as highlighted by science studies scholarship helps to draw out some of the more generalizable moments in which science and media interact. It also allows speculation about the ways in which a celebrity science may be distinct from a science in the spotlight at a given place and time.

While this science-media connection is evident across the contemporary history of science, this interplay is most manifest in the face of controversy at a given time and place. In other words, science-media interactions have been shown to be incredibly influential in shaping the practice of science, but these studies have shown this influence to be episodic. They have also shown the influence to be individualistic, specifically in how individual scientists use public platforms to advance research agendas. Controversy helps highlights the moments when, reasons why, and consequences of scientists seeking the media to promote their work, but the case study of ancient DNA research as a celebrity science demonstrates that the celebrity of the technoscience persists outside of controversy around the technoscience. The argument that the history of ancient DNA research can be characterized as a history of celebrity science is unique in that it argues that the entirety of ancient DNA's disciplinary development is the result of persistent science-media interactions. In this history, these science-media interactions are more than episodic and individualistic. Ancient DNA has been shaped by prolonged publicity over time. This public interest and media exposure certainly ebbs and flows throughout the ancient DNA's history from the 1980s to today. Indeed, some scientists were active agents in trying to distance the technoscience, or at least their part in it, from popular interest and influence. In their credibility contest over contamination concerns, researchers engaged, then disengaged with the race for the oldest DNA. But even this reaction was a response to media. Even the move away from multi-million-year-old DNA in exchange for a focus on less ancient but still scientifically significant specimens was a response to contamination concerns as well as celebrity. In other words, stepping on stage is just as important as stepping off or making a move in a way that redirects the media's gaze. Here, scientists shifted the attention away from the recovery of Cretaceous DNA to a focus on the analysis of DNA from other charismatic creatures such as mammoths, Neanderthals, and our ancient human ancestors. Even when *Jurassic Park* was not the star of the show, and even when controversy over contamination was not front and center, scientists found the hunt for DNA from fossils to be pervasive in the media and in the journals of *Nature* and *Science*. Here, "new" news values were created via the technology of NGS, the search for the first ancient genomes, and an anticipation of an understanding human origins, evolution, and migrations. Celebrity, its direction and to what degree it manifests itself, may change, but in a celebrity science the spotlight is not lost.

This is what makes a celebrity science distinct from a science in the spotlight at any given point in time – consistent appeal and constant attention over a prolonged period. First, celebrity in a celebrity science is not episodic or individualistic. The feedback between science and the media in the search for DNA from was not limited to a single event or individual in ancient DNA's history. Interactions can be, and often are, those things, but they must also be sustained over time even if they change in form or function. Although the science-media coupling has varied in intensity from time to time, the technoscience has remained in the spotlight with both researchers, reporters, and editors seeking one another's attention. This history has demonstrated that the press and public expectations have had a profound and prolonged influence on the shaping of the science. Second, it is not enough to be a science in the spotlight. Crucially, researchers have to respond to the spotlight. They are active agents in the publicity process whether their responses are positive, negative, interested, or disinterested. In a celebrity science like the case study in this thesis, practitioners in the spotlight actively cultivate and control celebrity. The making of a celebrity science is a process, the result of a relationship pursued by scientists and members of the media over time and in response to changing circumstances in the professional as well as public landscape.

It is important to note that this view of the celebrity science concept relies on two features of ancient DNA activity that may, in time, come to pass. One, the prolonged publicity of the technoscience is very much a result of the legacy of *Jurassic Park*. Generally, however, the popular appeal of the search for DNA from fossils is also very much a result of the less specific but just as speculative idea of resurrection. The prospect of bringing back an extinct organism – be it a dinosaur, mammoth, or passenger pigeon – is the ultimate fantasy-turned-to-reality that continually captures the public imagination. Here, the question is to what extent this vision is not just a contributing component to the technoscience's ubiquitous in the press but an essential element of ancient DNA's status as a celebrity science. In other words, once the fantasy of resurrection turns to a reality, will the search for DNA from ancient and extinct organisms continue to capture the popular imagination or will it cease in its celebrity? The importance of and answer to the question relies on recognizing that scientists' success in mobilizing the media for their purposes is very much dependent on timing. Massimiano Bucchi, science communication scholar, explained, "The 'turn to the public' is not only dependent on the will of the researchers and on their convenience. […] The public level can be more or less easily mobilized, depending on the intrinsic public resonance of the issue at stake (and therefore, on the chances of linking it with issues that are already prominent), or on the visibility of the scientific actors and institutions sponsoring it, or, again, on the relations (e.g. in terms of autonomy and visibility) between a scientific field and the public at a given historical time" (Bucchi 1996, 383–384). On this note of timing, another question arises.

Given more time, will researchers involved in ancient DNA activity seek the media less and less? There are three questions that follow from this: Does the amount of attention that an issue or subject of science receives from the media correlate to its status as a young or more mature area of research? If so, does the presence or absence of media, including the degree of interest and influence, say something about the scientific maturity and legitimacy of a discipline? Or can continued exposure of what seems to be an established science be sure evidence of a celebrity science? Bucchi argued that scientists often seek the media when defining and negotiating scientific boundaries. He also argued that this is a common and strategic tactic in the early or exploratory stages of research: "However, public support is particularly necessary when what is at stake is not just the negotiation (however massive) of the boundaries but rather their constitution. While a discipline is as yet unrecognized as such (and while it therefore lacks authority and prestige), it is essential that researchers prove the relevance of their work for society at large" (Bucchi 1996, 382). This history supports this statement. But Bucchi speculated that publicity will wane once authority has been achieved: "Once a discipline has been established and recognized as such, the public no longer plays a constitutive and fundamental role. Public legitimation of science begins then to rest precisely in its autonomy and indifference to public questions and curiosity" (Bucchi 1996, 383). The question for investigation in terms of the future of ancient DNA activity is this: Specifically, in this case study of celebrity science, how will researchers' enrollment of the mass media change as the discipline develops towards increased institutionalization or stabilization of as an autonomous area of research? Generally, must celebrity extend beyond emergence of new research programs as a generalizable feature of a celebrity science? These questions are open to investigation. Nonetheless, what is certain is that interviews with scientists today suggest a clear and continuous appreciation of press and public expectations of news values. These interviews suggest that scientists will seek the mass media in years to come and for a number of reasons.

4.3.3 Implications for Contemporary Science Communication

Ancient DNA's history offers the opportunity to trace the development of a discipline in a world of modern media, celebrity culture, and at a time when science and science communication expectations are evolving. The Public Understanding of Science (PUS) Movement, initially inspired in the UK in the 1980s, was a systematic endeavor to increase public awareness and appreciation of science and technology.[44] The idea was that improving scientific literacy would encourage public and political support of science and technology (Bodmer 1985; Gregory and Miller 1998, 1–18; Broks 2006, 96–117). The PUS Movement originated in the UK in 1983 when Walter Bodmer, Director of Research at the Imperial Cancer Research Fund, chaired a committee of scientists to review the current standing of the public's understanding of science. The Royal Society endorsed the meeting and it resulted in a report published in 1985 which played a profound part in mobilizing the movement (Bodmer 1985). As a result of the report, the Committee on the Public Understanding of Science (COPUS) was created. COPUS trained scientists to work with journalists and other media outlets, rewarding them for their efforts to engage the public. The report also stimulated the Economic and Social Research Council (ESRC) to fund a research program and journal dedicated to the systematic study of the public

[44] This section is not an exhaustive historical explanation of the PUS Movement in the UK or equivalent movements elsewhere. However, the US also took similar steps towards improving public understanding of science and technology. In the US, the American Association for the Advancement of Science (AAAS) initiated "Project 2061" with the goal of increasing science literacy, especially science education, across the country. "Project 2061" promoted science education and rewarded researchers and reporters for communicating scientific and technological studies to the public. Jane Gregory and Steve Miller wrote, "The decade since the establishment of COPUS in Britain and Project 2061 in the United States has seen many new ventures in the broad field of the public understanding of science in many different forms around the world – indeed they amount almost to a public-understanding-science-of industry, which is colonizing small corners of academia, commerce, and politics and generating its own momentum" (Gregory and Miller 1998, 7). In this thesis, I only mention the PUS Movement in the UK, mainly because it was the initial impetus behind popularization of science and technology at this time. However, if the PUS Movement is to be fully understood as the context for which a celebrity science like ancient DNA research could have, and indeed did, evolve then attention to and analyses of the effects of worldwide movements in science communication is necessary. In this thesis, it is also interesting to note that ancient DNA researchers from different countries (UK, US, and Canada to Germany, Copenhagen, and Australia) face different political pressures, as well as science and science communication traditions. However, these ancient DNA researchers all feel the pressure to publicize their research to the public through the press. Perhaps this is because many ancient DNA researchers are seeking high-profile publications in high-profile journals like *Nature* and *Science* that come from Anglo-Saxon traditions and are influenced by the UK and US science communication movements. The extent of these effects require further research.

understanding of science through surveys and other methods (Gregory and Miller 1998, 19–45). The PUS Movement required and rewarded scientists to be public-facing scientists. As science communication scholars Jane Gregory and Steve Miller wrote, "In the last decade or so, scientists have been delivered a new commandment from on high: *thou shalt communicate*. In the recent past, many scientists looked at involvement in the popularization of science as something that might damage their career; now, they are being told by the great and the good of science that they have no less than a duty to communicate with the public about their work" (Gregory and Miller 1998, 2). However, their ability to communicate on a public platform was also aided by other developments like the "commercialization" of the mass media and more recent movements in "mediatization," "medialization," and "celebrification" (Burnham 1987; Golinski 1992; Evans and Hesmondhalgh 2005; Broks 2006; Nelkin 1995; Rödder, Franzen, and Weingart 2012; Dunwoody 2014).[45] These developments, taken together, suggest a setting through which to understand the phenomenon of celebrity science and its implications for contemporary science communication.

With pressure to popularize, researchers quickly realized that the idea of de-extinction – one that often involved *Jurassic Park* – was an easy entry for talking to the press about their real research. This public fascination with resurrection always brought journalists back to these scientists: "I would say probably at least 50% of the time whenever I'm talking to anyone in the media they always ask that question: 'Can we bring thylacines or dodos or mammoths or whatever back to life again?'" (25-00:58:45). Students are often asked this question, too. One student said, "It's the first question people ask you. Honestly. I've been asked that so many times" (GI-2-00:10:20). A second student called it "the *Jurassic Park* effect" (GI-2-00:10:00). One interviewee put it this way: "There's always going to be some level of celebrity science around trying to recreate extinct species" (25-00:54:20). This interest put this community in the spotlight, and also attracted particular personalities interested in being in the spotlight: "I think it's a crowd that's in the limelight. I think it's a crowd that's multidisciplinary. I think it's a crowd that's used to having to

[45] The term "mediatization" refers to the increased presence and power of digital media devices in everyday life, whereas the term "medialization" refers to the closer coupling of science and mass media. See page 5 in Rödder, Franzen, and Weingart (2012). The phenomenon of "celebrification" references the process by which the mass media makes an individual into a celebrity. See page 12 in Evans and Hesmondhalgh (2005). There are several studies of the relationship between science and media that are particularly relevant to research in genetics and genomics. Also see Martina Frazen's "Making Science News: The Press Relations of Scientific Journals and Implications for Scholarly Communication" (2012), Stephen Hilgartner's "Staging High-Visibility Science: Media Orientation in Genome Research" (2012), and Simone Rödder's "The Ambivalence of Visible Scientists" (2012).

speak to people that are outside of a lot of what they do. And I think it's a crowd that kind of *likes* the idea that it's got a lot of media attention, and therefore, has a lot more practice at it" (22-01:49:00). For example, "I'm sure that there are amazing people in *Arabidopsis* and *Drosophila* who are equally good communicators, but they don't get the opportunities because who the fuck cares about *Arabidopsis*?! (At least relative to dinosaurs or human genomes or dogs or whatever.) […]" (22-01:49:00). But it is not just the media that understand the power of news values. Scientists, research institutions, and research journals also acknowledge its advantages, and this plays a real role in the process of science.

According to interviewees, journals like *Nature* and *Science* understand the significance of news values and pick what papers to publish with this in mind. Indeed, *Nature* and *Science* have historically tried to cater to both a professional and popular audience (Chapter One) (Kohlstedt 1976; Kohlstedt, Sokal, and Lewenstein 1999; Baldwin 2015). The concept of news value has affected, and continues to affect, their process of publishing. One interviewee presented this perspective: "On the one hand, it makes it very high-profile. It is presumably very much in the mind of *Nature* and *Science* editors when they are considering to accept a paper or not. How much media attention are they going to get and therefore, how many copies are they going to sell? And what [are the] citation indexes of the paper going to be and therefore, where their journal sits? That's the sole motivating – well, not sole motivating factor – but significant motivating factor" (32-01:11:00). To illustrate this point, another interviewee added, "If I went off and sequenced genomes of three animals in Australia, add some level of hybridization in the past, it would be interesting to me and interesting to a few evolutionary biologists around the world, but it wouldn't be newsworthy or not media-kind-of-newsworthy." Conversely, "But if you do it on modern humans, Neanderthals, and Denisovans – because two of them are extinct (one was meant to be a cave-dwelling thug and the other one no one even realized existed) – then that in itself makes it high profile and therefore, it creates greater interest and therefore, greater funding into that kind of research." In other words, "It's like a self-perpetuating system" (25-01:21:45). The publishing practices of journals such as *Nature* and *Science* feed back into practitioners' decisions about what science to pursue and where to publish their research results.

Scientists in a celebrity science like ancient DNA research often tailor the production and presentation of their work to target these journals. One scientist told this story to illustrate this point: "I was collaborating on a *really* nice ancient DNA project – good project, good results. They said, 'We're sending this to *Nature*.' I said, 'It's not a *Nature* paper. You're wasting your time.' I said, 'Send it to such-and-such journal.' 'Nope! No!' It went to *Nature*: rejected. Then they tried, I think, PNAS ('Previously Submitted to *Nature* and *Science*'): rejected. [Smiles]. And eventually it ended up in the journal I'd first recommended. [Laughs]." This scientist said, "[T]he top journals […] are *almost* the link to the popular media. If you look at *Nature*, it is more than a science journal. […] Although because they do publish high-level science, they also like a damn good story. *They do*. You know, *short* papers with a *punchy* headline." Further, "So, I think the attempt to get their work into these top journals, repeatedly (with a lot of success I might add in some cases), to some extent colors people looking for what you might call 'sound-bite-research.' 'Let's sequence that hominid.' You know that's going to be a *Nature* paper if that's got DNA in it" (3-01:34:10). In other words, news value affects the process of science and science communication. As science scholar Martina Franzen noted, these top journals, because of their link to the mass media, tend to favor spectacular or surprising results (Franzen 2011). Likewise, science scholar Peter Weingert said, "The link of top journals such as *Science* and *Nature* to the mass media by way of pre-publication press releases and related promotional activities that play to the news values of novelty and sensation has an impact on the communication process" (Weingart 2012, 29). Here, news values, as determined by the media, and journals like *Nature* and *Science*, filter back into scientists' decisions concerning the production and presentation of scientific knowledge. In a celebrity science, celebrity can shape science.

However, the top journals, while they are the link to the popular press, are also a link to public and political bodies that make decisions about funding. Researchers do not just tailor their work to attract public attention for attention's sake. They interact with the press to communicate that their research is worthwhile and deserving of further funding. This self-perpetuating system of high-profile publications leading to high-profile press, which then might lead to further funding, has created a sort of scientist that is skilled in communicating to the media. One practitioner mentioned that marketing is key: "Strategically thinking about how to package science into big picture questions that will get high-profile publications and/or grant funding. It's strategic thinking or writing […]. Your ability to sell, sadly, has become a key determiner, not just good science. I would argue, in fact, that your ability to sell has actually become more important than your ability to do the science, sadly, given the way that funding has actually gone" (32-01:21:00). Another researcher remarked, "I don't know if everyone else talked about funding, but funding is a huge issue, and I think, unfortunately, it actually really shapes the research that gets done because a lot of people try to chase the trends to try to capture the funding to then get their research done" (27-01:48:20). Even in a celebrity science, the spotlight moves on. But when the spotlight moves on, the scientists move with it.

Practitioners working within a celebrity science like ancient DNA research are often media-savvy scientists when it comes to communication. They predict and play to the trends. Current research in ancient DNA research is evidence of this. For example, in 2005, NGS changed the scale and scope of the technoscience, shifting the search for ancient DNA to the search for ancient genomes: "If you don't have a genome, you don't get into *Nature* or *Science* these days. I'm not saying that's the only reason, but you could say, quite cynically, that's one of the main drivers" (32-00:58:30). In this new era of exploration, researchers raced to sequence the first ancient genomes of the Neanderthal, Aboriginal Australians, and early humans from hunter-gatherers to farmers: "[...] [S]everal really big names in ancient DNA, they jumped onto the human train. […]. I guess, if they had all decided to work on megafauna, there would have been a bunch of papers already. It's coming, as soon as *Science* and *Nature* get tired of yet another ancient human genome paper. […]" (38-00:34:00). Crucially, it is not just journals that set these expectations. Scientists play a real role in creating and perpetuating these trends: "[…] There's almost now an expectation that if you work in ancient DNA you should be sequencing whole genomes and stuff, which I think is a bit weird because that's all being driven by two or three labs who have almost an infinite supply of money – sorry, yeah, an infinite supply of money – so they can do that kind of stuff. Whereas the rest of us mere mortals can't necessarily – [laughs] – go off and sequence the genome of something because we feel like it" (25-00:23:00). Although researchers recognize that these trends and the attention associated with them may dictate the direction of research, some still hold that the science comes first while the media comes second. An interviewee presented this perspective, "I mean, I'm here for the *science*. I mean, this is the first thing. […] [T]he *media – all media* – is a secondary thing" (7-00:44:00). Nonetheless, expectations are co-constructed by researchers, research journals, and press and public interest.

This pressure to find funding is creating a type of scientist who has learned the language of the media regarding news values. These people are not necessarily celebrity scientists or even visible scientists. Rather, in a celebrity science, there are scientists who have become media-savvy scientists as a consequence of the public interest and media exposure that has followed the field. However, these media-savvy scientists do share similarities with celebrity scientists or visible scientists in their ability to adopt current research trends and adapt when those trends change. Goodell noted a similar situation in her case study of visible scientists: "In short, dramatic changes in science and in communication are forcing changes in science communication, and, in the process, in the kind of scientist who gets communicated. […]. And if media continue to evolve as predicated, visible scientists evolve with them" (Goodell 1977, 6). In other words, science and media co-evolve, influencing one another: "To a certain extent, this means the visible scientists are a product of media fads. Topics move in and out of vogue, and with them the scientists associated with them" (Goodell 1977, 19). However, visible scientists, celebrity scientists, or media-savvy scientists are not just victims of fads. When the spotlight moves on, these scientists know how to move on with it. Goodell explained, "Timing is important not only in getting on the bandwagon but also in getting off. Deliberately or instinctively, scientists with lasting visibility and influence have moved from one issue to another with shifts in public interest. They are on the next peak before the previous one becomes a trough" (Goodell 1977, 19). Although these scientists are not necessarily visible scientists or celebrity scientists, ancient DNA researchers embody certain characteristics that make them and their research attractive to the media spotlight. In "Charles Darwin as a Celebrity," Browne considered these characteristics, explaining, "Every celebrity has possessed, to various degrees, ambition, action, achievement, and talent, and these are surely qualities that also drive scientists and creative artists" (Browne 2003, 176). These media-savvy scientists are good at guiding press and public attention regarding their research, facile at following the trends, and skilled at connecting back to ideas or interests that hold an apparently timeless appeal to the public.

In general, media-savvy scientists understand the differences between scientific and journalistic practices but try to balance the two, especially when disseminating their research results to the public. For example, the connection between the search for DNA from fossils and de-extinction is not necessarily a link in the technoscience itself, but is a link mostly made by the press in its reports to the public. Yet both scientists and journalists engage with the idea for the pragmatic purpose of linking their professional research to a popular reference to get their messages across to a wider audience. In reference to resurrection, one scientist said, "[…] [T]here's two ways you can approach that. As a scientist responding to those kind of inquiries, it's very easy to slam the reporters and say, 'Look. This has absolutely no connection. I don't know why you keep bringing this up.' I don't find that serves my benefit or the benefit of the general public *ever*. […]" (33-00:14:30). Further, "It's *very easy* to slam it and it's *very easy* also to jump on it […]. The question is whether you can have a meaningful discussion in the interstitial spaces between those two extremes" (33-00:16:30). As science communication scholar Sharon Dunwoody explained, journalism, including science journalism, revolves around "traditional news pegs" like "timeliness, conflict, and novelty" (Dunwoody 2014, 32). She further explained, "This reliance on news pegs also means that coverage of a long-running issue waxes and wanes with the presence/absence of pegs" (Dunwoody 2014, 32). Consequently, science in the news is more episodic while science in practice is a prolonged process of experimentation, success, and failure. This disjunction between the practice of science and its presentation in the media is a tension for some scientists. Yet other researchers recognize the difference in science and media expectations, accommodating both accordingly: "While the disjunction between coverage and process can be disconcerting to some scientists, others have learned to take advantage of reporter dependence on news pegs and have become facile at guiding coverage" (Dunwoody 2014, 32). But news pegs affect more than science reporting; they also affect how and what science gets published, playing into this self-perpetuating system in which high-profile publications lead to high-profile press which might lead to further funding. This system can shape the sort of scientist working in, or who wants to work in, a celebrity science context. As one researcher remarked, "It's producing a weird type of scientist. I would say a business type of scientist, right, who kind of knows how to function in this environment. […] The people publishing in *Nature* and *Science*, even if it's a low hanging fruit, they will be the one more likely to pick up a position at a university because, again, the university wants a researcher who produces media attention" (37-01:20:00). Here, it comes full circle to the science communication movement. Changes in science communication are also affecting the expectations and evaluation of scientific success today.

Today, scientists say funding is far and few between. Consequently, some see de-extinction as one way researchers can try to marshal resources for research. An interviewee offered this opinion: "I think de-extinction has proven to be very popular and interesting to the public, and it's also a lightning rod – maybe lightning rod is an exaggeration – is a point in which some investors might want to put some money in, and I think that kind of highlights just how underfunded we've become and scientists are reaching out to alternative funding sources. […]" (27-02:31:25). This interviewee explained, "It is like *Jurassic Park* playing out in real life, but it's also kind of like a means to an end. So, the real value of the human genome project wasn't really getting the human genome. It was all the technologies that came out of it. It was all of the additional things that developed as a result of having a lightning rod to focus money on to get things done" (27-02:33:45). This interviewee illustrated this point, explaining, "[…] So, I think Pleistocene Park is much more realistic than *Jurassic Park*, and people love charismatic megafauna. There is a reason why the panda is the lead organism on the World Wildlife Fund. People love big charismatic megafauna and what is more charismatic than a cute, cute woolly mammoth?" (27-02:36:00). In 2006, a journalist stated something similar: "For all its charisma, the mammoth is just part of a grand new strategy to restore long-gone megafauna. Scientists call it rewilding. The idea goes hand in hand with new thinking about the relationship between humans and nature – namely, that even the earliest civilizations had what we might think of as an unnatural impact on the natural world around them" (Margolis 2006).

Shapiro's *How to Clone a Mammoth* seems to suggest this point. While Shapiro is critical of de-extinction, she indicates that the goal is not just bringing back a mammoth, a passenger pigeon, or other organism for its own sake. Instead, Shapiro says the overarching objective of de-extinction is the revitalization and stabilization of ecosystems (Shapiro 2015). Indeed, it does seem that de-extinction is a lightning rod for some scientists who want to address broader environmental and ecological issues about past and present ecosystems. In the 1980s and 1990s, researchers were likely to dismiss de-extinction as science fiction. Today, however, this has changed and some are reconsidering resurrection as a technoscientific possibility. As one scientist said, "De-extinction is basically coming from academia […]" (20-01:57:25). For this interviewee, TEDxDeExtinction united disparate academic disciplines from paleontology, conservation biology, and synthetic biology with projects interested in cloning, rewilding, and genetic engineering. The crosstalk between these different disciplines, and the involvement of respected researchers, suggested that although de-extinction faces technological, biological, ethical, and political challenges, it is an idea that is at least worth listening to and learning something from in an academic context. However, the celebrity around the idea of resurrection was far from lost. Reflecting on TEDxDeExtinction, this same scientist said, "They're all coming from their perspective and looking for common ground at this conference, *National Geographic* […], which was a media enterprise all by itself. That's something important to realize. They weren't doing this necessarily out of the goodness of their heart or to advance the science. Yes, that was part of it, but they wanted this special issue out of it and they certainly got it. […]" (20-01:57:25). It could be that de-extinction, with some serious science behind it, is in a prime position to become a technoscience and celebrity science in its own right.

However, fame can come at a cost. Scientists in the limelight are often criticized by colleagues who say their science is compromised by the media. In the ancient DNA community, balancing celebrity with credibility was, and still is, an important issue. One researcher remarked on the negative and positive effects of media participation: "Amongst colleagues, they said, 'Oh, that is the guy that is going to TV. He can't do proper research.' But in the end, I think, it helped me and other programs helped people quite a lot to explain what they do and in order to get funding because that *is* the relation even if people deny it and say, 'Oh, we only do proper science in the lab.' It is. […]" (14-00:25:00). A second scientist presented this point: "I'm sure they – many sciences – have this belief that the more media attention you get the more money you get. I question that. I *really* question that. But I think most sciences, as well as most people out in the normal community, [have] that view, and therefore, it's a mixture of jealousy and seeing these people like me as a media whore, right? I'm just selling out to get more money, right?" (7-00:49:45). Despite the problems that public-facing scientists face, there is a still an incentive to seek the spotlight. Dunwoody wrote, "Such visibility can be harmful, as many *burned* scientists still ruefully report, but the social and scientific legitimacy that can attend such visibility is luring many scientists into acquiring greater communicative expertise" (Dunwoody 2014, 35). This same scientist explained the drive to publicize despite collegial criticism: "[…] [I]f I thought that it would have no effect on my possibilities of getting another *Nature/Science* publication, at all, I would probably say 'no' to participating in the media. […]." Further, "So, in other words, if I stopped communicating when I got a *Nature* or *Science* paper and said, 'Well, I really don't want to talk to anybody,' then I'm sure that would affect my chances of getting another *Nature* or *Science* paper. So, therefore, I'm kind of forced to do something […]" (7-00:55:00). As another interviewee added, "Sometimes the research is comprised by the media" (13-01:010:35). However, for this interviewee, this is all part of that self-perpetuating system: "I think it's a little bit dangerous that somehow the media have a big influence on the direction of research, not necessarily on the results but what is interesting to do because it sells so well. […] Even with the more intellectual higher-ranking journals like *Science* or *Nature* and so on. […]" (13-01:11:00). This system affects all science: "You are a bit forced, sometimes, to publish data premature. […]. I mean, we are living in a capitalistic system and science is connected to it. It's not completely independent of it. So, we all need the money to do our research and to have our own positions safe and so it will always be compromised in some way" (13-01:12:45). This tension between science and the spotlight is not unique to the search for DNA from fossils, but shared amongst sciences.

The assumption that quality of research is disproportionate to press and public exposure is not new. Goodell's and Fahy's accounts of visible scientists and celebrity scientists highlighted researchers' successful, and not so successful, attempts to navigate between both worlds (Goodell 1977; Fahy 2015). The life of Carl Sagan, as highlighted by science scholar Michael Shermer, is an excellent example. Shermer said, "So famous did he become that a 'Sagan Effect' took hold in science, whereby one's popularity and celebrity with the general public were thought to be inversely proportional to the quantity and quality of real science being done" (Shermer 2002, 490). Here, the assumption is that quality science and media cannot coincide without one or both being compromised.

This is a tension shared by stardom more generally. Turner offered several scenarios to suggest this: "Indeed, the modern celebrity may claim no special achievements other than the attraction of public attention; think for instance, of the prominence gained for short, intense periods by the contestants of *Big Brother* or *Survivor*, or even the more sustained public visibility of Kim Kardashian. As a result, and as the example of Kim Kardashian might suggest, most media pundits would argue that celebrities in the twenty-first century excited a level of public interest that seems, for one reason or another, disproportionate" (Turner 2004, 3). While this may be the case, I suggest that the real reason behind this tension is that in a new era of what science scholars have identified as an era of "medialization" – the closer coupling of science and media – academics do not have a method to measure scientific success in a way that accounts for both professional and public standing.

However, in 2014, Neil Hall, a geneticist at University of Liverpool, published a piece titled, "The Kardashian index: a measure of discrepant social media profile for scientists," that tried to quantify the public profile of a scientist by comparing his or her number of Twitter followers to their number of publication citations (Hall 2014). Hall explained, "I am concerned that phenomena similar to that of Kim Kardashian may also exist in the scientific community. I think it is possible that there are individuals who are famous for being famous (or, to put it in science jargon, renowned for being renowned)" (Hall 2014, 1). Hall did not disparage scientists for seeking the spotlight, but he did seem to suggest that in this age of medialization or celebrification that science needs some sort of measurement for assessing a researcher's professional and public persona: "I don't blame Kim Kardashian or her science equivalents for exploiting their fame, who wouldn't? However, I think it's time that we develop a metric that will clearly indicate if a scientist has an overblown public profile so that we can adjust our expectations of them accordingly" (Hall 2014, 1–2). Although comical, this was also a serious statement about the close connection between science and media, including its impact on the practice of science.

The ever closer connection between science and media, coupled with pressure to popularize their research, is a challenge to scientists who want to communicate to the public through the press while simultaneously sustaining authority over their message regarding its presentation and interpretation across audiences (Chapter Two and Chapter Three). While researchers may want to legitimize their research via the mass media, they are also concerned that publicity may compromise their credibility (Chapter Two Section 2.4.3). Peter Broks, cultural studies scholar, put the predicament this way: "To maintain its authority it needs to be set apart from the general public, but to maintain its legitimacy it needs to appeal to the general public. Being set apart increases its alienation; making it more 'popular' undermines its authority" (Broks 2006, 107). Broks argued that the science communication movement of the 1980s was an attempt to legitimize popularization, giving scientists a professional initiative and incentive to appeal to the public through the media.

However, Broks also argued that this movement to legitimize popularization was just as much a move to legitimize science at a time when public support of science and technology (intellectually and financially) was in decline. This was a consequence of the professionalization of science in the second half of the nineteenth century with individuals' and institutions' attempts to set themselves apart from the public in order to establish their authority within society. Now, however, to legitimize the social standing of their science, scientists and their scientific institutions must once gain appeal to the public (Broks 2006, 107). Broks said, "In other words, the problem lies in the expectation that there can be some measure of control over the meanings of an idea once it is placed in the public domain" (Broks 2006, 149). Dorothy Nelkin put it this way: "While they want their work to be covered in the press, they are constantly concerned about how it is covered, and this concern has led scientists and institutions not only to promote science through public relations, but also to control journalists' access to information" (Nelkin 1995, 145). One researcher specifically remarked on this challenge, "[…] [W]hen you put out a publication you are in so much control of it. […] [U]ltimately, you control your product […].” However, “[…] [W]hen you work with the media that’s a huge wild card and you have *no* idea what they’re going to do or say.” Nonetheless, this practitioner argued that communication is critical: “But I think that as difficult as it can be sometimes, it’s really, really important because ultimately we are paid out of taxpayer funds and if they don’t see the importance of what we’re doing they aren’t going to fund us. […]” Further, “That’s become easier and easier to do because the product we’re generating does have intrinsic, really high, interest and we are making all these findings and I think that’s helping a lot. […]” (27-01:55:45). In different domains there are often competing or conflicting values. In 2009, *Nature* stressed this strain: “In principle, there is no reason why science should not be accompanied by highly proactive publicity machines. But in practice, such arrangements introduce conflicting incentives that can all too easily undermine the process of the assessment and communication of science” (*Nature* 2009, 484). In this world of changing science communication strategies, it is imperative for researchers and reporters to try to strike a balance. This case study of celebrity science through the search for DNA from fossils offers an opportunity to observe the process of science and science communication in action.

4.3.4 Conclusion

This section outlined the celebrity science concept in terms of its definition, essence, and process. I argued that the mass media is necessary in the making of a celebrity science because they seek the science for its news value. However, the media is not enough. The second and most important part of the making of a celebrity science is that researchers participate in the process as well. In a celebrity science, researchers respond positively and negatively to the media attention, and even reinvent their reputation and that of the technoscience accordingly. Ultimately, a celebrity science is the result of a relationship that is actively pursued, then produced, by both scientists and the members of the media. In other words, the making of a celebrity science is an active, not a passive, process. To define and develop this new celebrity science concept, I drew on Fahy’s celebrity scientist concept as a preliminary starting point for thinking about celebrity in science in general, and for thinking about how celebrity can operate on a group level, not just an individual level. Here, it is important to note that celebrity, in my use of the term, is not a pejorative term. Rather, individuals working in or around a celebrity science are not necessarily victims of stardom; they cultivate it, control it, and try to strike a balance between the expectations of their professional and public persona. In a celebrity science, researchers try to take this tension between the two and turn it into a virtue.

This section also argued that the celebrity science concept has implications for contemporary science communication. The phenomenon of celebrity science is best understood in context with the PUS Movement, initiated in the UK but influential in the US and elsewhere. It must also be understood in the context of other movements like the "commercialization" of the mass media, "mediatization," "medialization," and "celebrification." With pressure to popularize, and public platforms for which to do so, researchers realized that the idea of de-extinction was an easy entry for talking to the press about their real research. But news value affects more than science reporting; it also affects how and what science gets published. One interviewee described this as a self-perpetuating system, where newsworthy studies that make high-profile publications lead to high-profile press, which leads to further funding. According to some scientists, this system has created a sort of scientist skilled in packaging their research to both scientific journals and journalists. The result is a media-savvy scientist who has learned the language of the press, including differences between scientific and journalistic expectations and practices when it comes to reporting research. Indeed, de-extinction, with some serious science behind it, could be one way some scientists are marshaling interest towards a future field of study. Celebrity affects the process of science and science communication. But there is a tension between science and the spotlight, as researchers can be criticized by colleagues who argue that science is compromised by the media. While this can be the case, I also argue that this tension can be understood as a result of the closer coupling of science and media.

4.4 CONCLUSION

In this chapter, I explored the close connection between the search for DNA from fossils and the idea of resurrecting extinct creatures such as dinosaurs and mammoths. I explained the connection between the two as a link not necessarily in the technoscience itself, but one made by the media in attempts to connect professional research to a popular reference, namely *Jurassic Park*. However, some scientists do engage with the idea of de-extinction because they understand the value of news value when communicating to public and political audiences for funding. In the 1990s, this helped generate interest and guide activity at a time when the technoscience was first emerging. The idea of resurrection played a part in its evolution into a technoscience and celebrity science. However, ancient DNA researchers' interest in de-extinction is changing as technology is changing. NGS, CRISPR, and influential individuals like Church, Shapiro, and Hendrik Poinar have caused the community to reconsider resurrection. These interviewees' past reflections and present reactions to de-extinction embody a timeline of evolving expectations regarding the technoscience's relationship with ideas of resurrection.

The link between ancient DNA research and de-extinction highlights the making of a celebrity science as a concept in terms of its definition, essence, and process. I argued that a celebrity science is the outcome of prolonged publicity advanced by a relationship that is actively pursued and produced by both scientists and members of the media. This case study of celebrity science demonstrates that news values affect more than science reporting. Here, news values affect editors' decisions about what science is published. This self-perpetuating system highlights that when it comes to producing and presenting science, celebrity can be an integral part of the process. However, researchers do not solely seek celebrity for a chance to be in the spotlight; they seek it because there is an expectation for them to popularize their research to the public through the press. The science communication movement and other developments towards "mediatization," "medialization," and "celebrification" have set the stage for the intense interplay between science and media. Here, we see that researchers are responding to the call to communicate and that their position within a celebrity science gives them opportunities to do so. In tracing the development of a discipline like ancient DNA research, its evolution into a celebrity science captures the consequences of the ever-closer connection between science and media, including its influences on the practice of science and science communication. In the ancient DNA community, balancing celebrity with credibility has been an important issue. This history demonstrates the tension between science and the media limelight, but also how the two can coexist, then coevolve with one another.

CONCLUSION

Main Contributions

This thesis has provided the first academic historical account of the search for DNA from ancient and extinct organisms and the first account of the celebrity science concept. This microstudy and disciplinary history of ancient DNA research is a contribution to the historiography of science with clear implications for understanding the broader process and practice of contemporary science today. Ancient DNA's history highlights the role of popular culture in the development of a discipline, spotlighting the fact that media can and does influence the practice, production, and communication of scientific knowledge over a prolonged period of time. This history demonstrates that the media is not just influential in how a single controversy, event, or episode in science plays out, but it demonstrates that it can have a sustained impact on the entire development of a discipline. This research offers both scientists and science scholars the opportunity to reflect on the role that the media and the contemporary science communication movement have on the practice of science itself.

Chapter One introduced a new narrative of the search for DNA from fossils, recovering information – ideas, instruments, and individuals – necessary for an integrated history of the technoscience. Focusing on the late 1970s and early 1980s, I revealed that the idea of extracting DNA from fossils, specifically from insects trapped in ancient amber, was pursued by different people in different places around the same time. I also revealed that from its beginning, the search for DNA from fossils was closely connected to the idea of resurrecting extinct species. In the beginning, speculation and spectacle played a part in turning ideas into experiments, then eventually into evidence for the theoretical preservation of DNA in fossil material. The interplay between science, speculation, and spectacle in the early days was central to the emergence of ancient DNA research. Appreciating the historically and intellectually close connection between the idea of extracting DNA from fossils and speculation about bringing back extinct species such as dinosaurs and mammoths – as well as the public interest in both these ideas – was necessary for understanding how and why the technoscience evolved in the way that it did.

Chapter Two focused on conceptual and technical evolvements from the late 1980s to the late 1990s. Specifically, I focused on how a growing group of researchers tested and imposed limits in the hunt for the first or the oldest DNA from paleontological, archeological, and botanical specimens with the new technology of the polymerase chain reaction. Technology accelerated this era of exploration but so did professional and popular interest in the book and movie *Jurassic Park.* As the search for ancient DNA evolved, it did so under intense public interest and extreme media exposure. The press created opportunities for publicity which scientists could, and often did, take advantage of for the pragmatic purposes of obtaining further funding for research. Scientists fashioned their own opportunities for publicity, too. But publicity could be problematic, especially in light of contamination concerns which challenged the credibility of technoscience. According to some scientists, popular interest and influence was a second source of contamination. Overall, media both helped ancient DNA's disciplinary development, but according to practitioners it simultaneously hindered its acceptance as an authentic activity within evolutionary biology. The key point made was that feedback between science and the media contributed towards the co-construction of ancient DNA research into a technoscience and a celebrity science.

Chapter Three outlined the technoscience's disciplinary development from the turn of the century to today. I analyzed how a handful of practitioners produced a strict set of scientific standards for how to properly practice the search for DNA from fossils at a time when the technoscience's credibility was contested. In light of skepticism regarding ancient DNA authenticity, a group of researchers tried to standardize the practice via criteria of authenticity. Replication became a measure of experimental expertise and credibility. While initially intended to reduce controversy regarding authenticity, criteria effectively engendered controversy within the community. In this chapter, I also explained how these standards, designed around the technology of the polymerase chain reaction, were challenged by the innovation of a new technology called next-generation sequencing. Ultimately, criteria of authenticity were a response to contamination concerns, but criteria were also a response to celebrity. In other words, contamination of research results and "contamination" by the media – according to some scientists – caused scientists to try to control the reputation of their work and of the technoscience as a whole. Scientists, in response to these concerns, engaged in boundary-work to try to demarcate credible from less credible research. This boundary-work was evident through researchers' activities, as well as their memories of their history. This boundary-work, in response to contamination concerns and celebrity, was a crucial component to disciplinary development, influencing how scientists tried to make sense of the status of their field.

In Chapter Four, I outlined researchers' reactions to the idea of de-extinction to understand how the search for DNA from fossils is linked to resurrection research. Here, the link between the two is not necessarily a link made in the technoscience itself, but a link made through the media via the legacy of *Jurassic Park*. Ancient DNA researchers engage with de-extinction, or at least entertain press and public interest in it, not necessarily because it represents their research, but because they acknowledge the advantages of news values when communicating to public and political audiences for support. In this chapter, I also outlined the celebrity science concept. I argued that a celebrity science is the outcome of prolonged publicity advanced by a relationship that is actively pursued and produced by both scientists and members of the media. This concept is unique in that it argues that the entirety of ancient DNA's disciplinary development is the result of persistent science-media interactions. In this history, science-media interactions were more than episodic and individualistic. Ancient DNA was shaped by prolonged publicity over time. This chapter highlighted the tension between science and the media limelight, but also showed how the two can coexist, then coevolve with one another. I further argued that the celebrity science concept is a phenomenon encouraged by the science communication movement and expectations for practitioners to popularize their research to the public through the press. The search for ancient DNA in the media limelight is a case study of celebrity science with implications for contemporary science communication.

Throughout this thesis, contamination has been a running theme. Contamination in its most obvious form, namely contamination of ancient samples or sequences by modern DNA, was an issue that plagued the practice for the majority of its history. Interestingly, contamination concerns, despite the division they caused the community, were also a source of cohesion. As an emerging practice full of multidisciplinary interests and interactions, scientists were united through common problems concerning the preservation, extraction, and sequencing of DNA from fossils. In the late 1980s and early 1990s, contamination was a source of community cohesion as researchers discussed it in newsletters and debated it at conferences. In the 2000s, it became a subject of community conflict. But regardless of what side of the schism one was on, "believers" or "non-believers," criteria of authenticity defined the discipline. It colored researchers' memories of their histories and contributed to the shaping of their identities.

Contamination, however, also took on a subtler form. For researchers, the intense public interest and extreme media exposure that surrounded the technoscience was could be a second but equally serious source of "contamination" that they felt affected their credibility. Initially, media mobilized researchers into action, especially in its early and exploratory phase. Ancient DNA, as a novel but controversial area of research, attracted popular attention. *Jurassic Park* placed it in the spotlight while researchers, reporters, editors, and funding agencies furthered the spotlight. Ancient DNA was newsworthy and practitioners involved in it enjoyed top publications, media coverage, grant funding, and publicity. But for the researchers, publicity produced community instability, too. In the face of instability, researchers tried to distance themselves from the attention or redirect the media's gaze. In response, they learned to cultivate, then control the spotlight. In this thesis, I argued that contamination concerns and celebrity – both its arguably positive and negative effects – played a part in driving, even defining, the search for DNA from fossils as technoscientific practice.

Overall, it is important to highlight that the evidence and argument for the celebrity science concept transpired from interviews with the researchers involved in the search for DNA from fossils. The fact that interviews with the practitioners themselves presents the close connection between science and media is a further testament to the increasing presence and power of the science-media connection in society today. These practitioners' perspectives reveal the science-media connection and how they actively engage, then disengage, with it depending on changing circumstances in the professional and public landscape. Most science studies scholarship has focused on the media or has investigated a controversy or subject of science already in the media limelight in order to examine how the two interact. This work was different. While this work began with the assumption that popular culture generally – and *Jurassic Park* specifically – played a part in ancient DNA's history, it started with the scientists and used their memories to explore their experience with the media. This work provided a history of ancient DNA's disciplinary development and how popular culture was a crucial component to its evolution over the past three decades.

Future Directions

Ancient DNA as a Discovery-Driven and Data-Driven Practice

Ancient DNA and the celebrity science concept offer various opportunities for future research. My immediate interest in furthering this thesis relates to the search for ancient genomes as a discovery-driven and data-driven practice (Chapter Three Sections 3.4.2 and 3.4.3). This thesis stopped at big data and the causes and consequences of data-driven research in the biological or biomedical sciences. Interviews from this thesis that speak to this topic merit more investigation in terms of situating the history of ancient DNA research within larger historical and philosophical debates about the process of science. I propose an additional article investigating the following questions: Does celebrity affect or exaggerate the discovery-driven nature of research regarding ancient genomics? How does the data-driven nature of ancient genomics figure into larger conversations about the role of hypothesis testing in science? How should celebrity be considered in conversations about the philosophy and process of science?

De-Extinction's Disciplinary Development

Another interest to explore is de-extinction's potential for disciplinary development in its own right (Chapter Four Sections 4.2.3 and 4.3.3). This thesis only scratched the surface, but it reveals striking similarities between the emergence of ancient DNA research in Chapters One and Two with what appears to be the beginnings of de-extinction's disciplinary development in Chapters Three and Four. This observation raises the following question: Is de-extinction another example of an emerging technoscience and celebrity science?

Celebrity Science and the Media

This thesis purposely pursued interviews with scientists and not members of the media. Future research will carry out interviews with the media to explore their perspectives on the celebrity science concept and their participation in the process. For example, how does the media perceive their participation in structuring narratives surrounding ancient DNA activity? Do media personnel, especially editors, see themselves as active agents in shaping science in terms of managing its content and communication? Future research will also carry out investigations into how knowledge of the celebrity science concept affects how media personnel view their role in covering and communication research. For example, how does knowledge of the celebrity science concept affect reporters' and editors' work in both theory and practice? Does the celebrity science concept have practical and ethical implications for media personnel?

Celebrity Science and Ancient DNA

This thesis focused on the search for DNA from fossils by scientists, research institutions, and research initiatives in North America, Europe, and Australia. Future research could examine ancient DNA activity with a focus on the role of the media in country-specific and culture-specific contexts. Future research could also explore the role of celebrity in ancient DNA activity in other continents such as South America and Asia. These additional analyses and cross-cultural comparisons could contribute to understanding of ancient DNA's overall disciplinary development.

Celebrity Science as a Global Phenomenon

A further and final direction to take this thesis would be to extend the celebrity science concept to other subjects of science. Questions for investigation include the following: Are there other scientific subjects that can be considered as an example of a celebrity science (i.e. forensic science, the space race specifically, astronomy more generally)? Additionally, to what extent is a celebrity science a product of specific political, economic, or cultural systems? In other words, is celebrity science a global phenomenon? This thesis has theorized about the celebrity science concept, drawing on science studies scholarship to highlight some of the more generalizable moments that could suggest a celebrity science in the making, but other studies of other sciences would enhance our understanding of the celebrity science concept and its applicability more widely.

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APPENDIX A – METHODS

DATA COLLECTION

The goal of this thesis was to trace the disciplinary development of ancient DNA research from an emergent to a more or established technoscience from the 1980s to today. To explore this history, my research methods included traditional historical research methods, but I also had the added approach of oral history interviews ("Principles for Oral History and Best Practices for Oral History" 2000; Thompson 2000; Shopes 2002; Doel 2003; Ritchie 2003; Perks and Thomson 2006; Thomson 2007). My goal was to interview scientists and students who work in or around the technoscience to use oral history to help write my history of how the search for DNA from fossils developed into a discipline. As a contemporary and interdisciplinary practice, its history can be told from various disciplinary viewpoints. However, this thesis has approached this history from the broad background of evolutionary biology and includes interviewees from various viewpoints. I also approached this thesis with an interest in the interplay between science and the mass media, particularly with an interest in the scientific, historical, and sociological connections between the search for DNA from fossils and de-extinction. Consequently, interviews also included less traditional participants or perspectives in the history of ancient DNA research because of this focus on de-extinction.

First, I selected a sample of scientists via the professional and popular literature (scientific publications to media reports and reviews) on the topics of ancient DNA activity and de-extinction. Second, interviewees were identified at "Ancient DNA: the first three decades," a commemorative conference hosted by the Royal Society in London in November 2013 to celebrate its thirty-year history. Third, interviewees were included via suggestions by my second supervisor, Mark Thomas at University College London, who works within this field. Finally, scientists were selected via "snowball sampling," the process by which potential interviewees are selected based on recommendations made by previous interviewees (Atkinson and Flint 2001). Ultimately, this thesis is an asymmetrical account of the history because it is based on practitioner's perspectives and does not include interviews with members of the media. This thesis is also an asymmetrical account of the history because of its Anglo-Saxon focus in terms of professional or popular literature on the topics of ancient DNA activity and de-extinction.

I conducted fifty-five interviews with scientists, as well as doctoral and postdoctoral researchers, involved in ancient DNA activity. These interviewees represent researchers from disparate disciplines within evolutionary biology and can be characterized within the following categories: paleontology, archeology, anthropology, botany, epidemiology, evolutionary genetics, population genetics, molecular biology, microbiology, and computational biology. These interviewees work within the following countries: United States, Canada, England, Ireland, Australia, Germany, Denmark, Sweden, Norway, France, Spain, and Israel. Efforts were made to interview researchers who represent different scientific or epistemic views regarding the proper practice of ancient DNA research. Overall, fifty interviews were individual interviews and five interviews were group interviews with doctoral and postdoctoral researchers. Of the fifty individual interviews, ten were female and forty were male. Of the five group interviews, which included a total of seventeen doctoral and postdoctoral researchers, nine were female and eight were male. Of the fifty-five interviews, forty-one were conducted face-to-face and fourteen online via Skype audio and video. There were ten further interview requests with five females and seven males that never received a response, were declined, or were never completed. These interviewees were not selected at random but are a sample of the population of practitioners in North America, Europe, and Australia.

INTERVIEW PROCESS

The interview method was semi-structured in style and on average two-hours in length. The interviewee questionnaire form included five sections; one) Background and Education, two) Professional and Theoretical Commitments, three) Perspective on Ancient DNA Research, four) Perspective on Ancient DNA Research and Mass Media, and five) Perspective on De-Extinction (See Appendix C). The first two sections provided context for the interviewees' personal and professional interests, but also served as casual conversation before asking questions that would require recollections of ancient DNA activity or opinions about its disciplinary development. The third section contained the most important information for the thesis. This is where interviewees shared their narratives of how ancient DNA research emerged and evolved over the past thirty years in terms of its technoscientific development, as as well as its community culture. The fourth section explored the role of the interviewee as a science communicator, including their opinion of the role of the mass media in ancient DNA research. The final section discussed their opinions on de-extinction in terms of its feasibility and its connection to ancient DNA research. Interviews followed this format, but interviewees were also encouraged to share stories or offer specifics or generalizations regarding what they thought was interesting about the history. Overall, interviewees were sent an e-mail with information about the

PhD project, a consent form, and a request to be interviewed face-to-face or via Skype audio and video (See Appendix D). When possible a conversation with the interviewee was conducted prior to the formal interview. Interviews were recorded on two recorders; an Olympus DSS Player and an iPhone 6 via Voice Memo Application.

INTERVIEW ANALYSIS

For this PhD project, transcriptions and all interview analyses were qualitative not quantitative. The goal of these transcriptions and analyses were thematic not phonetic. The recordings received a first listen and partial transcription. The first thirty individual interviews also received a second listen and partial transcription for the purpose of including additional information missed during the first listen and transcription. The last twenty individual interviews only received a first listen and transcription due to time constraints and information saturation. The goal was not to produce an archive of polished and publishable transcriptions. Therefore, interviews were not transcribed in their entirety. These transcriptions reflect my decisions to exclude false starts, random stutters, or details that may disclose anonymity. Edits are indicated by suspension points within square brackets. I also include grammatical edits, insertions, or exclusions where necessary. These are identified by square brackets. However, content and context are not neglected in the process. In general, italics are used for interviewee emphasis. Any quotations are cited in text and in parentheses using an interviewee code, followed by a dash, then followed by a recording time. For example, individual interviews (Number-Time) are cited like this (1-01:30:00). Similarly, group interviews (GI-Number-Time) are cited like this (GI-1-01:30:00). All interviews were reviewed for information and quotations to be included in the thesis. At least one quotation was selected from every individual interview and group interview for inclusion in the thesis.

The goals of these transcriptions were qualitative and thematic. Throughout the data collection, transcription, and analysis process, I generated a list of main themes and sub-themes regarding the technoscience's history. These themes were informed by professional and popular literature on the search for DNA from fossils, as well as information from interviewees. My analyses and arguments in this thesis reflect my effort to listen for reoccurring themes in the retelling of the history, as well as conflicting viewpoints that suggest there is more than one story to tell. I have tried to represent the community's various viewpoints, including their disagreements, while listening for overarching themes about the technoscience's history that are shared amongst scientists across space and time. The one common theme, in addition to concerns about contamination, running through their accounts is the role of the media. Interviewees all agree, though to differing degrees, on the role of the media as a direct or indirect influence on the development of the discipline. This is not the only theme, of course, but it is the theme I chose to explore in my analyses of these interviews. This theme was apparent following the first six to twelve interviews. Interviews afterwards were analyzed with attention to the role of the media in order to generate evidence for this theme and confirm the hypothesis of the celebrity science concept. These interviews were also analyzed with attention to information that deviated from or contradicted this hypothesis. Overall, interviewee information about the history confirmed an awareness of the media's role in the emergence and evolution of the field.

DATA MANAGEMENT

In the thesis, interviewee quotations were anonymized. This was decided because this is a contemporary and competitive community of scientists actively practicing and publishing today. This anonymity allowed for the candid stories and memories detailed in this history. It also allowed for the professional or personal protection of individuals in the community. Each interviewee was assigned a code for anonymity. This number was assigned to an electronic folder with the recorded interview file, transcribed interview file, and consent form. Each interviewee received a copy of this consent form. Each file for each interviewee was organized and secured in an electronic folder that is password protected and data encrypted. The source location for these materials exist on a personal data encrypted domain. Another source exists on a second and separate personal data encrypted domain. Although these interviews are the private property of the interviewer, this collection is the first oral archive to exist on the topic of ancient DNA activity. This oral archive is not available upon request. However, interviewees are interested in interviews being archived in an oral history archive. If this is pursued, consent forms for removing anonymity and archiving the recordings will be reissued. In addition to this oral archive, I have procured a material archive of the history of the technoscience including primary and secondary documents (letters, newsletters, media, conference information, unpublished manuscripts, and grant applications) that interviewees had collected throughout its thirty-year history. These documents are scanned, filed, and when referenced in the thesis they are referenced as "Elizabeth Jones Personal Collection" with information about who the file was from. This material archive can be accessed upon request.

ETHICS

I have completed the compulsory Ethics Procedures and this methodology has been approved by the Ethics Committee in the Department of Science and Technology Studies at University College London. Interviewees have given consent for me to use their interviews and information in the form of anonymized quotations for my thesis, publications, and presentations on this subject as it relates to this particular project. The Research Ethics Reference for this particular project is STSEth023.

APPENDIX B – QUESTINONAIRRE FORM

SECTION 1 – BACKGROUND AND EDUCATION

1. When and where were you born?
2. Where and what did you study as an undergraduate?
3. Where and what did you study as a graduate student and why?
4. How do you identify yourself as a scientist? What is your first loyalty?

SECTION 2 – PROFESSIONAL/THEORETICAL COMMITMENTS

5. Where do you currently work and how did you come to this position?
6. What are your research interests? What kinds of questions are you interested in answering?
7. Why do you think these questions are important to answer?
8. What methodologies do you use to answer these questions?
9. What is the most challenging and most rewarding part of your work?
10. How do you view your research's impact within the scientific community?
11. How do you view your research's impact within society?

SECTION 3 – HISTORY OF ANCIENT DNA RESEARCH

12. How do you view the introduction of molecular biology into evolutionary biology?
13. How do you think aDNA research fits here?
14. How and when did you first become interested in a career working with aDNA?
15. Where and how do you relate to the aDNA community? How do you fit in?
16. What do you think were scientists' initial expectations of aDNA research? Were these early expectations met, failed, altered, or exceeded?
17. What are the possibilities and limitations of aDNA research?
18. What is the most important research being done in aDNA research?
19. Is the aDNA community a competitive or cooperative group of researchers?
20. Overall, how has aDNA research evolved from a novel approach to studying ancient and extinct organisms into an accepted scientific practice?

SECTION 4 – ANCIENT DNA RESEARCH AND MASS MEDIA

21. Do you communicate your research to the public? If so, what forms of communication do you use and why?
22. What is your opinion of how the mass media communicates aDNA research to the public?
23. Are you often asked questions that are irrelevant or indirectly related to your research? If so, how do you answer these questions?
24. Do you think the media misrepresents your research? If so, why?
25. What do you think is the objective of the mass media? For example, what do you think is the goal of the journalist?
26. How do you think the aDNA community of researchers was influenced by mass media reports on the science and in particular the book and film *Jurassic Park?*
27. What is your interest in communicating your research to the public?

SECTION 5 –DE-EXTINCTION

28. What is your opinion of the idea of de-extinction?
29. How is aDNA research and de-extinction study related?
30. Is the de-extinction community of researchers distinct from the aDNA community of researchers? If so, why? If not, why?
31. What researchers do you see as part of this de-extinction community?
32. Do you think de-extinction study eclipses other ways in which aDNA is used?

CONCLUSION

33. If you were writing the history of ancient DNA research, what would be your take-home message?
34. Is there anything else you would like to share that I have not asked about or that you would like to talk more about?

APPENDIX C – CONSENT FORM

INFORMATION ON DOCTORAL RESEARCH FOR INTERVIEWEE

My doctoral research concerns the history of ancient DNA research and de-extinction. My first objective is to construct an accurate history of the development of ancient DNA research as a scientific and technological practice. My second objective is to understand the interplay between professional science and popular science and how these processes have influenced the history of ancient DNA research. I approach this project as a historian. The data collected from interviews will be used for my research in the form of anonymous quotations and quantitative analyses involving word frequency and word association.

CONSENT TO DOCTORAL RESEARCH FROM INTERVIEWEE

In consideration of the work that Elizabeth Jones is doing to collect and preserve the memories and perspectives regarding the history and science of ancient DNA research and de-extinction, I give her consent to use the information from my recorded interview for her doctoral research, publications, and presentations on these subjects.

1. I understand that I have the option to withdraw my contribution to this research at any time and without giving reason. I may withdraw my participation during the interview and I may withdraw my contribution following the interview by contacting Elizabeth Jones via email at elizabeth.jones.13@ucl.ac.uk.

2. I understand that the information from my recorded interview will be made anonymous and my identity will be protected. It will not be possible to identify me in the research published or presented.

3. I understand my recorded interview will be protected and held by Elizabeth Jones for five years following doctoral research completion. All recordings, transcripts, and notes will be securely disposed five years following doctoral research completion.

4. I understand that I have copyright to my recorded interview. I consent to transfer copyright to Elizabeth Jones for her research objectives and this will result in a joint-ownership of the recorded interview.

5. In the case that Elizabeth Jones decides to transcribe my recorded interview for an archive, I understand that she will contact me via phone or email for permission. I have the right to give or deny consent to this further and future action.

I understand that Elizabeth Jones has completed the required Ethics Procedures and has been approved by the Ethics Committee through the Department of Science and Technology Studies at University College London. I understand that my signature below provides consent for the information above.

PROCESSING NUMBER

INTERVIEWEE INFORMATION

Name ____

Address ____

Email ____

INTERVIEWEE SIGNATURE

Date ____

Signature ____

INTERVIEWER SIGNATURE

Date ____

Signature ____

IMAGE REMOVED

Figure 1

Charles Pellegrino's "Dinosaur Capsule" (1985)

IMAGE REMOVED

Figure 2

Photograph of the "The Extinct DNA Study Group" (1983)

IMAGE REMOVED

Figure 3

Alec Jeffrey's "Raising the dead and buried" (1984)

IMAGE REMOVED

Figure 4

Figure from Kary Mullis and Fred Faloona's "Specific Synthesis of DNA in Vitro via a Polymerase-Catalyzed Chain Reaction" (1987). This figure demonstrates the polymerase chain reaction amplification of a 1l0-bp fragment from the first exon of the human fl-globin gene, demonstrating PCR's ability to amplify DNA exponentially.

IMAGE REMOVED

Figure 5

Malcolm Browne's "Scientists Study Ancient DNA for Glimpses of Past Worlds" (1991)

IMAGE REMOVED

Figure 6

Jurassic Park Book Cover (1990)

IMAGE REMOVED

Figure 7

Ancient DNA Newsletter Cover (1992)

IMAGE REMOVED

Figure 8

Jurassic Park Movie Poster (1993)

IMAGE REMOVED

Figure 9

Virginia Morell's "Dino DNA: The Hunt and the Hype" (1993)

IMAGE REMOVED

Figure 10

Alan Cooper and Hendrik Poinar's
"Ancient DNA: Do it Right or Not at All" (2000)

IMAGE REMOVED

Figure 11

Graph from Eske Willerslev and Alan Cooper's "Ancient DNA" (2005). This graph highlights ancient DNA studies from 1984 to 2005 with attention to the types of DNA (mtDNA or nuDNA) recovered, age and environment of the samples, and whether the results have been replicated.

IMAGE REMOVED

Figure 12

Image from Marcel Margulies et al. “Genome sequencing in microfabricated high-density picolitre reactors” (2005) illustrating the process of next-generation sequencing.

IMAGE REMOVED

Figure 13

Figure from Hendrik Poinar et al. "Metagenomics to Paleogenomics: Large-Scale Sequencing of Mammoth DNA" (2006). This figure showcases the mammoth metagenome and the percentage of read distributions from the host organism, other organisms, and the environment.

IMAGE REMOVED

Figure 14

Figure from Richard Green et al. "A Draft Sequence of the Neandertal Genome" (2010) comparing the Neandertal genome to the human reference genome.

IMAGE REMOVED

Figure 15

Image from Ewen Callaway's "Ancient DNA reveals secrets of human history" (2011) illustrating new hypotheses based on ancient and modern genomic data about human origins, migrations, and evolution in relation to Neanderthals and Denisovans.

IMAGE REMOVED

Figure 16

Graph from Elizabeth Culotta's "New Life for Old Bones" (2015) tracking the increased professional interest in ancient DNA activity.

IMAGE REMOVED

Figure 17

Figure from Ann Gibbon's "Ancient DNA Divide" (2016) demonstrating disparities between ancient DNA funding in the United States and Europe.

IMAGE REMOVED

Figure 18

Cover of *National Geographic* featuring TEDxDeExtinction Conference with an article by Carl Zimmer on the prospects and problems associated with reviving extinct species (2013).