

# THE DANISH BRAINS - AN ARCHIVE

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# THE DANISH BRAINS

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Cover Illustration: The empty storage room  
at Psychiatric Hospital Risskov, 2018, after  
the Danish brains had moved to Odense.

Photo: Thomas Erslev

# THE DANISH BRAINS – AN ARCHIVE

*MATERIALITY AND TEMPORALITY IN A COLLECTION  
OF PATHOLOGICAL HUMAN BRAINS, 1945-2018*

*Thomas Erslev*

PhD Thesis

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# Explanations of key neurological concepts.

This thesis relies partly on very technical literature from the brain sciences of the past seven decades. Writing the thesis without the use of some neuroscientific jargon would have been almost impossible, and explaining the concepts' meaning in the course of the chapters would have introduced unnecessary excursions and would probably have confused more than clarified. Thus, I have decided to include here a list of explanations of the neurological concepts a humanist reader cannot be expected to command. The definitions are my own adaptations from neuroscientific handbooks, Wikipedia-articles, and talks with practicing neuroscientists. I have attempted to convey only the basic information necessary to understand the uses of these words in the thesis. Read it in advance, or use it for reference as you read on.

## **Histology**

The study of microscopic anatomy: Investigating tissues under microscope to determine their components and relations. Modern usage includes cytology, the study of cells. The word histology also refers to the technical manipulations required to see these structures under the microscope, including fixing or hardening tissue, slicing it very thinly, and staining it chemically to highlight different micro-anatomical features.

## **Cytology**

The study of cells (cyto- : cell). The science of how the different parts of the cells are arranged, of their normal function, and of their pathology (what can go wrong).

## EXPLANATIONS OF KEY NEUROLOGICAL CONCEPTS.

### **Staining methods**

In histology, the tissue for inspection under the microscope is manipulated in different ways in order to see different things. A staining method is a way of treating the tissue, usually with chemical solutions, to make certain structures appear more clearly under the microscope. Stains bind to different parts of the tissue, so one stain may highlight the neurofibrils, while another highlights the Nissl substance. The choice of stain depends on what the researcher is looking for.

### **Nerve cells/neurons**

The cells that make up the nervous system. Neurons come in many different varieties. They can be classified by function, morphology, location, and more. Some have many protrusions (see dendrites/axon), others only one. Some have very long protrusions, some very short. Some receive signals from the senses, some control the muscles, and others establish inter-connections inside the nervous system.

### **Dendrites (A)**

Branching extensions of the nerve cell. Dendrites are the “receivers” of the nerve cells, and they propagate impulses received from other neurons. Some neurons, called multipolar, have many dendritic trees (like the picture), branching in all directions from the cell body. Others, called bipolar, have one dendrite. Finally, unipolar neurons have only a single protrusion that splits, so that it works as both receiver and transmitter.

### **Nissl bodies (B)**

Small granules ‘swimming’ inside the cell close to the nucleus, named after Franz Nissl. Nissl bodies are particularly susceptible to aniline (basic) staining methods because they contain acidic RNA that easily absorbs basic substances. The number and position of the Nissl bodies changes under different physiological conditions, and they may disappear in some pathological cases.

### Cytoplasm (C)

All the material in the cell, except for the nucleus.

### Cell nucleus (D)

A membrane-bound 'kernel' within the cell, which contains almost all the DNA of the cell.

### Axon (E)

The long nerve fibre protruding from the nerve cell. It is the "transmitter" of the neuron, carrying information to other cells. The axon terminates (G) at the synapse, where its information is transmitted via dendrites to other nerve cells.

### Myelin sheath (F)

A fatty structure that insulates the axon in order to increase the speed at which information can travel from cell to synapse. Demyelination is the loss of these myelin sheaths, and it is a pathological process connected with multiple sclerosis. Demyelinated cells result in disrupted signals within the brain, and from the brain to other parts of the body.

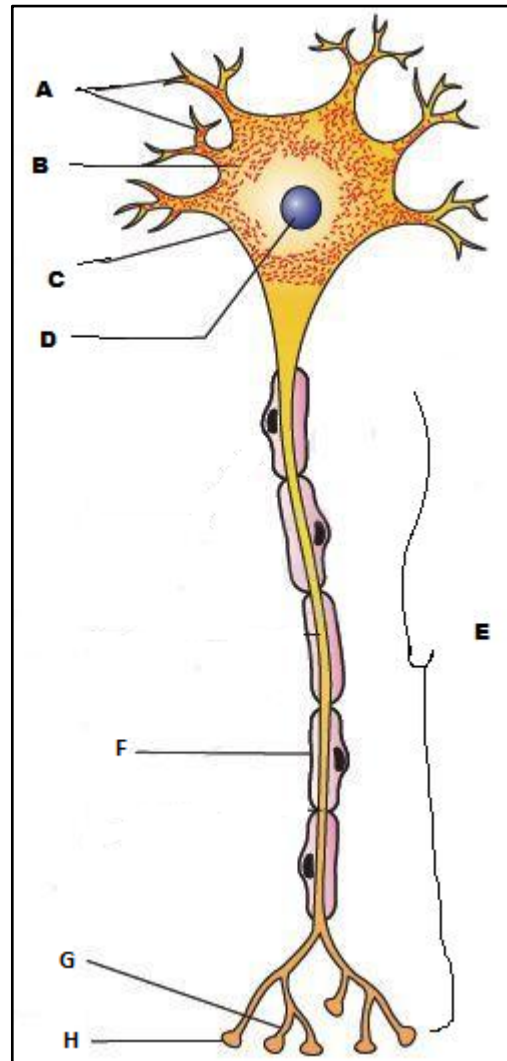


Figure 1  
Schematic of a neuron.

### Synapse (H)

The junction between nerve cells, where information is transmitted from an axon to the dendritic network of other cells. The synapse is a tiny gap between one

## EXPLANATIONS OF KEY NEUROLOGICAL CONCEPTS.

neuron's axon and another's dendrite, across which chemical or electrical signals pass information.

### **Neurofibrils**

Also called neurofilaments, these protein structures are found in the cytoplasm. Their primary function is support. As part of the neuronal cytoskeleton, they provide primary support to the axons, and thereby regulate axon diameter. This affects the speed of information travelling through the axon.

### **Nucleic acids**

The acidic element in the Nissl substance, that is easy to stain. DNA and RNA are both nucleic acids. Today we distinguish between the two on the basis of their function – DNA is the 'store' of genetic code, while RNA are the specific transcriptions of this code, that initiate and regulate cellular and bodily processes. However, while Lárus Einarson and his contemporaries in the 1940s and '50s could distinguish on a chemical basis, he believed them to be essentially the same, and to share a common purpose.

# Introduction

A brain in a vat, suspended in clear liquid. Conjure up this image, and associations multiply. Abstractions, concretions, and popularisations abound. Maybe you see a scavenged brain on Victor Frankenstein's lab bench moments before it is plopped into the scalp of his famous monster; maybe you think of the floating talking heads in jars from the animated TV-series *Futurama*. Others might not have a ready depiction of the situation, but think instead of the philosophical mind/body problem. The brain in a vat then symbolises the abstract possibility that our bodies are not really necessary, that the experience of life might be emulated by connecting said floating brain to enough in- and outgoing cords and tubes, feeding it sensory stimuli, providing feedback on its emissions, and connecting it to other hosts like some futuristic bio-internet. You may find this idea gruesome, dystopic, and see in it the possibility that the entire world is a lie as in *The Matrix*, or you may dream of the day we shed our limiting bodies, which require exercise and sustenance, becoming pure, unhindered thought.

This thesis is about a specific collection of 9.479 brains in vats. It is not about the brain-in-vat trope, nor is it about abstract imaginaries, or philosophical foundation problems. When the brains in vats are no longer fictional but real and material, the questions also become more concrete: How did they get there? Who put them there? Whose bodies were they taken from? What is their purpose? Are they valuable? Are they useful? What does their age mean? What does their material preparation mean?

## INTRODUCTION

The specific brains I write about have come from psychiatric patients who died in hospital care in Denmark between 1945 and 1982. In this period, the Institute of Brain Pathology in Risskov north of Aarhus received brains from hospitals all around the country. The brains were removed without consent from deceased patients and shipped to Risskov. There, a team of pathologists and lab technicians would examine each brain and write a thorough neuropathological report to send back to the psychiatrist at the supplying hospital, before storing the brains. Sometimes, researchers would come back to a brain with a particularly rare pathology, or compare brains from patients with similar diagnoses. Mostly, however, the team was so busy receiving and examining new brains that the store simply grew larger with no apparent uses. When the Institute of Brain Pathology shut down in 1982, the brain collection was kept. Today it is used in research in ways its creators never imagined.

This collection is the nexus that holds together my thesis. I have followed the Danish collection of brains as it has led me from field to field, moved in and out of contexts, and taken on new meanings. This is not a thesis about the physical brains so much as it is about their intersections with science, ethics, policy, and fiction. My source material as a historian is text, and the people who write it; I do not have the neurological expertise to learn much from the brains themselves. I have not lacked for material, for the brain collection has been many things besides its physical specimens. Over the past seven decades, it has inhabited the minds, lives, and workstations of physicians, psychiatrists, patients and their next of kin, politicians, ethicists, and the public in general.

What I have studied is how these people have interacted with the brains, both conceptually and materially, individually and collectively. In this introduction, I will first present in an abbreviated way the main arguments and contributions of my research. I will then summarise the collection's chronological history, while outlining along the way the contents and protagonists of each chapter.

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**Figure 2**

The brain storage room at Psychiatric Hospital Risskov as it looked in 2005. Each anonymously numbered bucket contains a formalin-fixed brain.

Photo: Karl-Anton Dorph-Petersen

## I ARGUMENTS AND CONTRIBUTIONS

The main research contributions of the thesis are both empirical and theoretical. New empirical studies of a previously undescribed collection have made possible a deep engagement with current discussions in the historiographic literature on scientific archives, on the interplay between time and matter, and conceptual and practical work.

Empirically, I present new knowledge about two things: The brain collection itself and affiliated actors, and an episode in the emergence of bioethics in Denmark, which jumped off from the brain collection, but which achieved much

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further reach than a local discussion of the collection. Theoretically, the employment of “the archive” as an analytical frame has allowed me to show in each of these empirical cases, how intricately connected and messy the relationships between time and matter, as well as word and thing are.

The collection itself has scarcely been described before. Its treatment has been limited to articles in newspapers and periodicals. Thus, mine is the first attempt at a thorough historical account of the collection. I bring new historical evidence about two actors connected to the collection, Lárus Einarson and Knud Aage Lorentzen, both of whom were prominent figures in the Danish medical establishment in mid-twentieth century, but are undescribed by previous historians. I also provide the first account of the collection practices themselves, as well as the contemporary reasons for its establishment.

This empirical contribution is not only interesting for people wanting to know more about the specific Danish collection. It also contributes to the history of neuroscience more generally. Work with human tissue has been a big part of neuroscience, but one that is often overlooked in historical accounts. These tend to focus on two areas of neuroscience: Animal studies, and in vivo imaging. The latter is interesting because it purports to show directly functional changes as they occur. The first is interesting because it has allowed for model simulations and experimental production of otherwise off-limits conditions. Therefore, these are the two fields that have most obviously contributed to the bank of neuroscientific knowledge. However, the neuroscience of human nervous systems would not be worth much without studies on physical human brains to underpin these research fields.

The other empirical contribution regards the discussions of the purview and purpose of the ethical council in Denmark. In some early discussions, the brain collection became a kind of synecdoche for bigger issues, such as patient autonomy, paternalism, the “right not to know”, scientific rigor and evidence, etc. Later, when the brain collection was discussed, these questions receded more and more into the background. As such, following the bioethical discussions of the brain collection uncovers a proxy measure for the societal accustomedness to and acceptance of the Ethical Council. When it was still new, a case like the brain

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collection could be the cause of a foundational discussion of the Council itself, and its power over the medical establishment. Later, the Ethical Council was referred to as an impartial agent of evaluation, whose decisions were cited by all stakeholders as beyond reproach. Less about the brain collection itself, this case measures a public attitude to a governmental organ, which can be very difficult to gauge by standard historical-sociological methodologies.

Histories of research ethics in recent years have focused on two areas: bioethics as a phenomenon of the public sphere, in which non-specialist gain say in discussions of medical ethics, usually the purview of medical professionals exclusively; and research ethics, an internal field where peers determine the ethical soundness of proposed projects. Research ethics is mainly organised internally (in Internal Review Boards (US) or Research Ethics Committees (EU&UK)) at universities or research institutes, and their work rarely sees public light. Bioethics, on the other hand, pertains primarily to principle discussions, about when life commences, what constitutes a body, where therapy ends and improvement begins, and so on. These discussions rarely have a direct relation to ongoing research, but often take the shape of hypotheticals and prescriptions of futures.

Theoretically, the concept of the archive is expanded in close engagement with the specific case study of the Danish brains, as well as bioethical and evaluative reactions to it. This case both reinforces the historiographic concept developed by Lorraine Daston et al., and also adds new layers of significance to its theoretical use. Central in my reading is the duality of time and matter. In the archive, these two meet not as oppositions or discrete concepts, but as inherently co-constitutive in a kind of feedback-loop. This was explored somewhat in Daston's volume, but in the case of the brain collection, through the employment of Hans-Jörg Rheinberger's historiographical framework, I deepen the understanding of this relationship significantly, and show it to be central in both scientific, managerial, bioethical, narrative, and evaluative work with concrete biological entities. This will be further elaborated in chapter 1.

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Finally, though not negligibly, I have spent considerable time and effort translating from the primarily Danish sources, in order to make this history available to an international audience. All translations from Danish sources, whether oral or written, archival or published, are my own.

### 2 CHAPTER OVERVIEW

In order to provide an overview of the structure and contents of the thesis, some basic knowledge of the collection's chronology is necessary. Here, I provide short abstracts of the individual chapters, interspersed between contextual and chronological background paragraphs. Readers only looking for chapter summaries can skip to paragraphs with chapter numbers highlighted in bold.

**Chapter 1** introduces and elaborates the theoretical framework that connects the individual chapters. Besides the collection itself, a number of concepts and tools hold this thesis together. While the source types and contexts in each chapter differ, my overall analytical strategy remains the same throughout the project. It hinges on viewing the brain collection as an archive, understood in a special way, which is developed in chapter 1. This analytical perspective includes paying attention to the ways in which material and conceptual work inform and rely upon each other; to the meanings and understandings of time that historical and contemporary actors bring with them; and to the multiplicity and variety of different collections that are constituted both materially and conceptually through time and matter. With these concepts as a constant anchor throughout the chapters, I show that even if the collection is multiple, unruly, and contested, we can achieve a deeper understanding by paying attention to underlying assumptions. The theoretical framework is revisited several times in succeeding chapters, when warranted by the empirical work.

---

The Institute of Brain Pathology (*Hjernepatologisk Institut*; shortened HPI) was established in 1945 for two primary reasons. One was managerial, the other scientific. Erik Strömberg, who became the head of the Psychiatric Hospital Risskov in 1945, expressly wished to model the hospital upon the Max Planck Institute in Munich, where he had spent some time during his postgraduate

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studies, and which he considered the world's foremost psychiatric research institution. This meant dividing the work in a similar way as it was in Munich, and included establishing an institute for pathological anatomy of the brain. Strömngren's primary motivation for HPI was the hope that it might improve the clinical side of psychiatry – that detailed knowledge of the lesions and physical alterations in brain disease, as well as more certain post mortem diagnoses, would help psychiatrists to better understand and diagnose their patients. Strömngren himself was open-minded and plural in his approach to psychiatry, and so the broader knowledge he could impose on his colleagues around the country, the better.

The second, scientific, reason related to professor of anatomy Lárus Einarson. In the 1930s, he had developed a histological stain that could uncover correlations between structural changes in the nerve cells and altered activity stages in the moment of death. After more than a decade of experimental animal studies, the hope was that Einarson's method could be turned to human psychopathologies; that his histological investigations of pathological brain tissue from humans might go beyond obvious lesions and elucidate some connection between changes within brain cells and mental diseases. To pursue this hope, a ready and plenty supply of human psychopathological tissue was needed.

Einarson is the protagonist of **chapter 2**. Here, I present his ideas about the past and future of neuroscience circa 1945, and show how his technical as well as historiographical convictions laid the foundations for the brain collection. This chapter does not deal with the brain collection as such, but presents Einarson's research project as an important cause for its institution. Looking at the collection's institutor provides important clues to its perceived scientific significance at the time, and facilitates a more nuanced understanding of its subsequent history.

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Neither Strömngren nor Einarson had the time to oversee the newly established Institute of Brain Pathology. Such an institution required much attention and detail. In connection with its inception, Strömngren oversaw the practicalities of

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procedures and instrumentation, but in the end, he needed to free himself of it to return to his duties as hospital chief. Einarson likewise had more than enough work running the anatomical department at Aarhus University, besides his ongoing research. Therefore, one of Einarson's previous graduate students and close collaborators, Knud Aage Lorentzen, was hired to direct HPI. This involved scientific work, of course, but also a great deal of management. More than research, Lorentzen spent his time on equally necessary, though perhaps less prestigious tasks, such as teaching, autopsies, and standardising practices at HPI.

**Chapter 3** is devoted to what I call the quotidian interactions with the collection. In telling stories such as this one, it is easy to be swept along by the novelty, the outlandishness, and the tropes of mad science or futuristic imaginaries. However, collecting thousands of brains in the course of almost four decades requires some people to simply go to work daily, dealing with the mundane trivialities present in any kind of work, and so seeing the collection not as a curiosity, a travesty, a triumph, but simply as a place of work, an everyday thing. The chapter presents first the work of Lorentzen, a meticulous and hard-working scientist who never revolutionised his art or made great breakthroughs. His contribution lay instead in the daily continuance of skill and care, including what could be called archival practices of organisation, data management, and preserving documentation for future generations of histologists. The chapter then proceeds with a description of collection practices at HPI, which were a somewhat complicated affair that also had to be learned and performed carefully, and which some contemporaries found in breach of codes of professional conduct at the time. Finally, the chapter documents some of the painstaking work that Strömngren put into establishing the institute and making sure its facilities were in order.

---

When K.A. Lorentzen retired in 1982, HPI also shut down. The collection itself was left in basement rooms at the Psychiatric Hospital in Risskov, without clear custodianship. Some research projects used tissue from the collection in the course of 1980s, '90s, and 2000s, but not before 2005 would the collection be reinstated as a proper tissue bank under the purview of the Translational Neuroscience Unit at Aarhus University. From 2006 to 2016, the collection was overseen by Karl-

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Anton Dorph-Petersen, a pathologist specialising in stereology, a mathematical method for reconstructing three-dimensional representations from two or more flat tissue slices. His primary job was to evaluate the usefulness of the tissues in the collection for a range of research methods. The collection, however, was in such a sorry state as he began, that he spent years just to establish an overview of its contents, and making sure the storage facilities matched contemporary requirements (e.g. he had to replace many of the buckets that had started leaking noxious formaldehyde vapours).

**Chapter 4** presents the main scientific research projects to rely on material from the brain collection. From the 1940s until today, research output has been limited but diverse. For heuristic reasons I compress this research overview into one chapter, rather than fit it chronologically between the other chapters. By doing so, the chapter demonstrates at once the scientific limitations and affordances of the brain collection, as well as the extremely varied nature of post mortem research over the 7 decades of its existence. The chapter continues the interests from chapter 2, as it shows how Einarson's research was continued at HPI, and from chapter 3, in maintaining the focus on quotidian practices after Dorph-Petersen's take-over of the collection. The chapter also revisits and underpins the framework from chapter 1, as it presents a concrete example of how archived preparations are open to 'epistemic recall' when they enter into novel research domains, and are submitted new questions. The specific case is the enrolment of the archived Danish brains in an epigenetic DNA methylation research programme.

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In 1990, the existence of the brain collection became public knowledge, along with the fact that it had been collected without consent from patients or their next of kin. This spurred public outrage and discussion, and led to relatively new bioethical authorities in Denmark to engage with the problematic history of the brain collection, and the question of how to use it in the future. Discussions of this nature have continued intermittently from 1990 until today. Throughout this period, destroying the collection has been a possibility, proposed at different times

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for different reasons including lack of scientific worth, its constituting an ethical travesty, and expensive upkeep.

Chapters 5-7 concern these more public engagements with the brain collection, still within the frame of the temporal and material archive. Chapter 5 in part resumes the theoretical framework from chapter 1, as I expand it with the concepts of 'waste' and 'value'. This expansion makes it possible to understand the public engagements with the Danish brains, and to integrate this understanding with preceding chapters.

**Chapter 5** uses Dorph-Petersen's 'inheriting' the collection as one of three empirical cases with which the concepts of 'value' and 'waste' are operationalised in relation to the brain collection. The first case is a British discussion of the state of the art of brain collections, in which use and waste of tissue is a central concern. The second case is Dorph-Petersen's work with reinstating the collection and updating it to make it a useful resource for contemporary science. The third case is a fictionalised account of the real collection, in a tween-novel about a wrongfully excised brain. This novel makes explicit some of the many lay concerns that attend the brain collection, but which medical stakeholders rarely engage with, and thus can easily be forgotten. The overall aim of the chapter is to introduce 'waste' and 'value' as theoretical concepts, and show how they relate to the larger framework developed in chapter 1.

**Chapter 6** engages specifically with bioethical discussions around the brain collection. The chapter presents the three biggest such public discussions, in 1991, 2006, and 2017. The 1991-discussions receive the most attention, because the brain collection at this time gave rise to debates about the purview and purpose of the newly established Ethical Council in Denmark. The brain collection thus became a weighty synecdoche in a much larger discussion. Besides mapping the positions and arguments of central actors, I further argue that the three discussions read together reveal a shift in the temporal alignment of bioethical arguments. Put briefly: In 1991, the actions of the past were discussed most fervently; the battle was over their significance as either ethically reprehensible or scientifically visionary, and these designations decided the brain collection's status in the contemporary. In 2006, the possibilities of the future became the most pressing

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issue, as actors debated how to make sure the collection could be made ethically sound for future generations, in order to avoid having to rehearse the bioethical arguments another time. Finally, in 2017, economic concerns of the immediate present made both the past and the future less important, at the same time as ethics receded and political prioritisation came to the fore.

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In 2017, the Regional Council of Central Jutland, the legal proprietor of the collection, decided it had become too costly. The construction of a new mega hospital had outspent its budget, and the council had to weigh all costs carefully. They judged that the scientific value of the collection did not justify the spending needed to move it to the new location. They cited the poor research output over the preceding decade as evidence for its low scientific value. This valuation of the collection was criticised by many actors both lay and scientific, but the Regional Council stuck with their decision. To the relief of all involved, in early 2018 the collection found a new home in Odense, where a research centre for translational neuroscience had agreed to take over the collection.

**Chapter 7**, the final chapter of the thesis, looks closely at the valuation practices that interested parties relied on in these very recent discussions. I show that economic and scientific values did not easily correspond, and that different actors use different registers of valuation, even when using the same value terms, e.g. scientific value, economic value, ethical value, etc. This chapter also draws threads back to previous chapters, as discussants rehearsed many of the arguments from earlier discussions, and relied on selective narrative accounts of the collection's history. Thus, these recent events elegantly demonstrate the historicity of the brain collection even today, and the need for historical engagement with science and bioethics in order to achieve a satisfactory understanding of the muddled, fragmented, and multiplying perspectives that attend these practices



# *Chapter I*

## Theoretical framework

In this chapter,<sup>1</sup> I provide the key theoretical and historiographical concepts that frame the rest of the thesis. First, I review the historiographic literature concerning two main features of the brain collection: its materiality and its archival nature. I then make the case, following Annemarie Mol, that to comprehend the brain collection we must approach it with a multiple ontology: not as a single, well-defined object, but as a mutating entity continually co-constituted together with whatever context we find it in, whichever actors engage with it. As will be clear from the final section of the chapter, this thesis takes just such a perspectival approach, with each chapter presenting one of the somewhat discrete lives the collection has lived over the past eight decades.

### **I MATERIALITY**

One important feature of medical collections is that they deal with material tissue. Whereas many neurosciences study either digitally represented brains or biochemical cultures grown in the lab, brain collectors deal with whole brains. This gives rise to certain practices – manipulations and exchanges of brains – which are

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<sup>1</sup> A previous version of this chapter appeared in my Master's dissertation: Thomas Erslev, 'Collecting Brains: From the Lab to the Archive' (MA, Aarhus University, 2017), <https://www.therslev.dk/wp-content/uploads/2018/10/Erslev-2017-Collecting-Brains-From-the-Lab-to-the-Archive.pdf>.

## CHAPTER I

### THEORETICAL FRAMEWORK

interesting to study for their own sake, and as pointers to ontological and epistemological assumptions underlying neuroscientific research.

The human brain is all but easy to handle when it comes out of the skull. It is almost liquid. To make it possible to study it, it has to be fixed by being either submerged in alcohol or formaldehyde, encased in wax or paraffin, or frozen. Once this is done, it can be sliced into thin sections, which can be studied under the microscope. In order to see anything useful, however, these sections must be stained with various reagents that reveal different kinds of brain tissue.

This process is called histological preparation, and it is crucial in determining what neuroanatomists see and what they are able to look for. Frank W. Stahnisch has shown the intricate co-development of neuroanatomical theories and histological staining techniques in the early to mid-twentieth century. The stains depended largely on the combination of chemicals, and often lasted only long enough for the histologist to retain a mental picture or quick sketch of what they saw, which made some critics question whether theories or autopsies came first.<sup>2</sup> The first neuro-histologists spent the bulk of their time thinking up new chemical preparations that might render the brains more legible, might reveal more anatomical structures with still greater clarity.

This painstaking process of making brains “legible” can be described in Hans-Jörg Rheinberger’s vocabulary as the fashioning of a certain class of *epistemic configurations* called *preparations*.<sup>3</sup> Rheinberger’s core conceptual pair ‘epistemic things’ and ‘experimental systems’ was developed in his book *Toward a History of Epistemic Things*. The two concepts are defined as co-constitutive, evolving in tandem. One is both a product and producer of the other, and this continuous process is the historiographical motor of science, as Rheinberger understands it.

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<sup>2</sup> Frank W. Stahnisch, ‘Making the Brain Plastic: Early Neuroanatomical Staining Techniques and the Pursuit of Structural Plasticity, 1910–1970’, *Journal of the History of the Neurosciences* 12, no. 4 (2003): 423, <https://doi.org/10.1076/jhin.12.4.413.27917>.

<sup>3</sup> Hans-Jörg Rheinberger, *An Epistemology of the Concrete: Twentieth-Century Histories of Life, Experimental Futures : Technological Lives, Scientific Arts, Anthropological Voices* (Durham [NC]: Duke University Press, 2010), pt. iv: Epistemic Configurations.

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He takes his cue from the French *epistemologie*, especially Gaston Bachelard, whose understanding of science as simplification Rheinberger summarises thus:

As Bachelard reminds us, “simple always means simplified. We cannot use simple concepts correctly until we understand the process of simplification from which they are derived.” It is only in the process of making one’s way through a complex experimental landscape that scientifically meaningful simple things get delineated.<sup>4</sup>

In this process, Rheinberger calls that which is already simplified *technical objects*. This includes instruments and well-defined scientific things. The vehicle for simplification is the *experimental system* (itself never fully simplified), and that which requires simplification or understanding is the *epistemic thing*. The epistemic thing, in other words, is that which the scientist wishes to understand. It is thus unstable, in flux. Defined by an expanding list of attributes, any change in this list also changes the object itself. Importantly, because the epistemic thing itself is yet unknown, the experimental system which provides knowledge access to it is similarly in flux. Changes in the system alter the thing; changes in the thing necessitates alterations to the system. Finally, it is important to note that the epistemic thing is at once conceptual and phenomenal, at once theoretical and material.

An episode from the history of neurosciences may help clarify this. The electrophysiologists of the 1930s were convinced that the key to understanding the brain was the synapses, and that the synapses functioned electrically. However, they could not observe this function directly, and so the electrical synapse became their epistemic thing – that which all their experiments attempted to define. For decades, they hypothesised, constructed models, collected nervous systems from diverse species, and attempted to record them in different ways.<sup>5</sup> For each new

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<sup>4</sup> Hans-Jörg Rheinberger, *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube*, Writing Science (Stanford, Calif: Stanford University Press, 1997), 28; Rheinberger’s reference is to: Gaston Bachelard, *The New Scientific Spirit*, trans. Arthur Goldhammer (Boston: Beacon Press, 1984).

<sup>5</sup> Max Stadler, ‘Circuits, Algae, and Whipped Cream - The Biophysics of Nerve, ca. 1930’, in *The History of the Brain and Mind Sciences: Technique, Technology, Therapy*, ed. Stephen T. Casper and Delia Gavrus, Rochester Studies in Medical History (University of Rochester Press: Rochester, NY, 2017), 107–35.

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specimen, the system of interventions and recording instruments had to be altered to accommodate little differences in size, tissue structure, etc. And with each new intervention and recording, the epistemic thing changed ever so slightly – which again gave rise to new hypotheses and new experimental systems to test them with. Electrical synaptic communication was the crux of the electrophysiologists' science. When they finally recorded and documented an electrical current in the late 1930s, the electrical synapse became suddenly a simple thing – a technical object that could unproblematically enter into the construction of new systems aimed and new epistemic things. Thus, in the 1940s and '50s, the idea that the brain consisted basically of an enormous network of 'on-off' switches, became the foundation for a cybernetic idea of the 'brain as a computer', which had to be understood through formal logics or reverse electrical engineering. What had in the 1930s been an enigmatic, fluctuating thing – the electrical synapse – became a stable fact able to sustain a whole field of theory. What had required expansive systems and infrastructures of experimental devices and practices, became a routine observation once experimental access was achieved.

The example shows how epistemic things, though undefined and somewhat immaterial, cause palpable changes in both material objects and systems. As with histological preparation, documenting synaptic electrical communication required a host of material interventions in the specimens under study. These interventions (themselves part of an experimental system) in turn make possible some observations, but preclude others, thus shaping what kind of experimental system can feasibly be constructed. Well-known technical objects are put to new uses, in new relationships, and thus also become less simple, more in need of adaptation and re-evaluation. This is the "complex experimental landscape", in which neither the experiment nor its expected results can be fully grasped.

Equally important in Rheinberger's Bachelard quote is the need to understand the process of simplification. Without an understanding of how things used to be complex and undefinable, and how they came to be trivial and well understood, there is a risk that the use of simple concepts will entail simplistic (read: bad) science. Simple concepts only work as far as we understand the complexities they

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simplify. Once we believe them to be simple *a priori*, they lose their usefulness. This, of course, is also a strong argument for practicing the history of science.

To return, now, to the ‘legible’ histological brains that opened this section, I move on to Rheinberger’s 2010 book *An Epistemology of the Concrete*. Here, he expands on his previous framework, among other things by introducing the concept of *epistemic configurations*. This term refers to the specific material cultures in which the co-constitution of experimental system and epistemic thing take place. One of these epistemic configurations is the *preparation*.<sup>6</sup> Preparations are privileged sites in an epistemology of the concrete, because they carry within them the interplay of matter and theory, of experiment and definition described above.

Preparations are prepared with a certain outcome in mind, but the result cannot be known until the procedure is finished. If the elements one wants to see are not to be found, either one’s theory is wrong, or the preparation has failed. That means the researcher can either rethink the hypothesis – essentially redefining the epistemic thing – or refine the preparation procedure, thereby altering the experimental system. When an agreeable preparation has been achieved, the painstaking process of fixing, slicing, staining, etc. finally *disappears* in the seemingly stable thing. Whether it be a microscopic slice, a dried plant in a herbarium, or a dyed dried body part, it shows not the work gone into its creation, but rather what it has been made to show: the arterial network, a lesion, a cellular anomaly, or something else. We look at it *as if* it was still the same as before, but a more easily approachable version of its former self. By looking at a preparation of a hand, so we think, we can see how all hands actually work. By filtering out the non-relevant cellular structures and highlighting precisely those of interest, we can *see* the cell in a way that the unprepared cell does not allow. Of course, with Bachelard we are reminded of the importance to keep in mind the extensive simplification that scientific preparations hide.

Preparations are not models, but they are not quite bare nature either. This is the deception: “whereas a model can at best claim to “look deceptively like” the

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<sup>6</sup> Rheinberger, *An Epistemology of the Concrete*, chap. 12. Preparations.

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modeled, a preparation participates in the very materiality of the thing under investigation. It is made of the same stuff. It is a figuration that this stuff has been made to yield”.<sup>7</sup> This is the reason why we look at it and think we see nature directly. It is a kind of paradox that our knowledge of a preparations provenance makes us so easily forget its transformation.

This is important in an epistemological sense because the preparation’s participation in the materiality of the research object is the primary guarantee for the validity of the knowledge claims produced by studying such preparations. Preparations are artefacts produced through myriad manipulations. They are artefacts that *were once* facts, so to speak. And this temporal or residual ontological connection to a previous state of being – inside a human skull in the case of neuroscience – is what makes it possible to say something about living brains by studying prepared brains. In Rheinberger’s words: “the essence of organic preparations qua knowledge objects resides in this material complicity, which ensures their duration and the permanent possibility of their epistemic recall”.<sup>8</sup> As I introduce the concept of the ‘archive’ below, the importance of this feature will become clear.

Material complicity, or continuity, guarantees the epistemic value of preparations. On the other hand, too much continuity becomes problematic for political, judicial and economic reasons. Klaus Høyer has used the intentionally strange-sounding neologism *ubject* to point to the non-trivial process of turning something partaking in subject-hood into an object. In his work he shows the various ways in which bodily material is semantically, materially and institutionally disentangled from and entangled with ideas of person and body.<sup>9</sup>

Bronwyn Parry and Cathy Gere have investigated the judicial meaning of property claims in and commodification of human biological artefacts with a view to how ontology informs commodity: “What a ‘thing’ is – the nature of a ‘thing’ – or at least what it is determined to be – profoundly affects how that thing may

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<sup>7</sup> Rheinberger, 235.

<sup>8</sup> Rheinberger, 238.

<sup>9</sup> Klaus Hoeyer, *Exchanging Human Bodily Material: Rethinking Bodies and Markets* (Dordrecht ; New York: Springer, 2013).

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subsequently be used – economically, socially, lawfully and ethically”.<sup>10</sup> The problem with human tissues, in their analysis, is their “ontological indeterminacy”,<sup>11</sup> which like Høyer’s ‘ubject’ muddles the distinction between subject and object, making it very hard to reach a verdict on rights to usage and property, both legally and ethically.

Catherine Waldby and Robert Mitchell have made similar observations, but with a focus more on the networks, institutions and practices surrounding the exchange of tissues. They show among other things the important difference between gifted material and bought material, mirrored in differences between for-profit research, which involves patents and financial returns on donors’ bodily ‘investments’, and public research ‘for the benefit of mankind’.<sup>12</sup>

Such transformations and commodifications seem the more problematic the more the entity in question resembles a person. Biological stuff that is more anonymous and more commonplace – such as blood – is also typically less problematic than entities that are more obviously metonymic with a person. This may have become less pervasive with the advent of widespread knowledge of genetics, as the genome has come to challenge the brain as the ‘organ of the self’. Nevertheless, visually and metaphorically, such entities as brains, faces, and foetuses most clearly signify the personhood of the deceased.

A concrete example of the need to continuously alter the ontological status of collected brains is Carleton Gajdusek’s mid-century work on so-called ‘kuru’ brains from the indigenous Fore people of Papua New Guinea. Kuru was an unexplainable brain disease that seemed only to affect the small population of Fore, and Gajdusek devoted most of his career to researching this disease.<sup>13</sup>

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<sup>10</sup> Bronwyn Parry and Cathy Gere, ‘Contested Bodies: Property Models and the Commodification of Human Biological Artefacts’, *Science as Culture* 2, no. 15 (A 2006): 139.

<sup>11</sup> (*ibid.*)

<sup>12</sup> Catherine Waldby and Robert Mitchell, *Tissue Economies: Blood, Organs, and Cell Lines in Late Capitalism*, Science and Cultural Theory (Durham [N.C.]: Duke University Press, 2006).

<sup>13</sup> Warwick Anderson, *The Collectors of Lost Souls: Turning Kuru Scientists into Whitemen* (Baltimore: Johns Hopkins University Press, 2008); Warwick Anderson, ‘The Possession of Kuru: Medical Science and Biocolonial Exchange’, *Comparative Studies in Society and History*, no. 42 (2000): 713–44.

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In this case the personhood attributed to the brains proved beneficial to Gajdusek in some contexts, disadvantageous in others. Gajdusek became somewhat synonymous with kuru research. He profited by having developed very close relations to the Fore over the years, because of which the Fore allowed him to use their deceased relatives' brains in his research. This meant that to the outside world, the kuru brains were Gajdusek's brains. In order to become his brains, however, they had to pass through his lab:

The improvised bush laboratory became a local redoubt for the making or stabilization of scientific facts, the production of novel "epistemic things" [...]. Bodies became disaggregated into tissue and fluid samples, translated into scientific things which were then mobilized as lasting bits of valuable data.<sup>14</sup>

[...] yet never were these specimens completely detached from their origins; they retained an aura of the person from whom they came. Even when pulverized and distant from the site of extraction, Gajdusek's kuru brains were also identifiably the brains of Aoga or Tasiko or some other Fore person. Each brain, even in pieces, had a name.<sup>15</sup>

This is the central problem of preparations. In order to be valuable for research, they must be anonymised and objectivised to the extent that research can be reasonably carried out, and often so that the preparation can be said to represent a whole class of objects. However, knowledge of provenance and genealogy of the material is crucial in pathological brain research where nosography, diagnosis, and anamnesis are continually co-developed, and inheritable disease is often involved.

There are many interests in play in the fashioning of preparations. Warwick Anderson's work on Gajdusek, as well as Høyer's, Waldby & Mitchell's, and Parry & Gere's, is full examples of political, personal, ethical, and financial motivations and factors. As extensive as these works are, however, one thing is not covered by their analyses: How one might understand the preparations after they leave the lab, as they in many cases enter into a collection of some sort or other. In order to

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<sup>14</sup> Anderson, *The Collectors of Lost Souls*, 76.

<sup>15</sup> Anderson, 109–10.

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approach this question, I now begin to think about how to understand biological collections as archives.

## 2 THE ARCHIVE

The field of archive studies is full of disagreement. Some scholars follow Jacques Derrida and Michel Foucault in broadening the concept of “archive” to include veritably anything that has a past, or as a metaphor for memory collective or individual.<sup>16</sup> Others insist on reserving the word for its more common sense signified: a specific institution charged with storing, preserving, and making accessible documents deemed to be of historical importance.

The archive can be likened to a tomb, to the death drive of the anal retentive, an apparatus for halting time, a radically conservative institution, a tool for subjugation and dehumanisation. Or it can be called constructive, creative, and the foundation for revolutions, the first thing real despots rush to destroy, the testament to the lives of those who had no voice, a place for taking back history from those who wish to control it. Few if any scholars completely buy in to any of such similes, but they are all recurring tropes, and their diversity shows the fertility of the archive as a theoretical and historiographical concept.

The collected volume *Archives, Documentation, and Institutions of Social Memory: Essays from the Sawyer Seminar* showcases the plethora of meanings and uses “the archive” can give rise to, as well as the wide variety of contexts and conditions there are for collection, curation, and use.<sup>17</sup> There is, of course, no “The Archive” definite singular. It is therefore necessary that I devote some space to developing exactly what I mean when I say I want to consider the brain collection as an archive.

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<sup>16</sup> Jacques Derrida and Eric Prenowitz, ‘Archive Fever: A Freudian Impression’, *Diacritics* 25, no. 2 (1995): 9–63, <https://doi.org/10.2307/465144>; Michel Foucault, *Archaeology of Knowledge*, Routledge Classics (London ; New York: Routledge, 2002).

<sup>17</sup> Francis Xavier Blouin and William G Rosenberg, eds., *Archives, Documentation, and Institutions of Social Memory*. (University of Michigan Press, 2011).

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### 2.1 Medicine as an archival science

It has long been common for histories of medicine to talk of the discipline's progression through different stages: From 'bedside medicine' to 'hospital medicine' to 'laboratory medicine'.<sup>18</sup> As any attempt at creating a historical 'big picture' this narrative has been challenged many times, but has also proven persistent.

John Pickstone has challenged the progressive narrative in the 'stages' version of history of medicine, opting instead to conceptualise different "ways of knowing", that do emerge in different points in history, but not as stages, and they do not take the place of each other. For Pickstone, laboratory analysis did not make other practices obsolete, it simply joined itself to the ever growing number of ways of knowing life and the world. Further, ways of knowing unlike paradigms or similar conceptualisations can work together in the production of knowledge, making it possible to maintain plurality and complexity while analysing scientific practices.<sup>19</sup>

Collecting is one way of knowing that has long played a role in medicine. A rough overview of collecting practices in the history of medicine could begin with Thomas Sydenham's collection of patients' journals and their conglomeration into *nosology*, in which a disease is defined as a collection of essential symptoms extrapolated from numerous sick persons believed to suffer the same illness.<sup>20</sup> That illness could be 'the same' across sufferers, rather than individual reactions to humoral imbalances; this idea is made possible by the collection of data about sick people. Such published collections of disease histories, or "contributions to the collective store of observations",<sup>21</sup> also indicate the emergence of a collective medical profession.

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<sup>18</sup> see N. D. Jewson, 'The Disappearance of the Sick-Man from Medical Cosmology, 1770-1870', *Sociology* 10, no. 2 (1 May 1976): 225-44, <https://doi.org/10.1177/003803857601000202> for a classic example.

<sup>19</sup> John V. Pickstone, *Ways of Knowing: A New History of Science, Technology and Medicine* (Manchester: Manchester University Press, 2000).

<sup>20</sup> cf. William F. Bynum, 'Nosology', in *Companion Encyclopedia of the History of Medicine*, ed. William F. Bynum and Roy Porter (Taylor & Francis, 1997), 335-56.

<sup>21</sup> Pickstone, *Ways of Knowing*, 68.

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From the seventeenth century physicians have established post-mortem collections of pathological body parts. This meant that a new way of knowing the sick body became (slowly) available. Instead of afflicting the whole person, some diseases came to be recognised as localised in specific body parts. These two knowledge-forms, symptomatic (from nosology) and organ-specific (from post mortem collections), were institutionalised in the Paris hospitals, which would group together patients with similar symptoms, and established specialised wards dedicated to the care of particular kinds of disease (e.g. respiratory diseases), gradually focusing on particular organ groups (e.g. the lungs).

The concentration of cases in medical wards in the Paris hospitals facilitated the introduction of statistical methods into medicine. Pierre-Charles-Alexandre Louis is famous for having developed the ‘numerical method’ in medical research. He walked the wards of the Hôpital de la Charité, where he collected thousands of case histories, which formed the basis for his statistical analyses of phthisis and typhoid fever and of the efficacy of bloodletting. In 1832 he became co-founder of the Société Médical d’Observation, a group dedicated to the new method for numerical analysis. Though Louis’ own methods were crude, criticised by his peers, and fairly quickly abandoned, statistical analysis had by then become a standard way of knowing in the medical profession.<sup>22</sup>

Given the importance of statistics even today, for example in randomised controlled trials, and given that statistics radically rely on the collection of data, it seems strange that collection practices have been understood as ‘passé’ after the advent of laboratories in medical science. However, statistical collection of data is far from pathological tissue collections, not to speak of archives.

Bruno Strasser has argued very convincingly that collection practices continue to play an important role in modern life sciences.<sup>23</sup> The most crucial part of his argument is, to my mind, that scientific collections are always instituted with the

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<sup>22</sup> Erwin Heinz Ackerknecht, *Medicine at the Paris Hospital, 1794-1848* (Baltimore: Johns Hopkins Press, 1967), 102-4.

<sup>23</sup> Bruno J. Strasser, ‘Collecting Nature: Practices, Styles, and Narratives’, *Osiris* 27, no. 1 (1 January 2012): 303-40, <https://doi.org/10.1086/667832>; see also Robert E. Kohler, ‘Finders, Keepers: Collecting Sciences and Collecting Practice’, *History of Science* 45, no. 4 (1 December 2007): 428-54, <https://doi.org/10.1177/007327530704500403>.

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purpose of being *used*. Thus also in contemporary science: “the stellar rise of the experimental life sciences in the twentieth century obscured the fact that their success was not necessarily the result of experimental practices, but emerged also, as I argue, from practices centered on collections”.<sup>24</sup> Strasser’s analysis builds explicitly on Pickstone’s “ways of knowing”, by which he is able to see the integral connection between collections and laboratories rather than upholding a historian’s analytical distinction between them.

Karin Tybjerg has followed Strasser’s claim, and showed the central and continued importance of collections in medical science,<sup>25</sup> beautifully demonstrated in Medical Museion’s exhibit *The Body Collected*. The exhibit features medical collections from the earliest *wunderkammer*-like preserved specimens of “nature’s wonders” through nineteenth-century anatomical collections to today’s biobanks of DNA.<sup>26</sup> Tybjerg’s comments on the pasts and futures that manifest themselves in collections are noteworthy:

By comparing medical museums, often associated with the past, with biobanks, often associated with the future, new aspects of both become apparent. Biobanks are and will inexorably become related to the past as the persons and patients from whom the samples derive age. This will be felt in a very practical manner when registration and storage systems need to be updated. Conversely, in its own time, the medical museum was made for the future – for doctors to diagnose new patients on the basis of knowledge from the past. Both anatomical-pathological collections and biobanks thus provide not only material links to the past, but also windows to the future.<sup>27</sup>

Tybjerg, along with Strasser, stresses this feature of collections: That they are made with a purpose, a future, in mind, and that they construct this future with the

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<sup>24</sup> Strasser, ‘Collecting Nature’, 322–23.

<sup>25</sup> Karin Tybjerg, ‘From Bottled Babies to Biobanks: Medical Collections in the Twenty-First Century’, in *The Fate of Anatomical Collections*, ed. Rina Knoeff and Robert Zwijnenberg, *The History of Medicine in Context* (Farnham, Surrey; Burlington, VT: Ashgate, 2015), 263–78.

<sup>26</sup> Karin Tybjerg et al., *Det indsamlede menneske: lægevidenskabens råmateriale fra kadaver til DNA* (Kbh.: Medicinsk Museion, 2016).

<sup>27</sup> Tybjerg, ‘From Bottled Babies to Biobanks: Medical Collections in the Twenty-First Century’, 278.

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present as its past: that which will have been. The practice of collection thus commingles tenses in making the material present into a future past.

This became evident in a recent book, *Science in the Archives* with the subtitle *Pasts, Presents, Futures*.<sup>28</sup> Tenses commingle and become non-trivial in archives and collections, and conceptions of time play a crucial role in determining the usefulness and viability of maintaining a brain collection.<sup>29</sup> According to Daston:

scientific archives generally share two properties: they are opportunistic and open-ended. These two features stem from the same root cause, the unpredictable development of research agendas. No one knows in advance what questions future historians or climatologists will pose and what traces from the present (and whatever of the past has already been preserved) will be needed to answer them.<sup>30</sup>

That archives are opportunistic means that their purpose is ever shifting. Documents once thought to be of no value may suddenly be pivotal in a new research question. This means that the epistemological value of established archives is all but impossible to determine. It also explains why archivists are generally interested in collecting everything they can. The ethos of the archive is that anything and everything *may* become valuable, and therefore must be preserved. As stated above, this ethos springs from a specific understanding of temporality, in which the future always relies on the past, but is also radically other from it, and therefore it is impossible to guess at how the present or past will be of use to the future. To reiterate a point from above, biological preparations are always open to epistemic recall, which further complicates foreseeing their future use. In other, prevalently older, cases, the opportunism of the archive is evident in the shifts in collection practices, as the epistemic hopes and anxieties of the time determined what kinds of material and information was collected.

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<sup>28</sup> Lorraine Daston, ed., *Science in the Archives: Pasts, Presents, Futures* (Chicago; London: The University of Chicago Press, 2017).

<sup>29</sup> See Thomas Erslev, 'A Brain Worth Keeping? Waste, Value and Time in Contemporary Brain Banking', *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 67 (1 February 2018): 16–23, <https://doi.org/10.1016/j.shpsc.2017.12.002>.

<sup>30</sup> Lorraine Daston, 'Introduction', in *Science in the Archives: Pasts, Presents, Futures*, ed. Lorraine Daston (Chicago; London: The University of Chicago Press, 2017), 5.

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That archives are open-ended means that besides not knowing *for what* they can be used in the future, we also cannot know *how*: Which technologies will make possible novel uses that may be relevant to an entirely foreign scientific field from what was the original archivists'. It also means that things that have never before been considered archival become archives with the development of new epistemic systems and research questions, such as it has happened with "DNA and the fossils embedded in the earth's stratigraphy, metaphorically described as nature's archives".<sup>31</sup>

Daston's introduction and epilogue to the volume<sup>32</sup> make much of this feature of the unknown future utility, perhaps taking the rhetoric a bit too far, as we learn that "time is the archive's enemy" because they are material entities that rot, decay, and are forgotten if not used. However, "time is also the archive's friend. At any moment, a new research question – Is this really a brand new disease? Were there mass extinctions? Does dark matter exist? – can rouse a Sleeping Beauty archive from its slumbers".<sup>33</sup> This pathos, I think, too easily hides the practices of the archivists and gives too much agency to Science, which takes the role of Prince Charming swooping in to kiss the Princess back to life. Daston herself is aware of the importance of archivists, as she notes that

Without continuity of practices, the archive would not just slumber from time to time; it would sink into a coma. Stable practices of collecting, selecting, canonizing, scrubbing, and ordering data insure that the contents of the archives are commensurable and retrievable.<sup>34</sup>

In the Danish case, a lack of just such practices between 1982 and 2006 had all but rendered the brain collection in a coma, and it took researchers more than a decade to revitalise the collection and make it usable again.

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<sup>31</sup> Daston, 7.

<sup>32</sup> Daston, 'Introduction'; Lorraine Daston, 'Epilogue: The Time of the Archive', in *Science in the Archives: Pasts, Presents, Futures*, ed. Lorraine Daston (Chicago ; London: The University of Chicago Press, 2017), 329–32.

<sup>33</sup> Daston, 'Epilogue: The Time of the Archive', 330.

<sup>34</sup> Daston, 331.

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Besides the overall framework of *Science in the Archives*, which gives the reader an impressive understanding of the way scientific archives work and are used, the myriad forms they take, and the diverse politics surrounding them, one chapter is especially relevant here as it makes the argument that nosology – the classification and description of diseases – is fundamentally an archival discipline.<sup>35</sup> Mendelsohn argues that many “new” diseases discovered in the last two centuries (he lists Hodgkin’s disease, leukaemia, Stokes-Adams disease, coronary occlusion, Graves’ disease, stomach perforation) were not “discovered” by the bedside by a single physician coming across an exemplary case, but rather constructed from a library of published but not analysed cases. This practice of publishing “raw” case histories, bare facts, Mendelsohn argues, is archival exactly because it is open-ended and opportunistic. In this practice basically everything is published in periodicals, which aren’t necessarily meant to be read instantly, but rather

it was to say: I don’t know – what exactly these histories mean, what they add up to. And yet just as importantly: Someday (through this very activity) you will, so I write them to you, whoever you are, out there in the future of medicine. [...] These publications] gave medicine a future – not a future in some future innovation, but a future founded in the (practices of the) present.<sup>36</sup>

This could happen, Mendelsohn argues, because the library was an archive of the unknown as much as it was a collection of knowledge. Data mining goes far back in the history of medicine.

Warwick Anderson has claimed that the files in the archive are less important than the authority the archive provides:

The authority of the clinical archive seems to depend more on its organization of paper technology, its serial disposition of individual cases, than on the retrievable contents of any file. [...] Certainly in the hospital the archived file did not do much, but

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<sup>35</sup> J. Andrew Mendelsohn, ‘Empiricism in the Library: Medicine’s Case Histories’, in *Science in the Archives: Pasts, Presents, Futures*, ed. Lorraine Daston (Chicago; London: The University of Chicago Press, 2017), 85–109.

<sup>36</sup> Mendelsohn, 96–97.

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the presence of an archive meant a lot. It provided a sort of authorization.<sup>37</sup>

This may be the case in hospitals, where the primary interest is therapeutic, but archives, as Mendelsohn has shown, play a very important role in medical *research*. The authority the archive brings to the clinic or the hospital is partaking in the authority of science. Medical science depends on archival practices. It guarantees the profession's claim to knowledge, and it is in many cases the prime site for furthering that knowledge.

### 2.2 The Danish brain collection as archive

The Danish brain collection is archival in many of the ways described above. It was collected because an opportunity presented itself. The necessity of post-mortem analysis in psychiatric medicine called for an institution such as HPI, and medicine having a long tradition of collecting and compiling, it was only natural that the autopsied brains be stored and researched. It is open-ended, as the diverse research topics presented in chapter 4 make clear, and it probably was so from the very start in the institutors' intentions, considering the lack of collection criteria: they wanted it all, not necessarily to analyse now, but as an archive for the future.

The recent debates surrounding its closure have been full of the same concerns that are present when other kinds of archives are threatened. On the one hand, there are people warning that the collection is irreplaceable and, because archival, its value cannot be estimated on the grounds of current research output. On the other hand, there are arguments against its usefulness, also presented in the register of mingled tenses. The concerns are several: Whether the material is of sufficient quality, or if its older form of preservation is incompatible with modern science. Whether the accompanying metadata are comprehensive enough that they might be of any use. Whether the patient's journals and pathological reports can be retrieved, or if it would be too costly to locate and collect them from various

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<sup>37</sup> Warwick Anderson, 'The Case of the Archive', *Critical Inquiry* 39, no. 3 (1 March 2013): 546, <https://doi.org/10.1086/670044>.

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archives around the country. Finally, the publication output from the collection has been low.<sup>38</sup>

The question at the centre of these concerns is, ‘does the collection belong to the past or to the future?’ In other words, is past uses (the publication record) an indication of future usefulness? Has the material become obsolete? Can future scientists make use of case files compiled according to long abandoned practices? Moreover, are the ethics of today to determine how we value past actors’ actions and attitudes?<sup>39</sup> These questions, in which either the past determines the future or the future determines the past, come increasingly into focus in the second half of the thesis, which also engages more specifically with the concept of ‘value’.

By concentrating my research, my readings, and my analysis around the concept of the archive, these questions that seem otherwise so far removed can be treated as part of the same problem; the problem of the open-ended, opportunistic, and always precarious archive.

### 3 MULTIPLICITY

The material and archival nature of the collection will serve as the nexus around which my thesis is constructed. In order to comprehend the collection properly, however, we must acknowledge that it has led and is leading several somewhat separate lives. It plays different roles and means different things if considered epistemologically, ethically, politically, socially, etc. To approach this problem, I follow Annemarie Mol who in *The Body Multiple* showed that a multiple ontology of the body in medical practice is not only possible, but also necessary if we want to grasp theoretically what is already happening in practice.

In this section, I detail how I understand multiplicity, and how it informs the structure of my thesis. I begin by outlining the multiple ontologies in the neurosciences generally, and then retrace Mol’s argument to understand this

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<sup>38</sup> See chapters 6 and 7.

<sup>39</sup> see Duncan Wilson, ‘A Troubled Past? Reassessing Ethics in the History of Tissue Culture’, *Health Care Analysis* 24, no. 3 (1 September 2016): 246–59, <https://doi.org/10.1007/s10728-015-0304-0> for an argument against judging the past by contemporary standards.

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multiplicity as productive and positive. Finally, I argue that to reduce the brain collection to one thing, to limit it to one context, would be unproductive.

The neurosciences developed since Cajal's discovery of individual neurons, have been full of disciplinary, methodological, and ontological diversity. Early histologists argued about what they saw in individually produced preparations that only lasted seconds and could not be reviewed, but needed to be drawn quickly to reach the scientific community. They got so caught up in trying to fixate the images they saw, that many of them denied the possibility of a plastic nervous system.<sup>40</sup> Simultaneously, cytoarchitects also used histological methods to produce intricate correlations between cell structures and functions, and draw maps of brain areas based on the concentrations of cell types.<sup>41</sup> Electrophysiologists sparked squid nerves, and focused intensely on the synaptical connections between neurons,<sup>42</sup> as did neuro-endocrinologists, although their methods led them to very different conclusions.<sup>43</sup> More recently, the brain has become a digitized, probabilistic object as imaging technologies render voxelated conglomerations of "supernormal" brains.<sup>44</sup> These different understandings of the brain did not supersede one another, making previous insights obsolete. Rather, they exist to this day alongside each other.

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<sup>40</sup> Stahnisch, 'Making the Brain Plastic'.

<sup>41</sup> K. Brodmann, *Vergleichende Lokalisationslehre der Grosshirnrinde in ihren Prinzipien dargestellt auf Grund des Zellenbaues* (Leipzig : Barth, 1909), <http://archive.org/details/b28062449>; Constantin Freiherr von Economo and Georg N. Koskinas, *Die Cytoarchitektonik Der Hirnrinde Des Erwachsenen Menschen* (Wien und Berlin: J. Springer, 1925), [https://catalyst.library.jhu.edu/catalog/bib\\_124152](https://catalyst.library.jhu.edu/catalog/bib_124152).

<sup>42</sup> Stadler, 'Circuits, Algae, & Whipped Cream'.

<sup>43</sup> Elliot S. Valenstein, *The War of the Soups and the Sparks: The Discovery of Neurotransmitters and the Dispute over How Nerves Communicate* (New York: Columbia University Press, 2005).

<sup>44</sup> Anne Beaulieu, 'Voxels in the Brain: Neuroscience, Informatics and Changing Notions of Objectivity', *Social Studies of Science* 31, no. 5 (1 October 2001): 635–80, <https://doi.org/10.1177/030631201031005001>; Anne Beaulieu, 'Images Are Not the (Only) Truth: Brain Mapping, Visual Knowledge, and Iconoclasm', *Science, Technology, & Human Values* 27, no. 1 (1 January 2002): 53–86, <https://doi.org/10.2307/690275>; Anne Beaulieu, 'From Brainbank to Database: The Informational Turn in the Study of the Brain', *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 35, no. 2 (June 2004): 367–90, <https://doi.org/10.1016/j.shpsc.2004.03.011>.

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This dissimilarity has also been noted inside the neuroscientific community. The sociologically inclined neuroscientist Steven Rose lamented in 2005 the disparity of different brains being studied. It is worth citing him in full:

Naming ourselves neuroscientists doesn't of itself help us bring our partial insights together, to generate some Grand Unified Theory. Anatomists, imaging individual neurons at magnifications of half a million or more, and molecular biologists locating specific molecules within these cells see the brain as a complex wiring diagram in which experience is encoded in terms of altering specific pathways and interconnections. Electrophysiologists and brain imagers see what, at the beginning of the last century, in the early years of neurobiology, Charles Sherrington described as 'an enchanted loom' of dynamic ever-changing electrical ripples. Neuro-endocrinologists see brain function as continuously being modified by currents of hormones, from steroids to adrenaline – the neuromodulators that flow gently past each individual neuron, tickling its receptors into paroxysms of activity. How can all these different perspectives be welded into one coherent whole, even before any attempt is made to relate the 'objectivity' of the neuroscience laboratory to the day-to-day lived experience of our subjective experience? Way beyond the Decade of the Mind, we are still data-rich and theory-poor.<sup>45</sup>

Rose's worry here is that the brain has simply become too many things, too many simplifications and reductions, for neuroscientists to have a conversation across disciplines. If every neuroscientist speaks only of his or her narrowly constructed epistemic thing, which relies on subtractions and additions to 'raw' nature, it is impossible to talk about the brain as a holistic organ. When Rose says we are "data-rich and theory-poor", he means unified theory. Every neuroscience lab has theories, but they often relate to a subset of "the brain" more generally: A specific disease, a reaction pattern in a single area, a developmental stage in language acquisition, the function of a single molecule. Many of these theories can be confirmed in lab settings, and over the decades, neuroscientists have amassed much knowledge about very specific functions and mechanisms in the brain. They are data-rich. However, when confronted with principle questions, such as 'What

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<sup>45</sup> Steven P. R Rose, *The 21st Century Brain: Explaining, Mending and Manipulating the Mind* (London: Vintage, 2006), 5.

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is the substrate of reason?’ or ‘What produces consciousness?’ every sub-discipline within neuroscience points to its own epistemic thing as the *sine qua non* for the brain writ large. When Rose says “we are theory-poor”, he means the neuroscientific community have no shared theories for how the brain works. Most handbooks of neuroscience presents knowledge of the brain in chapters devoted to specific functions, pathologies, or mechanisms (e.g. hormones, cell structure, memory, language). These subjects are approached through different methodologies, and rarely if ever are they synthesised into a coherent theory for how the brain works.

The brain, like all bioscientific epistemic things, depends on the manipulations and preparations that it is subjected to. Different material arrangements reveal different images and allow different interventions, as Rheinberger has shown. This in turn informs the contexts in which the different brains are made to appear. In Rheinberger’s terminology, the experimental system and the epistemic things are co-constitutive. Annemarie Mol’s anthropology of arteriosclerosis allows us to broaden the context beyond the experimental system, and include arrangements that are more social. In her study, she follows the object that is arteriosclerosis through its permutations in a Dutch hospital. Like the brain, depending on who investigates, and which manipulations they perform, the object becomes radically different things. In the GP’s office, the disease is difficulty walking and pain in specific areas. In the post mortem lab, it is a visible and tangible blockage in the artery of a sectioned leg. To the vascular surgeons, it is likewise a blockage, but one that requires intervention in a live limb, and which is only visible on an X-ray before the arteries are laid bare. To the haematologists, it is more a symptom than a disease, signifying high cholesterol levels in the blood. Epidemiologically it is an entity correlating with a range of variables including age, sex, smoking and eating habits, etc. To the patient, the disease is primarily a hindrance to ordinary use of their body, which in some cases might lead to crises of identity.

This disparity, according to Mol, is not a problem in and of itself. In fact, she argues, it is productive in the hospital setting, because it allows for case based judgments, individualised treatment options, and patient involvement that would

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be hard to imagine in a rigid one-definition understanding of the disease.<sup>46</sup> Simultaneously, arteriosclerosis as a boundary object allows medical disciplines that are normally far removed from one another, to come together and exchange knowledge regarding a common, but not commonly understood object. Thus, multiplicity is not only therapeutically productive, but also epistemologically.

Going back to Rheinberger, it is a well-established observation that an epistemic object, as far as it is still under study, is never fully defined. In order to grasp an entity, we must be able to simplify it, to reduce it to some core qualities or concepts. But before such a simplification is reached, scientific investigation leads to a proliferation of possible ways of being, an always open list of defining qualities. Change one, and both the object and its context changes with it. Until we fully understand the brain – simplistically in a Bachelardian sense – it must remain multiple, inhabiting several possible states of being at once.<sup>47</sup>

The brain collection and its contents similarly to Mol's arteriosclerosis has moved through different contexts, met different actors, occupied different spaces, and has undergone its own changes each time such a shift took place. The chapters of my thesis present the different and sometimes discrete lives of the brain collection; some of them lived in close proximity, some never converging.

#### 4 CONCLUDING REMARKS

The brain archive is contested. It is the object of disputes crossing the borders of ethics, epistemology, and politics. Historians of ideas are wont to historicise such contestations in order to understand their complexities and causes. But the brain collection is already being historicised. Whether its history is told by wide-eyed fascinated youths on the playground (as it was when I attended public school), by neuroscientists, by politicians, or by religious authorities, its meaning and use are dependent on a historical narrative of mingled tenses. To understand the brain

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<sup>46</sup> Annemarie Mol, *The Body Multiple: Ontology in Medical Practice*, Science and Cultural Theory (Durham: Duke University Press, 2002).

<sup>47</sup> Rheinberger, *Toward a History of Epistemic Things*, chap. 2; Nikolas S. Rose and Joelle M. Abi-Rached, *Neuro: The New Brain Sciences and the Management of the Mind* (Princeton, N.J.: Princeton University Press, 2013).

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collection, I argue, we must not only historicise it, but also chart the myriad uses of its history; trace the pasts and futures ascribed to it by a wide range of actors.

When HPI began collecting, there was no one cause. Some hoped to come closer to an understanding of the brain's normal function through study of pathological cell changes. Others were interested in the ways such a collection could help practicing psychiatrists as a diagnostic tool. For yet others the collection was a means to perfecting their histological skill. Most actors probably believed that such a grand collection would somehow gain value in the future. Today, however, its institution is called a waste of resources by some, an ethical monstrosity without regard for human life by others, or an act of incredible foresight by its proponents.

The collection is all these things dependent on the kind of narrative it is inscribed into. If the past seventy years are taken as an indication of its usefulness, the archive may not have been worth the investment. If the future is made to look promising with novel technologies and a return to the study of material brains the collection becomes invaluable. If the focus is not on investigative technologies but on methods of preservation, the collection is beginning to look like a mere relic of the past, of use to none but historians. If the time of the archive is felt to swallow up the time of lived life, it becomes a terrible sepulchre for the weakest of society.

Pasts determine the future, and futures determine the past in the archive. The tenses of "has been", "is", and "will be" cannot be kept apart. When actors mingle epistemic anxieties with existential fears, it is time and history that ultimately determines the verdict. In the Ethical Council, the ethical transgressions of the past were measured against the personal narratives of kin, and the archival timeframe of the collection. The promises of the future were deemed greater and more valuable than righting wrongs that were not wrong when they were performed.

The materials of the archive inhabit the past; they guarantee epistemic congruity much as Rheinberger's preparations are only epistemically productive as far they partake in the materiality of that which they represent. In order to maintain this link, and their value, the materials must remain as unaltered as

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possible, but only when used do they have actual value. The archival infrastructure is geared towards the future. By continually following epistemic trends and updating its systems and metadata accordingly, the archive may not only accommodate future science (or await it like Daston's *Sleeping Beauty*), but actively shape it. It is all too common that archival methods and practices is the last thing to be updated. *Sleeping beauty* is thought to lie in wait no matter the amount of thorn bushes grow up around her. But the matchmaking of a good archivist might be necessary to make proper use of the archive.<sup>48</sup>

The Danish brains belong neither to science, to ethics, or to policy. They cross borders and muddle distinctions. This is to my mind what makes the archive such a useful concept in studying them. Because the archive as a concrete object brings together pasts, futures, and present hopes and anxieties, it can make visible the narratives that structure complex attitudes not reducible to any one branch of knowledge or society. An actor's view of the mingled tenses of the archive informs their ethics, their epistemology, their politics, and perhaps even their ontology.

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<sup>48</sup> cf. Kathleen Marquis, 'Not Dragon at the Gate but Research Partner', in *Archives, Documentation, and Institutions of Social Memory.*, ed. Francis Xavier Blouin and William G Rosenberg (University of Michigan Press, 2011), 36–42.



## *Chapter 2*

### Pre-collection: Histological hopes

In order to understand the collection's development and significance, it is worth looking at its provenance. I do not here mean the provenance of the individual brains, but the original scientific idea formation from which the collection began. As noted in the introduction, the collection was instituted by two professors at Aarhus University, Erik Strömngren and Lárus Einarson. Strömngren was involved mainly in the administrative side of the collection's institution, as I will show in chapter 3. Einarson is the protagonist of this chapter.

Lárus Einarson (1902-1969) was something of a local celebrity at the time. He had come to Aarhus from his birth town Reykjavik in Iceland. After receiving his medical degree from the University of Iceland in 1928, he spent some years in different positions in Denmark, Germany, USA, and Iceland, before becoming professor of anatomy at Aarhus University (AU) in 1936. Established in 1928 with 64 students and teaching exclusively in philosophy and languages, AU had constituted its medical faculty in 1935, and Einarson thus became its first ever professor of anatomy. Beyond teaching and research, he had to shoulder the responsibility to build this subject from the ground at AU. That the university was so young, and the professors largely independently responsible in their own fields is worth keeping in mind, as it carries some explanation of the relatively swift and unquestioned establishment of HPI.

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An avid and technically skilled histologist, Einarson had developed his own staining method in 1932 for specific staining of the Nissl substance and the nucleus of brain cells, and had since worked on understanding these cell structures. He was particularly interested in the changes in stainability that followed certain experimental manipulations of *in vivo* tissue. In other words, he wanted to see if changes occurring while the experimental subject was alive would be visible in the microscope after death. This research programme led, among other things, to the founding of the HPI, which would afford Einarson a steady supply of research material for his studies.

This chapter introduces, in section 1, Einarson's scientific project, which shaped practice in HPI for the first two decades of its existence.<sup>1</sup> The chapter also presents Einarson as an interesting historiographic case, who shaped a historical narrative around his technologically informed science. This is the subject of section 2. Both as a historical actor and as a writer of historical narrative, Einarson is a kind of 'odd one out'. To better understand his position, I review the standard narrative in the historiography of brain science, and argue (along the lines of Tobias Rees), that it is best understood as a perspectival 'history of truth', a kind of teleological history determined by an accepted scientific position. Finally, I compare this standard history with Einarson's own history of truth.

#### I EINARSON'S BRAIN SCIENCE: FUNCTION THROUGH STRUCTURE

In 1945, months after establishing HPI, Einarson published a long review in Danish of the state of his field, including the history of the development of the neuron theory and brain cytology.<sup>2</sup> This 140-page piece is a valuable historical document because it gives us Einarson's own understanding of his context. In it, he listed his inspirations and theoretical opponents; he praised some earlier brain researchers, because he saw similarities in their work or life with his own situation, and treated

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<sup>1</sup> In chapter 4, I show the legacy of Einarson's research in the first decades of HPI.

<sup>2</sup> Lårus Einarson, *Om Nervecellernes Indre Struktur Og Deres Histologiske Tilstandsændringer Ved Eksperimentelt Fremkaldte Funktionelle Aktivitetsstadier*, Acta Jutlandica Supplementum 17 (København: Universitetsforlaget i Aarhus, Ejnar Munksgaard, 1945).

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others only cursorily if they did not bear direct import on his career or scientific understanding. The document shifts between the registers of hope, doubt, excitement, and sobriety as Einarson reflected on the status of brain science, on what had been achieved, and how much was yet to be done. The reader feels Einarson's great pride and sense of breakthrough as he presented his own methodology for correlating structure and function, as well as his frustration that other researchers had largely ignored his results. This first section of the chapter will delve into a detailed reading of this one publication, letting it stand in for his larger production, which basically continued the same project.<sup>3</sup> While it does not amount to a complete history of neuroscience (if one such was possible), it is valuable because it supplies Einarson's own context and situates him within it.

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<sup>3</sup> Lárus Einarson and Knud Aage Lorentzen, *Om Nervecellernes Indre Struktur Og Deres Tilstandsændringer under Irritation, Inaktivitet Og Degeneration*, Acta Jutlandica - Aarsskrift for Aarhus Universitet 18 (Copenhagen: Universitetsforlaget i Aarhus, Ejnar Munksgaard, 1946); Lárus Einarson, 'Om Galloctyanin-Kromalunfarvningens Theori, Belyst Ved Hjælp Af Nervecellerne Som Objekt', *Ugeskrift for Læger* 109 (1947): 143-49; Lárus Einarson, 'Notes on the Histochemical Aspects of the Changes of the Spinal Motor Cells in Anoxia, Vitamin E Deficiency and Poliomyelitis', *Acta Orthop Scand* 19 (1949): 55-85; Lárus Einarson, 'On the Internal Structure of the Motor Cells of the Anterior Horns and Its Changes in Poliomyelitis', *Acta Orthop Scand* 19 (1949): 27-54; Lárus Einarson, 'Galloctyanin-Chromalum Staining of Basophilic Cell Structures.', in *Microscopic Technique in Biology and Medicine*, ed. EV Cowdry (Baltimore: Williams & Wilkins Co., 1951); Lárus Einarson, 'On Diffuse Brain Sclerosis and Its Histopathogenetic Relationship Especially to Amaurotic Idiocy', *Acta Psychiatr. Neurol. Scand. Suppl* 74 (1951): 180-83; Lárus Einarson, 'On the Basophilia of Nerve Cells and Its Quantitative Estimation by Staining with Inner-Complex Dye-Metal Salts', *Acta Psychiatr. Neurol. Scand. Suppl* 74 (1951): 85-87; Lárus Einarson, 'On the Theory of Galloctyanin-Chromalum Staining and Its Application for Quantitative Estimation of Basophilia. A Selective Staining of Exquisite Progressivity', *Acta Pathol Microbiol Scand* 28 (1951): 82-102; Lárus Einarson, *Critiquing Review of the Concepts of the Neuro-Muscular Lesions in Experimental Vitamin E Deficiency, Preferably in Adult Rats*, Acta Psychiatrica et Neurologica Scandinavica, Supplementum no. 78 (Copenhagen: Ejnar Munksgaard, 1952); Lárus Einarson, 'Om Fluorescerende, Syrefaste Stoffer i Nervesystemet Hos Voksne Rotter i Kronisk E-Vitaminmangel', *Ugeskrift for Læger* 114 (1952): 1186-90; Lárus Einarson, 'Deposits of Fluorescent Acid-Fast Products in the Nervous System and Skeletal Muscles of Adult Rats with Chronic Vitamin E Deficiency', *J Neurol Neurosurg Psychiat* 16 (1953): 98-109; Lárus Einarson, 'Structural Changes and Functional Disturbances in the Nervous System. Diffuse Brain Sclerosis', *Anatomiske Skrifter* 1, no. 2 (1954): 25-51; Lárus Einarson, 'Structural Changes and Functional Disturbances in the Nervous System. Neuromuscular Lesions in Vitamin E-Deficient Adult Rats', *Anatomiske Skrifter* 1 (1954): 1-23; Erik Krogh and Lárus Einarson, 'Nucleic Acid Metabolism in Nerve Cells under Different Forms of Activity and Hyperactivity, Shown by the Galloctyanin-Chromalum Method', *Anatomiske Skrifter* 1, no. 2 (1954): 65-79; Lárus Einarson and Erik Krogh, 'Variations in the Basophilia of Nerve Cells Associated with Increased Cell Activity and Functional Stress', *J Neurol Neurosurg Psychiat* 18 (1955): 1-12.

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Einarson's historical account began with Theodor Schwann, "an amicable and charming gentleman, who often enjoyed life on the sidewalk in front of the small cafés of Louvain".<sup>4</sup> Schwann published his work in 1839, in which he established the cell as the "structural and functional base element of all animal tissue".<sup>5</sup> Almost simultaneously, Remak and Purkinje, whose favorite cafés (or first names) Einarson did not mention, identified respectively the unmyelinated nerve fibres (Remak's ganglia) and "small bodies in the nervous tissue, from which extended thread-like runners"<sup>6</sup> (a type of neuron later dubbed Purkinje cells). It was "the great physiologist Helmholtz", Einarson continued, who collected these different observations and concluded that the threads really did protrude from the nerve cells. Kölliker established the unipolarity of the spinal ganglia (the nerve cells in the spine that receive sensory inputs from extremities, etc.) and identified myelinated nerve threads originating from these cells. Ranvier in 1875 demonstrated the T-split of the spinal ganglia dendrites, and Wagner found that one runner (the axon) did not branch out, but did establish connection with other cells. In Einarson's view, in the period from Schwann until the 1880s "the central nervous system became a chaos of cells and connective threads, while real understanding of its structure, and therefore also its function, remained absent".<sup>7</sup> The scene was set for Golgi and Cajal, Einarson's histological heroes who would develop the methods to look beyond this chaos and into the individual cells.

Camillo Golgi's black reaction, a method for staining that gives very clear images of the nerve cells' outer shapes, their branching and interconnections, led to "a total upheaval of earlier ideas and concepts about the structural elements and architectural connections of the nervous system".<sup>8</sup> Similarly, Paul Ehrlich's discovery of the staining qualities of methylene blue became ground-breaking in other cytological fields, but, according to Einarson, ultimately confused neuroscientific research. This was because tissues impregnated with this stain showed continuity between the dendrites and axons of distinct neurons, which

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<sup>4</sup> Einarson, *Om Nervecellernes Indre Struktur Og Deres Histologiske Tilstandsændringer Ved Eksperimentelt Fremkaldte Funktionelle Aktivitetsstadier*, 8.

<sup>5</sup> Einarson, 8.

<sup>6</sup> Einarson, 8.

<sup>7</sup> Einarson, 9.

<sup>8</sup> Einarson, 9.

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then seemed like they were not distinct, but actually one very large, continuous cellular structure. Finally, Santiago Ramón y Cajal, “whose investigations more than any other were of such crucial importance for the discovery of the neuron as a structural and functional unit in the nervous system, and the development of the neuron theory” famously disproved this idea of anastomoses (fusion) between nerve cells, leading to the identification of the synapse.<sup>9</sup> Einarson’s introductory history of the discoveries preceding Cajal concludes with a quote from American anatomist Lewellys F. Barker on Cajal’s early career:

[Cajal had] applied for a position in microscopy, which was refused him. His pride wounded keenly, he renounced his social relations, purchased a small library on histological subjects, paid special attention to certain technical methods, worked like a slave at his subject, and a decade later found himself famous.<sup>10</sup>

While he admitted the likely falsehood of this account, Einarson throughout his writings fashioned himself like the mythical Cajal in the style of the lone genius, slaving away on underappreciated subjects, waiting for his work to be discovered.

Following these introductory pages, Einarson spent 42 pages on a thorough review of the outer and inner structure of the nerve cells, with special attention to the neurofibrils and the Nissl substance. Like his introduction, this review is full of heroic portrayals of great discoveries. Especially interesting, for what it says about Einarson’s view of the science, is his attention to technique and his hierarchy between anatomy and physiology. Einarson was dutifully detailed in his descriptions and evaluations of the *technical* histological discoveries (stains, cuts, instruments) that often in his account preceded and eclipsed theoretical progress. Another recurrence is Einarson’s insistence that knowledge of the structure of the nervous system would eventually lead to insight into its function. For example, he began both his account of the neurofibrils and the Nissl substance with a thorough account of who discovered them, where they are located, and their structural relation to nearby entities, before even mentioning their function, and even then remaining somewhat agnostic on this matter. He also expressly opined the necessity of studying structure before function. “Nowhere else”, he wrote, “is the

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<sup>9</sup> Einarson, II.

<sup>10</sup> Einarson, II.

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correlation between structure and function achieved as fully” as in the neurons, and the only way to further the understanding of the nervous system’s function, he believed, was “through the study of their inner structure.” Now, this was no simple endeavour, as he made clear: “To this end, we require new methods that will allow us to penetrate deeper into the nerve cells’ interior, and study the structure of the cytoplasm that they are made of”.<sup>11</sup> As his review showed, previous histologists had made some progress in this respect. The final step, however, would be taken by Einarson himself, and his collaborators, by way of his own new method

Overall, Einarson’s brain science was one in which refinement of histological technique rendered ever clearer pictures of the intricate structure and interrelation of elements, which then made possible the deduction of function. This is evident from his historical account, which is largely adapted to follow this trajectory with technical innovations and structural insight preceding functional theories. It also set the stage nicely for the subject proper of his text, which was his own histological technique, and his method for viewing functional alterations in histological post-mortem structural images.

#### **1.1 Einarson’s method: Structural activity changes and staining reactions**

Lárus Einarson’s main achievement was his development of the so-called ‘gallocyanin chrome alum’ stain.<sup>12</sup> It was a technical breakthrough because it *only* stained those parts of the cell that Einarson wanted to study. Essentially, it stained the same cell parts as the method developed by Franz Nissl, another of Einarson’s heroes. Nissl published his staining method in 1885. It relied on dousing the tissue slice in a basic dye (originally methylene blue), which dyes the whole slice blue, and then “differentiating” the stain in aniline oil, which removes dye from the less stainable areas, until only the parts of interest remain blue. The problem with this method, Einarson informed his readers, was that “the quality of the preparation relies on the individual researcher’s estimation of when the optimal staining

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<sup>11</sup> Einarson, 22.

<sup>12</sup> Larus Einarson, ‘A Method for Progressive Selective Staining of Nissl and Nuclear Substance in Nerve Cells’, *The American Journal of Pathology* 8, no. 3 (May 1932): 295-308.5.

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intensity has been achieved”.<sup>13</sup> Einarson’s method relied on other chemical principles than Nissl’s, which meant that no such differentiation was necessary. Whereas staining with methylene (and similar dyes such as toluidine, thionine, cresyl) relied on a “physical” dyeing, simply covering the desired elements in a blue or violet dye, Einarson’s method formed entirely new chemical compounds when the solution reacted with the molecules in the cell. The staining compound, galloxyanin chrome alum, bound specifically to the nucleic acids (RNA and DNA), and left all other parts of the cell unaffected. At the same time, the chemical binding was so strong that once dyed, the colour could not be removed but through great difficulty.

Beyond the obvious benefits to such a stain in terms of efficiency and ease of use, the key importance for Einarson was the elimination of the subjective element in the staining process. A more objective stain had the added benefit of showing variation in between not only different cell types, but between the same cells from different subjects (whether animal or human), thereby making them comparable. In other words, when the staining intensity no longer relied on the subjective estimate of the researcher, differences in stainability had to be a reflection of some real difference between the cells. A recurrent problem in histology has been whether what was seen in the microscope reflected real *in vivo* structures, or if what it showed was artifice – a product of the many manipulations the tissues were subjected to before microscope viewing. This problem also attended the Nissl bodies, as some researchers did not believe them to be real structures. Einarson summarised it thus:

Ever since Nissl published his original technique for microscopic investigation of the nervous cells, and thereby initiated a new epoch in the study of their inner structure and pathology, the problem has persisted if the basophilic Nissl bodies, as they show in fixed and dyed preparations, are artifices entirely contingent on the effect of the used fixative, or if they are actually a preformed, *in vivo* existing structure. This

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<sup>13</sup> Einarson, *Om Nervecellernes Indre Struktur Og Deres Histologiske Tilstandsændringer Ved Eksperimentelt Fremkaldte Funktionelle Aktivitetsstadier*, 56.

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question has been in the foreground as one of the most important histological problems.<sup>14</sup>

This was a crucial moment in Einarson's work. His entire project relied on proving that variations in stainability were the result of *in vivo* variations between individual cells, rather than contingent artifices produced in the lab. This is evident from the considerable amount of snark with which he treated proponents of the artifice theory. For example, he credited two histologists called Hardy and Fischer with inciting this damaging belief:

that all the intracellular, cytoplasmatic structures seen in fixed and stained preparations were artifices produced by the fixative. [...] *to be blunt, they were in the process of flattening out our concepts of the living protoplasm, to the point that it more and more resembled a slimy splodge of egg-white lipid solution with other compounds mixed in randomly.*<sup>15</sup>

He rebutted that since then it had been concluded that the prepared tissues do in fact reflect the condition of the living nerve cells.<sup>16</sup>

Einarson's big hope for his method was that it would provide the epistemic nexus between structure and function. If neurohistology was to move on from mere mapping of cell structures and be able to say something about the function of the cells, some kind of translation was necessary between the post mortem microscope picture, and the living cell. Einarson believed he had found this translation in the stainability of the cells, which seemed to change with altered function of the living cells. For example, in 1933 he conducted a study with rabbits, which he manipulated experimentally to induce a running movement where the reflexes made one side of the body tense up and the other side relax. In some cases, Einarson could make the animal stay fixed in such a position. Then, he wrote,

By killing the animal in such a prolonged fixed position and immediately fixate the spinal cord for subsequent histological examination, I was able to show that the rigid contracted extensors [muscles] on one side had been innervated by highly chromophobic [hard to stain] cells, while the completely relaxed

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<sup>14</sup> Einarson, 79.

<sup>15</sup> Einarson, 85–86 [cursive in original].

<sup>16</sup> Einarson, 87.

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extensors on the other side had been innervated by either strongly or extremely chromophilic [easy to stain] cells.<sup>17</sup>

As the quote shows, Einarson distinguished between chromophilia (literally “affection for colour”; easily stained) and chromophobia (literally “aversion to colour”; hardly stained). These concepts referred to the ease with which the nucleus and Nissl substance received his stain – how deep the final stain was, and how much ‘stuff’ in the cell was affected by it.

Since first perfecting his stain in 1932, and using it for pathological examination in 1933, he had developed a schematic for linking these microscope pictures with abnormal functions of brain cells. Normal, resting cells (also called chromoneutral or parapyknomorph cells) were the baseline. From there, he constructed a scale in both directions in three steps, “light, medium strong, and extremely strong chromophobia”, and likewise for chromophilia.<sup>18</sup> This scale was possible because the ‘gallocyanin chrome alum’ stain bypassed subjective variations in staining intensity, so that variations could be said to reflect changes inherent to the cells. However, the scale itself relied in high degree on the expert judgement of the researcher.<sup>19</sup> Einarson therefore had to be very specific in his descriptions of *what to look for* when defining these stages, as in the following:

Lightly and medium strength chromophobe cells are somewhat broader and with more rounded contours, the Nissl substance is more pale, more scattered, and there is plenty of unstained cytoplasm between individual Nissl bodies, though these often are more closely arranged around the nucleus. The dendrites are almost uncoloured. In strongly chromophobe cells the Nissl substance is very strongly reduced, completely disappeared around the nucleus but often with clearly visible remains in the periphery. The dendrites are not coloured at all.<sup>20</sup>

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<sup>17</sup> Einarson, 103.

<sup>18</sup> Einarson, 99.

<sup>19</sup> For the history of distinction and overlap between objectivity and judgement, see Lorraine Daston and Peter Galison, *Objectivity*, Paperback ed (New York, NY: Zone Books, 2010). Einarson’s method can be seen as an interesting instance in which a mechanically secured objectivity is still dependent on what Daston & Galison call ‘trained judgement’, where it is the researcher’s training that guarantees the objectivity of an observation.

<sup>20</sup> Einarson, *Om Nervecellernes Indre Struktur Og Deres Histologiske Tilstandsændringer Ved Eksperimentelt Fremkaldte Funktionelle Aktivitetsstadier*, 99.

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Einarson then continued, describing in detail the different pictures of the nucleus in the three stages, and then proceeded to do the same for chromophilia.<sup>21</sup> He also supplied his reader with prints of exemplar types of each stage (Figures 4 & 5). To the uninitiated these very detailed, conscientious descriptions do not mean much, and determining when one is dealing with light and medium chromophobia may seem like guesswork. However, to an experienced histologist, these descriptions would refer to very well established differences, and though ultimately relying on a subjective judgement, this did not make the scale itself subjective. Even so, Einarson was not satisfied: “even with correct judgement, it is important to deliver proof that it really is so”.<sup>22</sup> He therefore constructed an elaborate apparatus from a photoelectric cell (an instrument to measure the intensity of light, and transform it to an electrical current. Used for example to harvest solar energy). In simple terms, he put this photoelectric receptor in front of the eyepiece of his microscope, so that it “saw” what was under the microscope’s lens. He then shone the microscope’s backlight at full intensity through the stained specimen, so that the intensity of coloration of the specimen worked as an aperture, allowing more or less light to pass through to the photoelectric cell. The cell’s received effect was then transferred to a galvanometer, a very sensitive instrument for measuring electrical currents. This measure was entirely dependent on the intensity of the staining of specific cell structures in the examined specimen. “Thus,” Einarson concluded, “we achieve a quantitative, numeric expression of the cell’s staining intensity, that is to say its degree of either chromophilia or chromophobia.”<sup>23</sup>

After Einarson had established the six stages of chromophobia/philia, he went on to pair each with an “activity stage” (Figure 3). As the example with the rabbits shows, these activity stages could be provoked experimentally, by forcing certain cells to work hard for prolonged time, while forcing others to relax entirely. The scale went thus: When activity began, the cells grew slightly and increasingly chromophilic, what Einarson described as “chromophily of initial activity”.<sup>24</sup> When this activity was prolonged, or increased through electrical irritation, the cells

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<sup>21</sup> Einarson, 102–3.

<sup>22</sup> Einarson, 115–16.

<sup>23</sup> Einarson, 116.

<sup>24</sup> Einarson, 102.

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switched over and became lightly chromophobic. From here, increased chromophobia correlated with increased activity, and the highest degree of chromophobia meant exhaustion – the cells die from overexertion. Extreme chromophilia did not represent an activity stage, but could be produced experimentally by inhibiting the cell’s activity, making it harder for it to perform even its resting functions. One such inhibiting method was withholding vitamin E. This was something Einarson’s protégé K.A. Lorentzen worked on, and I will go into further detail in chapter 4. Here, I will quote Einarson’s summary of this condition:

In the extremely chromophilic cells it is as if the nucleus’ entire production of nucleic acids and proteins becomes useless, as they are not excreted from the cell, but instead accumulate with the nucleus and the cytoplasm, so that the cell’s entire metabolism stagnates at the same time as the physiological inhibition of the neuron’s impulse discharges.<sup>25</sup>

Even resting cells have some degree of metabolism, just as the body processes do not cease entirely in sleep. Through induced vitamin E deficiency, these resting state processes could be brought to a halt, making it impossible for the cell to uphold its own existence, and unable to communicate with the cells around it. Extreme chromophilia, then, is not to be expected in normal activity changes, but represents a pathological condition.

<b>Figure 3</b>	
Staining intensity and activity changes according to Einarson	
Extreme chromophobia	Highest activity → exhaustion/depression
Med. strong chromophobia	Accelerated/prolonged activity → fatigue
Light chromophobia	Increased activity
Chromoneutral	Resting cell
Light chromophilia	Initial activity
Med. strong chromophilia	Initial activity
Extreme chromophilia	Inhibition (depressed activity) → sclerosis

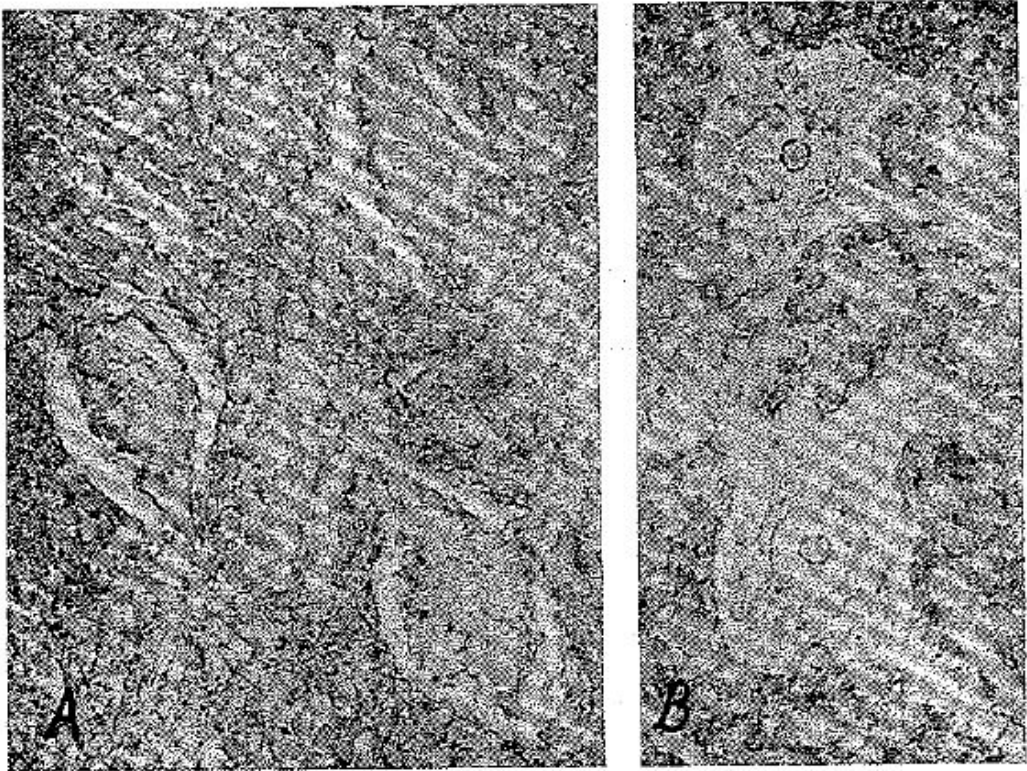
The promise of Einarson’s method was profound. His hope was that histologists like himself, who had been studying nothing but structure for six decades, could finally begin to say something about the function of the brain. This hope is most clearly evident from the way he positioned himself in the

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<sup>25</sup> Einarson, 104.

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historiographic review. In the next section, I devote some more attention to Einarson's historiographic ambitions, which I believe was closely connected with his scientific ambition, and I attempt to say a bit more about the purpose and character of internal histories of scientific disciplines, such as Einarson's.



**Figure 4**

Illustration from Einarson (1945, p 120). Rat neurons. These preparations have been stained with aniline, a standard stain at Einarson's time.

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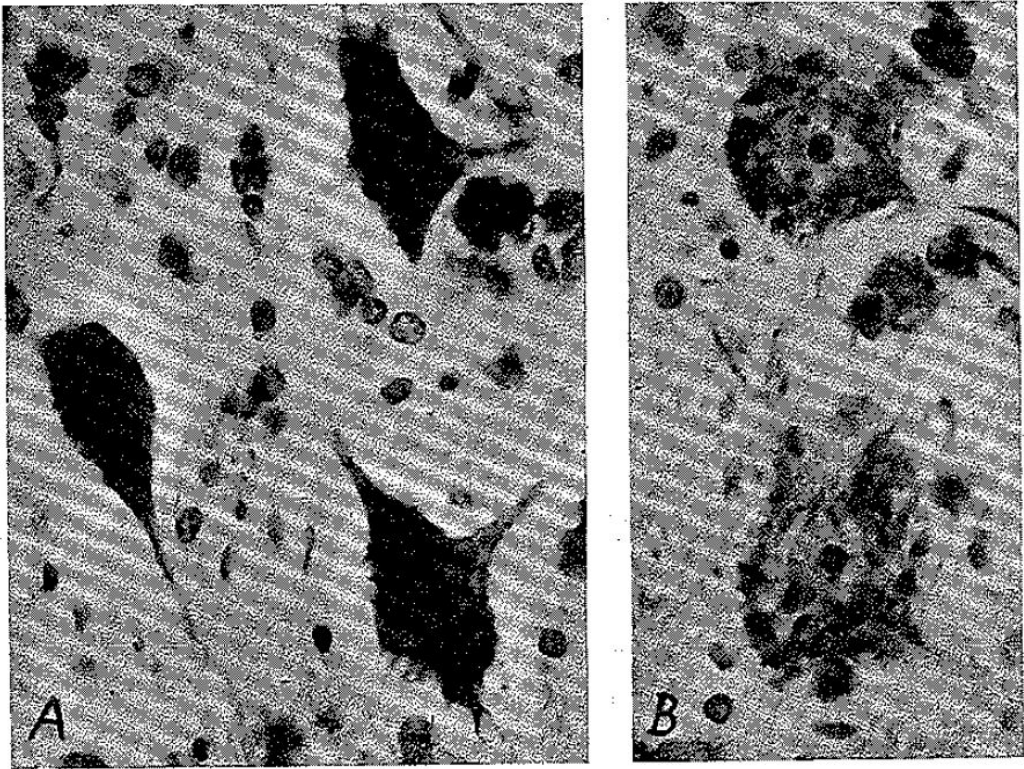


Figure 5

The same cells as in figure 4, now prepared in Gallocyenin Chrome Alum, according to Einarson's method. In this picture, the difference between the two samples becomes discernible. The left sample (A) show cells in chromophilia, while the right (B) shows chromophobic cells.

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## 2 HISTORIOGRAPHY

Einarson is historiographically interesting on two levels. First, because his historical review is a perfect example of what has been called “rewriting the history of truth”.<sup>26</sup> That is to say, Einarson was writing a teleological history of his discipline, in which he himself was the telos. Second, because this particular teleology looks so strange compared with the current standard narrative within the neuroscientific community, in which the technological, theoretical, and therapeutic revolutions of the 1950’s are considered the origin of modern neuroscience.

In this section, I consider Einarson’s place in the historiography of neuroscience. I will proceed by reviewing the ‘standard’ narrative (as far as there exists one such) of the genesis of modern neuroscience. I will then compare this narrative with Einarson’s, while introducing the concept *histories of truth*, with which I will reflect more broadly on the practice of internalist historiographers, who write the history of their own discipline from within their discipline.

### 2.1 Origin stories

The modern neurosciences are often traced back to one of two origins: the 1880s or the 1950s. 1888 is the year when Santiago Ramón y Cajal identified the nerve cell as a singular entity, initiating the so-called “neuron doctrine” in the sciences of the brain. 1960, sometimes including the preceding decade, is often identified as the year when “neuroscience” became a recognisable term, collecting dispersed theories, techniques, societies, and identities under one umbrella term anchored in the common object: the *neuro*, denoting a common object of study.

The latter narrative has been most expressly formulated by Gordon M. Shepherd in his book *Creating Modern Neuroscience. The Revolutionary 1950s*.<sup>27</sup> As suggested by the title, Shepherd’s book is a compendium of all the insights in

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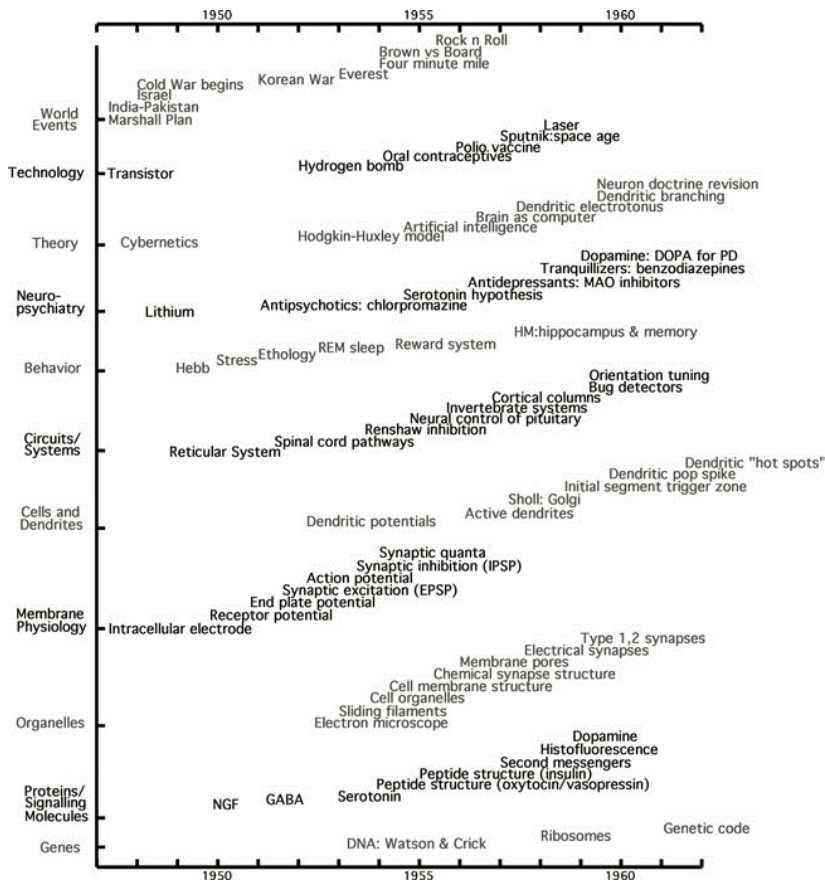
<sup>26</sup> Tobias Rees, ‘Histories of Truth’, in *Plastic Reason: An Anthropology of Brain Science in Embryogenetic Terms* (Oakland, California: University of California Press, 2016), 91–94.

<sup>27</sup> Gordon M. Shepherd, *Creating Modern Neuroscience: The Revolutionary 1950s* (Oxford ; New York: Oxford University Press, 2010).

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diverse disciplines that provides, in Shepherd's view, the *origins* of modern neuroscience. These revolutions, including DNA, behaviourism, synaptic action potentials, brain imaging, and psychopharmacology, are schematically summarised according to subspecialties.<sup>28</sup> The schematic in itself, with its linearly rising events, its jargon shorthand, and its categories that really only make sense from a retrospective internalist perspective, is a reliable synecdoche for the book as such.



**Figure 6**

Gordon M. Shepherd's schematic of the discoveries and revolutions in brain science in and around the 1950s.

<sup>28</sup> Shepherd, 12.

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Shepherd himself calls his book a collection of “essays rather than definitive historical accounts”. The latter he leaves for “professionally trained historians of science”, acknowledging that he, as an active researcher in the field, can contribute something different, which he likens to “the introductory chapter of a doctoral thesis, where the student absorbs everything about the history of the subject relevant to his or her research interests. In addition, it includes the lore of the field”.<sup>29</sup> He thus makes no claim to either exhaustive or definitive coverage, and he wishes to leave the narrative open for further interpretation and revision. Nevertheless, with such a title and subject matter, his book cannot but establish a powerful *grand récit* of a modern neuroscience that became its modern self through a series of visionary insights and fortunate discoveries made six decades ago.

The same story is found in accounts from outside the neuroscientific community as such. Nikolas Rose and Joelle M. Abi-Rached in their 2013 book *Neuro*, dated “the birth of neuroscience” to 1962 when, according to them, “a number of distinct lines of thought and practice seem to come together to create a difference.” In their brief review of this convergence, they distinguish “three intertwining pathways that seem to intersect in that event – the path through the nerves, the path through the brain, and the path through madness”.<sup>30</sup> The path through the nerves follows the etymology and use of the word *neuron*, and its uptake in the names of societies, disciplinary names, journals, and so on. From there, on to the development of the entity *neuron* into a singular, meaningful object of study, which functions and borders were finally well-defined after the “war of the soups and the sparks” in the 1950s, in which the chemical view of synaptic neurotransmission superseded the electrical theory.<sup>31</sup> The ‘modern’ *neuro-* (as both scientific and social concept) is then available by the end of the 1950s.<sup>32</sup> Rose & Abi-Rached’s path through the brain sees multi- and interdisciplinarity as a vital component of modern neuroscience, and it follows the development of brain research from distinct medical specialties into a coherent subject in need of its own name and community. They see this established in 1961, when UNESCO’s

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<sup>29</sup> Shepherd, 13.

<sup>30</sup> Rose and Abi-Rached, *Neuro*, 31.

<sup>31</sup> Valenstein, *The War of the Soups and the Sparks*.

<sup>32</sup> Rose and Abi-Rached, *Neuro*, 31–32.

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International Brain Research Organisation became an independent body.<sup>33</sup> Their third path, through insanity, follows the investigation of psychopathology through examination and intervention in the brain. From nineteenth-century pathological anatomists, through psychosurgery and lobotomy to the formation of a community of biological psychiatrists, the final stretch of this pathway goes through the development of the first psychopharmacological treatments in the early 1950s.<sup>34</sup> It ends with the neurochemical theory of mental disorders, which would have neuroscientists “understand the action of actual or potential psychiatric drugs in terms of their actions on the secretion, breakdown, depletion, and reuptake of these amines in the synapses or the receptors”.<sup>35</sup> In other words, the theory held that imbalances in neurotransmitters like serotonin, dopamine, noradrenaline, and acetylcholine were responsible for mental disorders. The authors pinpoint the emergence of this overarching theoretical vision in the early 1960s.<sup>36</sup> Finally, Rose & Abi-Rached also include evidence external to neuroscientific theory proper. For example, the establishment of an international infrastructure for collaboration and coordination of neuroscientific research, which also culminated in the early 1960s.<sup>37</sup>

Tobias Rees, an anthropologist of brain science, has suggested yet a different way of arriving at 1960 as the year of a fundamental paradigm-shift in brain research. To him, the change is found in the way the object is conceptualised. Before World War II, he holds, there were still many competing and equally possible brains in play both theoretically and experimentally: The reticular brain, which was not made up of distinct self-contained cells, but rather was one continuous structure with tiny intercellular elements providing continuity, thus closing the synaptic gaps between neurons; the electrophysiological brain, which proponents wanted to prove the systematic electrical communication between nerve cells; the growing brain, which was to be understood in terms of cell development and composition in the growth of the neuronal forest; the

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<sup>33</sup> Rose and Abi-Rached, 32–33.

<sup>34</sup> Rose and Abi-Rached, 33–38.

<sup>35</sup> Rose and Abi-Rached, 37.

<sup>36</sup> Rose and Abi-Rached, 38.

<sup>37</sup> Rose and Abi-Rached, 38–41.

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cytoarchitectonic brain, where the form of each neuron was linked to its function; and the brain of localisation theory, where gross anatomical areas corresponded to bodily functions, rational abilities, or emotional states.<sup>38</sup> After World War II, according to Rees, this “conceptual and methodological heterogeneity” gradually gave way to the “synaptic brain”.<sup>39</sup> This happened in three stages. First, the idea of the synapse was globalised as a fundamental problem to be solved. Second, “in the late 1940s and early 1950s, the synapsis was chemicalized”.<sup>40</sup> This refers again to the battle of the soups and the sparks, after which consensus became that synaptic communication was chemical not electrical. And third, electron microscope studies of nervous tissue finally provided visual evidence of the synapse and, even more importantly, showed that all cells in all areas of the brain make synaptic connections, thus making the synapse key in understanding the brain’s functions – perhaps even more so than the nerve cells themselves. Rees concludes, “out of the multitudinous past, a single conceptualization of the brain emerged – the chemical, synaptic brain machine. In the late 1950s and early 1960s neurochemistry emerged as *the science of the brain*”.<sup>41</sup>

Three different accounts of modern neuroscience all place its origin somewhere close to 1960. Shepherd argues by sheer volume: the multitude of “revolutions” that took place in the 1950s is enough that a qualitative shift took place, which leaves post-1960 neuroscience radically different from what was before, which in turn is effectively rendered pre-modern. Rose & Abi-Rached attempted to trace at least three genealogies that come from different theoretical or practical places, but that convergence in a single event ca. 1962 to ‘give birth to neuroscience’ as it were. Rees sees the shift in the object itself, from plural to singular, from brains to brain both conceptually and methodologically.

What is interesting about Rees’ account of the emergence of *the brain* is that he himself frames it as a corrective to another narrative, which he was presented with in the course of his anthropological fieldwork in a French neuroscience lab.

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<sup>38</sup> Tobias Rees, *Plastic Reason: An Anthropology of Brain Science in Embryogenetic Terms* (Oakland, California: University of California Press, 2016), 72–74.

<sup>39</sup> Rees, 74.

<sup>40</sup> Rees, 75.

<sup>41</sup> Rees, 76.

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“According to the histories of neuronal research I read during my first months in Alain’s lab”, he writes, “Ramón y Cajal and Sherrington established that the brain is a synaptic organ. What followed was merely refinement.” In this narrative, once Ramón y Cajal had observed the neuron as a singular entity, and Sherrington had coined the term synapse for the communicative space between neurons, it was inevitable that all the other discoveries followed. It is a kind of history of science in which the initiating event of a paradigm shift is elevated to stand in for the shift itself. No matter that Ramón y Cajal’s contemporaries were undecided, and that it would be decades before Sherrington’s concept bore any practical significance. As far as we today should be concerned, the modern neurosciences were born in 1888, and all the theories and experiments that followed has simply been its growing up, hitting puberty, and becoming a fully formed discipline.

Not only among the scientists in Alain’s lab were such narratives prevalent. In the 1990s, before 1960 became the accepted birth-year of neuroscience, a number of books took a much longer view on the discipline’s origin. Edwin Clarke & Stephen Jacyna wanted to “trace the origins in the first half of the nineteenth century of certain anatomical and physiological concepts that have proved fundamental in the human neurosciences”.<sup>42</sup> They argue through a number of conceptual historical chapters “that by 1850 the foundations of modern neuroscience had been laid”.<sup>43</sup> Again, the authors here attempt to find the earliest identifiable instance of something akin to a modern concept or theory, and cast the history of neuroscience as one long refinement of these early ideas. Stanley Finger, now professor emeritus of psychological and brain science, wrote his monograph *Origins of Neuroscience* after he had begun to introduce “historical interludes” in lectures and conference talks, and received positive feedback and enquiries for more information on the history of neuroscience.<sup>44</sup> His book, much like Gordon Shepherd’s *Creating Modern Neuroscience*, is ordered according to modern subspecialties. His main subdivisions are: “theories of brain function”,

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<sup>42</sup> Edwin Clarke and L. S. Jacyna, *Nineteenth-Century Origins of Neuroscientific Concepts* (Berkeley: Univ. of Calif. Pr, 1987), Preface.

<sup>43</sup> Clarke and Jacyna, 1.

<sup>44</sup> Stanley Finger, *Origins of Neuroscience: A History of Explorations into Brain Function* (New York: Oxford University Press, 1994), ix.

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“sensory systems”, “motor functions”, “sleep and emotion”, “intellect and memory”, “speech and cerebral dominance”, and “treatments and therapies”. Unlike Shepherd, however, Finger finds origins all throughout the ages, beginning with anthropological studies of the skulls of early hominids. Each chapter proceeds chronologically and goes back to the beginning as it were, to where Finger finds the first mention (often in ancient Egypt) of whatever is the subject at hand. The book is a classical piece of internalist history of science, where theories progress seemingly by themselves, through the actions and words of some prominent big thinkers, and where social context is often reduced to entertaining anecdote. Finally, Gordon M. Shepherd’s first historical monograph, *Foundations of the Neuron Doctrine*, hails the twin event of first Cajal and his late 1800s contemporaries’ discovery of the neuron as an individual entity, and the 1950s electron microscopy-aided confirmation and expansion of the neuron doctrine.

Somewhat crassly perhaps, what these books all have in common, beside subject matter and often source material, is their conviction that once a “true” concept or theory has been presented, even if in rudimentary form and with a limited reception, the work that remains is simply refinement. The theory itself, because true, is predetermined to prevail, and the path to modern knowledge was there in some form from the first conceptual origin. In recent years, this kind of history of neuroscience has been challenged by trained historians of science, who present less streamlined narratives in favour of a more detailed engagement with the crises and junctions, as well as the messiness and plurality of brain science.<sup>45</sup>

### 2.2 Einarson’s history of truth: yet another origin

As a historical actor, Einarson would seem to be out of place in the synaptic origin story. He was active ‘in between’ periods of interest – too late for the *fin de siècle*

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<sup>45</sup> I am thinking especially of the volume by Stephen T. Casper and Delia Gavrus, eds., *The History of the Brain and Mind Sciences: Technique, Technology, Therapy*, Rochester Studies in Medical History (University of Rochester Press: Rochester, NY, 2017), but also; Katja Guenther, *Localization and Its Discontents: A Genealogy of Psychoanalysis and the Neuro Disciplines* (Chicago: The University of Chicago Press, 2015); Fernando Vidal and Francisco Ortega, *Being Brains: Making the Cerebral Subject*, First edition, Forms of Living (New York: Fordham University Press, 2017). For an attempt at a synthesising review of this trend, see Alfred Freeborn, ‘The History of the Brain and Mind Sciences’, *History of the Human Sciences* 32, no. 3 (1 July 2019): 145–54, <https://doi.org/10.1177/0952695118815554>.

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revolution, too early for the 1950s breakthrough. His methods did not redefine the neuron doctrine, nor did it lay the ground for any modern neuro-discipline. In narratives such as Finger's, Shepherd's, and Clarke & Jacyna's he would be but a mere curiosity, someone who followed a wrong hunch and ultimately got it wrong. However, when considered as a historiographer in his own right, Einarson is strikingly like these authors. Not because they share a narrative – they don't – but because they share a narrative style; the one that has been called history of truth. It is from Tobias Rees that I borrow this term.<sup>46</sup> He introduces it, with a nod to Georges Canguilhem, to explain the new histories of brain plasticity that began to be written during the course of his field work. I think it is worth quoting at length:

Canguilhem observed that every major scientific discovery – that is, a discovery that generates a far-reaching new insight – seems to require the scientists who make it to rewrite the history of their discipline. It is, he explained, as if in moments in which a genuinely novel truth emerges – and a formerly well-established one gets discarded – the history of truth has to be rewritten.

[...] with plasticity research the brain had outgrown the history of truth that until recently had structured the very idea of progress around which the neuronal sciences evolved. The telos of this history was the synapse. [...] As there was now a new telos – plasticity – there had to be new origins and, with them, new histories of truth.<sup>47</sup>

While I think the term is very useful, and Rees applies it very well to his own case, I do not agree with his reading of Canguilhem. Rees would reserve the need for rewritten histories of truth to those discoveries that “generates a far-reaching new insight”. This, however, is something that can only be judged in retrospect. Canguilhem, as I read him, did not reserve this kind of history writing for successful scientists. In the text that Rees refers to, he expresses it thus:

Celui qui parvient à un résultat théorique ou expérimental jusqu'alors inconcevable, déconcertant pour ses pairs contemporains, ne rencontre aucun soutien, faute de communication possible, dans la cité scientifique. Et parce que,

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<sup>46</sup> Rees, 'Histories of Truth'.

<sup>47</sup> Rees, 91–92.

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savant, il doit croire en l'objectivité de sa découverte, il recherche si d'aventure ce qu'il pense n'aurait pas été déjà pensé.<sup>48</sup>

The central quality here, that which prompts a scientist to write a new history of truth, is that, as a scientist he *must* believe the truth<sup>49</sup> of his own discovery. Histories of truth are not necessitated by the discovery's importance, or with its generation of far-reaching insights, at least not to anyone but its discoverer. The urge to rewrite the history of truth depends rather on the scientists' wish to assert the importance of their own research. Lacking contemporary interlocutors, scientists in this position turn to the past for intellectual approval. Already convinced himself, Einarson just like Rees' plasticity-researchers wanted to convince a larger scientific audience, and in order to this, he would paint himself as breaking new ground while continuing the work of some choice intellects who constitute the 'origin' of the telos that is his own contribution.

Einarson shared many of the heroes in his narrative with the more recent histories, and he did highlight Cajal's establishment of the split between neurons (the synapse) as a central and very valuable discovery. However, Einarson's project was cytological at the core. His method was very useful for studies of the intra-connections within the cell, but did little to aid the study of their inter-connections in synaptic meeting points. The bulk of his review, as mentioned, was dedicated to internal cellular structures, namely the neurofibrils and the Nissl substance. His history of truth began not with Ramón y Cajal, but with Schwann, not with the neuron but with the cell, simply. Also, where the historiographers in section 4.1 talked of "the neuron" as a singular abstract, because they were mainly interested in the interconnections between neurons, Einarson clearly distinguished between different types of nerve cells, and credited their individual discoverers and investigators accordingly. In Einarson's brain science, the

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<sup>48</sup> George Canguilhem, 'L'Objet de l'Histoire Des Sciences', in *Études d'Histoire et de Philosophie Des Science*, 4th ed. (Paris: Libraire Philosophique J. Vrin, 1979), 11.

<sup>49</sup> Canguilhem has 'l'objectivité de sa découverte' whereas Rees speaks of 'histories of truth'. Here, I follow Rees and write "truth" instead of "objectivity", in order to not confuse the issue with that of different kinds of objectivity, cf. Daston and Galison, *Objectivity*. Objectivity and truth of course are not generally interchangeable, but here I think Canguilhem's meaning can be retained without confusing the issue, by using 'truth'.

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distinction in type and internal structure of the cells was the key to understanding nerve system function. How much Nissl substance in this cell compared to that one? How does the concentration and position change in different circumstances? How do the neurofibrils react to stimuli, and how do they relate to the other parts of the cell? These intraconnections, and the differences between the internal structure of cells, Einarson believed, needed to be mapped in order to solve the question of how the brain works at large. Only after the multitude of cellular functions were understood did it make sense to move on to their inter-cellular functions in the synapses.

Certainly, Einarson was aware that other neuro-investigators had started down different alleys of inquiry. After his medical examination from the University of Reykjavik in 1928 he continued his studies abroad, first at the Anatomische Anstalt in Munich, then at the physiology lab at Harvard University, the Marine Biological Laboratory in Woods Hole, Massachusetts, and at the institute of anatomy at Johns Hopkins University, until returning to Scandinavia in 1932.<sup>50</sup> His first methodological article on Gallocyenin-Chrome Alum was published 1932, and lists his affiliations in Munich, Copenhagen, and USA, which further documents his global awareness of research.

One joint article with Erik Krogh lists the Stazione Zoologica in Naples as an affiliation.<sup>51</sup> Though it is unclear, Krogh was probably the one affiliated with the Naples station, but it documents Einarson's awareness of the institution. This is important because the Stazione was the second home of John Zachary Young FRS, and it is where he discovered the squid giant axon, which throughout the 1930s served as a model organism for electrophysiologists such as Alan Hodgkin and Andrew Huxley, who in the 1930s investigated the role of electricity in relaying nervous messages.<sup>52</sup>

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<sup>50</sup> Th. Blackstad, 'NEKROLOG: Lárus Einarson', in *Aarhus Universitet. Årsberetning 1969-1970* (Aarhus, Denmark, 1970), 6–9.

<sup>51</sup> Einarson and Krogh, 'Variations in the Basophilia of Nerve Cells Associated with Increased Cell Activity and Functional Stress'.

<sup>52</sup> A. L. Hodgkin and A. F. Huxley, 'Action Potentials Recorded from Inside a Nerve Fibre', *Nature* 144, no. 3651 (October 1939): 710–11, <https://doi.org/10.1038/144710a0>; Kenneth S. Cole and Alan L. Hodgkin, 'Membrane and Protoplasm Resistance in the Squid Giant Axon', *The Journal of*

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In 1939, Young discovered the squid giant synapse, which corresponded to its giant axon. This synapse is the largest chemical junction in nature and served as a model for the study of synaptic functions generally, because it was easy to observe. After World War II, Hodgkins and Huxley, along with John Eccles, would use this synapse as a model in their Nobel-prize winning research, which contributed greatly to the chemical-synaptic brain that was the telos in the histories reviewed in section 2.1. Though these studies came later than Einarson's 1945 review, he would certainly have been aware of the electrophysiologists' work in the 1930s, and with Young's 1939 *Philosophical Transactions* article, in which he introduced the squid's giant "discontinuous synapses which are perhaps more clear and easy to study than any yet described".<sup>53</sup> With this, Young clearly alluded to the promises of synaptic research, which already seemed inviting as a way to a deeper understanding of brain function.

Simultaneously, beginning in the early 1930s in Germany,<sup>54</sup> research with the electroencephalograph promised a physiological route to understanding the brain's function. This new technology likewise threatened the epistemic authority of classical histologists like Einarson when it came to the question of brain function. Historian of neuroscience Cornelius Borck has put it thus: "Electroencephalography turned the whole brain into an object that wrote electrical signals and thus paved the way for approaching, analysing, and explaining brain functions quite independently of anatomical and structural considerations".<sup>55</sup> Electroencephalographic studies could render printed graphs of electrical activity in the whole brain, thus seemingly visualising thought itself. The

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*General Physiology* 22, no. 5 (20 May 1939): 671–87, <https://doi.org/10.1085/jgp.22.5.671>; John Zachary Young, 'The Functioning of the Giant Nerve Fibres of the Squid', *Journal of Experimental Biology* 15, no. 2 (1 April 1938): 170–85; for a historical account, see Stadler, 'Circuits, Algae, & Whipped Cream'.

<sup>53</sup> John Zachary Young, 'Fused Neurons and Synaptic Contacts in the Giant Nerve Fibres of Cephalopods', ed. Edwin Stephen Goodrich, *Philosophical Transactions of the Royal Society of London. Series B, Biological Sciences* 229, no. 564 (25 May 1939): 465–503, <https://doi.org/10.1098/rstb.1939.0003>.

<sup>54</sup> Hans Berger, 'Über das Elektrenkephalogramm des Menschen', *Archiv für Psychiatrie und Nervenkrankheiten* 94, no. 1 (1 December 1931): 16–60, <https://doi.org/10.1007/BF01835097>.

<sup>55</sup> Cornelius Borck, 'How We May Think: Imaging and Writing Technologies across the History of the Neurosciences', *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 57 (June 2016): 116, <https://doi.org/10.1016/j.shpsc.2016.02.006>.

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resulting hype was not unlike that accompanying the modern *in vivo* imaging techniques, such as fMRI. Readers may remember the triumphant sense in the 1990s and early 2000s, that scientists were now finally able to see the brain ‘at work’ through these scanners. Besides their being imbued with high hopes and dreams of solving the brain puzzle, the graphic method of investigation and electrophysiology also share the pivotal trait that they investigate living brains (or parts of brains). Remember that a big part of Einarson’s project hinged on his being able to show that *post mortem* structural changes actually did reflect *in vivo* changes. Both the electrophysiologists and the electroencephalographers sidestepped this obstacle by experimenting directly on living tissue, either *in vitro* or *in situ*.

If Einarson’s project looks peripheral to modern readers, it is certainly not because he was unaware of the theoretical and methodological progress that took place in the world’s leading research centres. Rather, he spent years visiting top institutions, educating himself as to the state of the art of neuroresearch in the 1930s. This knowledge adds yet another layer to Einarson’s historiography of brain science. Rather than blindly continuing a tradition that others would have called outdated, it seems to me Einarson consciously worked against these new theories and methods because he fervently believed, and indeed worked hard to prove, that his new method was the appropriate route to understanding brain function. In other words, Einarson’s history of truth can be read as a bulwark against the new methodologies that all made attempts at authoritative discourse on the brain. To counter these new trends, he would have to do two things at once: To establish his own technology as a groundbreaking novelty, which afforded unheard-of answers, and to establish the questions to these answers as the absolute core of brain research. He did this by praising previous histologists, and showing how especially their new techniques had helped further their theories, all the while noting exactly where these techniques came short, allowing his own to fill the holes. Whether or not previous histologists had been aware of these shortcomings, or would even consider them as such, in Einarson’s narrative the gallocyanin chrome alum stain was both the natural culmination and an unexpected breakthrough in this long history of truth.

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**3 CONCLUDING REMARKS**

The purpose of this chapter has been to establish the intellectual backdrop for the brain collection. I have done this by way of a review of Lárus Einarson's scientific methods and ambitions, as well as his historiographic self-understanding portrayed in his disciplinary review. In addition, I have shown that his historiography is similar to other histories of neuroscience, including recent ones, in that they try to establish a 'history of truth' on the basis of their respective theoretical beliefs about the nature of the brain.

Einarson based his history of truth on his own specialty: histological methods. His Gallocyenin chrome alum stain was a radical improvement to previous stains, both in terms of reproducibility (read: objectivity), ease of use, and for what could be reasonably inferred from a stained slice. It made a kind of 'reading' of the tissue possible that had previously been dismissed as fiction, explained away with reference to experimenter-induced artifice in the slices, which contaminated the pure data. These new readings, including measurements of volume and position of nucleic acids, led Einarson to over-estimate the significance of his own stain, as he came to consider it the most plausible route to an understanding of brain function through structure.

Hence, his history of truth neglected other burgeoning fields of neuroscience, notably electrophysiology and detailed studies of the synapse, in favour of classical post mortem histology. Many new disciplines had begun looking at living tissue, either in petri dishes or still attached to a body. Researchers in these fields believed the understanding of function had to come from *in vivo* studies, because they made it easier to infer direct causation from an experimental intervention (e.g. electric impulses). Einarson stuck to post mortem studies, though exactly why he did so remains unclear. If we follow Canguilhem, the answer is that, because he was a scientist, Einarson had to believe in his own theory and defend it until the last objection.

Whether this is the explanation or more prosaic reasons also played a role, Einarson's case is not singular. In neuroscience there has been, and still is, a tendency to intense specialisation. Often this is a specialisation in method,

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technique, and subject matter. However, as the quote from Stephen Rose showed in the previous chapter, these specialisations more often than not result in theoretical diversity also. Each specialty builds a theory of the brain that is based on their experimental engagements with it, effectively reducing the whole organ to a narrow epistemic thing. This tendency is clearly demonstrated with regard to Einarson, for whom the brain became its cells, and the cells became their constituent matter.

Nucleic acids were the ‘atoms’ of Einarson’s brain science. Understand their behaviour and we would understand the brain writ large. It is worth remembering in conclusion that Einarson never coupled nucleic acids with inheritance. To him, they were indicators of the cells’ functional activity stages, but he did not know their specific function. Crucially, in historiographic concern, Einarson’s methods or contributions were not disproven. His results were sound, only his predictions of the future were wrong. He is an example of a scientist working hard at the cutting edge of science and achieving good results, but who to modern readers looks to go against progress, because his paradigm was superseded soon after.

A case like Einarson’s reminds us that scientific progress is not inevitable, and that there is no natural progression in theoretical insight or indeed technical capabilities. Progress and knowledge creation are contingent upon many factors, and it is not at all obvious which route is the most promising from the vantage point of contemporary science. It is difficult, standing at the technological cutting edge, to imagine scientific progress might take a different route in just a few years. We do well to keep this in mind when writing histories of science, and to include episodes in which hopes and ambitions did not ultimately bear fruit.

With regard to HPI and its the brain collection, it was to be yet another technological tool in Einarson’s project towards function from structure. After a decade of animal studies, it was an obvious next step to test his theories in human post mortem tissue. Erik Strömngren, who ran the psychiatric hospital in Risskov, had several interests in collaborating with Einarson. First, as I will show in the next chapter, he wanted to model the Risskov hospital on a German pre-war structure in which neuropathological histology was an integral part of psychiatry. Second,

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he shared Einarson's hope that the theory of structural activity changes could uncover some cytological mechanisms for brain diseases, and thus point to new forms of therapy.

## *Chapter 3*

# Quotidian work: Establishment

From its inception, besides being anything else, the brain collection was a place of work, with all the triviality and day-to-day practicality that this involves. In this chapter, I want to present examples of the different kinds of work that took place at HPI. These examples will serve to show the mundane and traditional underpinnings of the collection. Several 21st-century commentators (see chapters 5-7) have called the institutors of HPI bold and forward thinking – which they were in some respects. However, the way the collection was run, and the attitudes of its director, seem more in line with classical pathological collections, than with a proto-biobank.<sup>1</sup>

The perspective in this chapter comes from the ‘turn to practice’ in the history of science. Since the late 1970s it has been increasingly recognised that a comprehensive historical understanding of science must include its practical and social aspects alongside more internalist histories of theoretical development. To this end, historians of science have incorporated methods from sociology and

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<sup>1</sup> cf. Tybjerg, ‘From Bottled Babies to Biobanks: Medical Collections in the Twenty-First Century’.

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anthropology, and focused increasingly on what scientists<sup>2</sup> do<sup>3</sup> to what,<sup>4</sup> with whom,<sup>5</sup> and where they do it.<sup>6</sup>

With this chapter, I acknowledge that an account of the quotidian practices of scientists is crucial for the understanding of a scientific institution. This is especially true for HPI, where the scientific output has been limited because, as I will show, the ambitions of the actors involved lay in areas outside publication, namely in the furthering of practical knowledge, artisanal skill, and *autopsia* in the practice of psychiatry and among medical clinicians more largely.

I begin, in section 1, with a description of Dr Knud Aage Lorentzen's work. As the director of HPI, Lorentzen personally carried out or oversaw any work tied to the collection, and his ideas about the science and vocation of pathology were pivotal in moulding its practice and purview. This section builds mostly on oral history interviews with K.A. Lorentzen's son Martin Lorentzen, who also became a pathologist and shared his father's work for a brief period in his youth.

These interviews show a man who archived brain tissue not for the sake of novelty or with some imagined future in mind. Rather, he did it because it was what thorough, conscientious pathologists had always done, because he believed the material tissue to be the foundation of medical knowledge, and because it was good practice with regard to the reproducibility of his findings. If hopes of some future use entered into his decision, it would only have been after these primary reasons, as an added benefit.

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<sup>2</sup> This word chosen for heuristic reasons. I include of course natural philosophers, technicians, illustrators, and all who engage in a broadly understood practice of 'science'.

<sup>3</sup> e.g. John V. Pickstone, 'Working Knowledges Before and After circa 1800: Practices and Disciplines in the History of Science, Technology, and Medicine', *Isis* 98, no. 3 (1 September 2007): 489–516, <https://doi.org/10.1086/521154>.

<sup>4</sup> e.g. Samuel J M. M. Alberti, 'Objects and the Museum', *Isis* 96, no. 4 (2005): 559–571, <https://doi.org/10.1086/498593>; Rheinberger, *Toward a History of Epistemic Things*.

<sup>5</sup> e.g. Paula Findlen, ed., *Empires of Knowledge: Scientific Networks in the Early Modern World* (London ; New York: Routledge, Taylor & Francis Group, 2019).

<sup>6</sup> e.g. Catherine M. Jackson, 'The Laboratory', in *A Companion to the History of Science*, ed. Bernard V. Lightman, Blackwell Companions to World History (Chichester, UK ; Malden, MA: John Wiley & Sons, 2016), 296–309; Robert E. Kohler and Jeremy Vetter, 'The Field', in *A Companion to the History of Science*, ed. Bernard V. Lightman, Blackwell Companions to World History (Chichester, UK ; Malden, MA: John Wiley & Sons, 2016), 282–95.

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Any scientific collection requires an extensive infrastructure for acquisition, thoughtful documentation and indexing, and proper storage conditions. Moreover, the collected items themselves must be handled correctly and their quality ensured. No less so in the brain collection. Section 2, therefore, describes the practice of fixation and transportation from the psychiatric hospitals to the brain collection. Much attention was given to the standardisation and uniformity of these procedures, to make sure that the collected brains were as comparable as possible and avoiding ‘artefacts of technique’. Sometimes the ambition to secure optimal brains went too far, as I show in an interview with a then junior doctor at Risskov, who felt pushed by her patriarchal superiors to procedures that made her uncomfortable.

Section 3 further investigates the storage and documentation infrastructure in the brain collection, and the instrumentation attendant on these endeavours, specifically the procurement of glass storage jars and equipment for microphotography. Negotiations in both these regards show a tendency at Risskov to prefer tried and tested, well known methods and instruments, rather than experiment with new equipment. This further illustrates the temporal alignment of the brain collection – it was not in its conception a collection for the future, but simply a continuance of pre-war science.

### I LORENTZEN’S WORK

The Institute of Brain Pathology (HPI) initially consisted of three persons: One assistant researcher (Erik Krogh), one scientific consultant (Lárus Einarsson), and one unnamed non-academic assistant. However, Krogh was already engaged at the time at the normal anatomical institute at AU, and soon had to resume this position.<sup>7</sup> His replacement, Knud Aage Lorentzen, would remain at the Institute until its closure. Substituting for Krogh from January 1946, Lorentzen achieved his medical specialist degree in pathological anatomy and histology in 1949. In 1950,

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<sup>7</sup> Erik Strömngren to [illegible], 13 November 1946, Direktoratet for Statshospitalerne (1273) - Journalsager 1922-1962 - pakke 889 - journal H5.

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he became director of the Institute, while maintaining teaching positions at Aarhus University in anatomy and pathology.<sup>8</sup>

Lorentzen had worked closely with Einarson, the professor of anatomy at Aarhus University, for some years before HPI was conceived. As discussed further in chapter 4, the two had written several articles together, and Lorentzen's doctoral thesis relied on Einarson's histological staining method. Therefore, when Einarson and Strömngren had the idea to establish an independent institute for neuropathology within the psychiatric hospital, Lorentzen was an obvious choice to head it.

Lorentzen died in 1983, a year after HPI closed down.<sup>9</sup> In effect, he was the Institute. Involved in pathological and histological work from the beginning, he ran the collection for the majority of its active period and for some time the collection even resided in his private residence.<sup>10</sup> To understand the practices in HPI, then, a closer look at Lorentzen's life and work is fitting. It is worth noting that even as the brain sciences and medical collections underwent many changes in the period, the practices established in the late 1940's continued at HPI until the early '80s.

In a 1946 letter,<sup>11</sup> Erik Strömngren, the chief psychiatrist at Risskov, heartily recommended Lorentzen for the position. Here, he described Lorentzen as a tenacious and extremely productive worker. Indeed, he seems to have held at times three to four jobs at once, some of which required no small amount of travel.

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<sup>8</sup> Aarhus University, *Årsberetninger Fra Psykiatrisk Institut Og Statshospitalet Ved Århus (Aarhus University Year Books), 1943-1983*, Aarsberetning, Acta Jutlandica (Aarhus: Universitetsforlaget i Aarhus, 1983); Århus amtskommunes sygehusvæsen, *Administrativ Beretning 1980-1984*, 1986; Erik Strömngren, 'Sindssygehospitalet i Aarhus i 1950. Lægevidenskabelig Og Statistisk Oversigt', in *Årsberetning for Sindssygehospitalet i Danmark 1950*, 1950, 79–87.

<sup>9</sup> Elisabeth Jeppesen, *Den danske lægestand 1982-1993*, 16. udg. / hovedred.: Elisabeth Jeppesen / i red.: Ditte Jørgensen ... et al. (Kbh.: Lægeforeningens Forlag, 1993).

<sup>10</sup> Johannes Nielsen, ed., *125 års jubilæum - Fra jydsk asyl 1852 til psykiatrisk hospital i Århus 1977* (Århus, 1977), 16, 21; Strömngren, 'Sindssygehospitalet i Aarhus i 1950. Lægevidenskabelig Og Statistisk Oversigt', 85. In the villa Riislund, Lorentzen lived with his family on the ground floor and first floor. From 1950 to 1968, the brains themselves were kept in the basement, while the second floor was fitted as a laboratory with workbenches, microscopes, and so on. Lorentzen stayed in the villa when the brains and labs moved to a new building in 1968.

<sup>11</sup> Strömngren to [illegible], 13 November 1946. Unfortunately, the last page of this letter is missing, and no recipient is indicated.

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Besides being involved with HPI from the beginning, first unpaid on a voluntary basis and later as its director, he worked as a lecturer at the Institute of Anatomy at Aarhus University, and a private tutor in anatomy and pathology. He also functioned as a consultant pathologist in hospitals all over Jutland, and performed autopsies of especially intricate cases on requests from clinicians.

### **1.1 Prosector, tutor, and pathologist**

In an interview with his eldest son, Martin, who had often accompanied and sometimes substituted for his father in these latter roles, I heard fond memories of very meaningful work in which the pathological and clinical professions worked closely together and achieved real dialogue.<sup>12</sup> Martin, now a retired pathologist himself, is proud of his skill with the surgical knives and values highly the knowledge gained from *autopsia* – seeing for oneself.<sup>13</sup> As we shall see, the same was true of his father

The work began early in the morning. Lorentzen would take the car to the post-mortem theatre at hospitals in Randers, Kjellerup, Viborg, or Silkeborg. He would arrive very early, as the autopsy had to be finished by 8:00. Martin recalled how the functionary at the morgue would telephone the staff room to let the doctors know that “prosector has arrived”. They would then go to observe the autopsy before beginning their workday at 8:00. The clinicians in these cases had requested the autopsy themselves to learn more about the cause of death, and whether any lines of therapy had been overlooked, which might have helped the patient. Routine autopsies in pathology departments rarely made it back to the patients’ attending physicians. Therefore, these special sessions provided a unique possibility to cross the boundary between the clinic and pathology lab. As Martin put it: “it is important that the pathological report becomes a kind of investigation of the clinical work. Sometimes a clinician has overlooked a disease that the pathologist then finds. They may have misinterpreted the symptoms. This dialogue

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<sup>12</sup> Martin Lorentzen, interview by Thomas Erslev, recording, 21 February 2019.

<sup>13</sup> I employ the concept ‘autopsia’ here to indicate the long tradition of first-hand experience as a cornerstone in medical knowledge production, see Gianna Pomata, ‘Observation Rising: Birth of an Epistemic Genre, Ca. 1500-1650’, in *Histories of Scientific Observation*, ed. Lorraine Daston and Elizabeth Lunbeck (University of Chicago Press, 2011).

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is very important. They have to learn from it, the clinicians”.<sup>14</sup> In these morning sessions, they did.

After the autopsy, the pathologist was invited back to the staff room. The doctors had their morning meeting, where night shift reported any events to day shift. Difficult cases and other matters, perhaps the morning’s autopsy, were discussed before the ward rounds. Meanwhile, a tray of tea, bread rolls, and cake stood before the prosector, who enjoyed his breakfast after a job well done. “We were well received – with style!” recalled Martin with fondness.<sup>15</sup>

These clinicians no doubt valued Lorentzen’s (both son and father) pathological skills. It is clear that being a prosector involved a certain pride in “seeing for one self”, that final judgement of medicine. According to Martin, it is important as a pathologist

to really be in the work. Otherwise you’ll never learn it right. You may have heard about the patient, who you never saw alive. But not before you’ve seen the organs, described how they look, and found out why they look like that, and perhaps performed a supplementary microscopic examination – only then can you write up the concluding report.<sup>16</sup>

He mentioned that colleagues in some of the hospitals where he has worked would let the attendant perform the sections and merely describe from a distance what they could see. “This is not good enough! I did it myself”, he concluded.

Martin shared his father’s preference for hands-on work. One *post-mortem* assistant was a bit lazy and squeamish about opening the cranium and inspecting the brain. Knud Aage and Martin would sometimes discuss this man; that he needed to do his work, and it was necessary for a valuable autopsy to see the brain. They shared a private joke: Waiting until all other parts of the body were sectioned and described, and just when the assistant thought he was done, they raised their voice and issued the order “Mortensen; kraniet op!” – ‘open the skull!’ resulting in

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<sup>14</sup> Martin Lorentzen, interview, 44:20.

<sup>15</sup> Martin Lorentzen, 55:30.

<sup>16</sup> Martin Lorentzen, 43:25.

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the assistant sulking into an “ugh!”<sup>17</sup> The standards of quality were not up for discussion in a Lorentzen autopsy.

These standards, Lorentzen carried with him to his students. Beside his ad hoc autopsies and the work at HPI, Lorentzen was a keen educator. In the first years of directing HPI, he tutored students in anatomy, pathology, and *autopsia* from his private home. In 1946, just months after HPI had begun its work, he took a tour to all Danish state psychiatric hospitals in order to teach psychiatrists and technical staff there first-hand how to properly remove and preserve a brain. Throughout his career, he also lectured at Aarhus University’s Institute of Anatomy, where his students and colleagues knew him as an articulate and eloquent reader, whose lectures ran like clockwork from 8:15 precisely. He co-authored two textbooks, which both came in several editions and were standard curriculum in Danish medical schools throughout the ‘60s and early ‘70s.<sup>18</sup> It is not hyperbole to say that a generation of Danish doctors learned the anatomy of the nervous and circulatory systems from Lorentzen.

His private tutoring sessions give evidence to Lorentzen’s feeling that direct contact between instructor and student was important for learning, as was first-hand experience and *autopsia*. In a letter to the Danish medical periodical *Ugeskrift fra Læger*, Lorentzen expressed concern about the lack of qualified teachers in physiology, biochemistry, and anatomy. “The professor”, he wrote, “and a few assistants obviously cannot teach 500-600 students a year in any other way than by lecturing to large audiences. Personal contact between qualified instructors and students no longer exists”.<sup>19</sup> He therefore advocated for greatly increased numbers of professorships in the basic disciplines taught to first-year medical students.

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<sup>17</sup> Martin Lorentzen, 54:30.

<sup>18</sup> K. A. Lorentzen, Erik Andreasen, and H. H. Jacobsen, *Nervesystemets anatomi*, 1st ed. (Kbh.: Munksgaard, 1961); K. A. Lorentzen, Erik Andreasen, and H. H. Jacobsen, *Nervesystemets anatomi*, 2nd ed. (København: Munksgaard, 1964); K. A. Lorentzen and J. C. L. Holtet, *Hjerte, arterier, vener*, 1st ed. (Skanderborg: self-published, 1957); K. A. Lorentzen and J. C. L. Holtet, *Hjerte, arterier, vener*, 2nd ed. (Aarhus: Akademisk Boghandel, 1961); K. A. Lorentzen and J. C. L. Holtet, *Hjerte, arterier, vener*, 3rd ed. (Århus: Akademisk Boghandel, 1964).

<sup>19</sup> Knud Aage Lorentzen, ‘Lægeuddannelsen’, *Ugeskrift for Læger*, 1962.

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His wish was for well-rounded medical students, who had more than basic generalist knowledge before they began specialisation. They needed to acquire the skill, experience, and trained judgment that were so central in Lorentzen's own practice. To be able to dissect with one's own hands, to analyse properly, to explain symptoms, and to minutely describe pathogenesis were skills not learned from memorising schematics and facts out of context, but from repeated attempts and guided supervision only possible through close contact between instructors and students.

#### 1.2 Standardiser and archivist

Still a small field in Denmark, neuropathology was emerging as a speciality in its own right in post-war Britain. However, the field suffered under its "particular, artisanal techniques"<sup>20</sup> which meant, among other things that "links made between *intra vitam* states and *post mortem* findings were easily dismissed as artefacts of technique, accompanying disease, or death".<sup>21</sup> British neuropathologists did much rhetorical, organisational work to get their discipline recognised as a proper medical speciality. Anna Kathryn Schoefert has shown that they made simultaneous use of the tropes of skill and knowledge. Keen to legitimise their skilful work as highly valuable, they feared its element of routine, which might in time reduce neuropathologists to mere "skilled technicians". Knowledge and research, therefore, had to go hand in hand with skill when fashioning the neuropathologist's identity.<sup>22</sup>

Whether or not Lorentzen was aware of British attempts at legitimising neuropathology as a full-fledged specialty, he put great effort into the standardisation of thorough and skilful work at HPI and among the hospitals supplying tissue to the collection. It was important that all brain examinations be performed in the same way, with notice of the same macroscopic features, and applying the same histological techniques. Martin Lorentzen did part of his

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<sup>20</sup> Anna Kathryn Schoefert, 'Neither Physicians Nor Surgeons: Whither Neuropathological Skill in Post-War England?', *Medical History* 59, no. 03 (July 2015): 407, <https://doi.org/10.1017/mdh.2015.27>.

<sup>21</sup> Schoefert, 407.

<sup>22</sup> Schoefert, 'Neither Physicians Nor Surgeons'.

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medical internship in his father's institute. In our interview, he remembered the diligence his father required from pathological examinations:

When preparing the histological replies to psychiatrists, it was important that we saw to the whole brain by way of a standardised sectioning and routine staining methods suited to whatever we were looking for. It required different staining methods. Originally, we made three different stains per section. That amounted to quite a lot of slices.<sup>23</sup>

When we did microscopic investigations of these brains, and when we sectioned them, we followed a very minute system. We had to be diligent and make sure to examine all the important regions of the brain, the brain stem, and the cerebellum. There was a fixed progression in the treatment of subjects – a standardisation, which meant that we did the same thing every time, and selected standard sections, which were representative for the region under examination. This is also why there is such a wealth of material for later studies.<sup>24</sup>

This standardisation was for the benefit of the psychiatrists receiving their replies, who could then be sure nothing had been overlooked, and might be able to do their own evaluations based on the HPI examination reports. Meanwhile, standardisation and uniformity also shored up documentation and indexation, meaning that any future inquiries as to the holdings of the collection could be answered in a complete, detailed manner, listing all abnormal anatomical structures, lesions, and histopathological alterations without the clinical diagnosis influencing the results.

Thus, standardisation not only worked as an unbiased “examination of the clinical work”, but also as a sound archiving practice with future reference in mind. However, Lorentzen seems to have intended his archive primarily for reproducibility and clinical comparisons in the style of compiling medical case histories,<sup>25</sup> not large-scale statistical studies, or use in other neuro-fields.

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<sup>23</sup> Martin Lorentzen, interview, 27:21.

<sup>24</sup> Martin Lorentzen, 25:10.

<sup>25</sup> cf. Anderson, ‘The Case of the Archive’; Mendelsohn, ‘Empiricism in the Library: Medicine’s Case Histories’.

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Nothing was ever thrown away in pathology. Specimens have always been kept in case a later use should arise. It is not until very recently, when the volume of material have grown so big, that a different practice became necessary. The most important is to keep something for documentary purposes, for example the paraffin-blocks or something cryopreserved. But in my time, everything was kept. In all the pathology departments I've been to, nothing was ever thrown away. The unusual thing in this case was that my father probably did think it could be used by someone else at some later point. It is a great wealth of scientific material, and suddenly it is possible, even in spite of the formalin fixation, by way of modern techniques to use it in some way – to confirm something or use it as a starting point for something else. Then suddenly the collection is world famous and utterly amazing. I don't know which thoughts have passed through my father's head, but I think he was animated by common practice back then: nothing was thrown away. Of course, he only needed to keep the preparations or blocks but, because of the way he did it, the whole brains have been available in cases when other researchers had the interest and need for such material.<sup>26</sup>

The main logic of keeping was the idea that the key to medical knowledge lay in bodily material, and that textual descriptions were only derivatives from this original.<sup>27</sup> Like his son, K.A. Lorentzen was a firm believer in *autopsia*, and collected brains was too good a resource in this regard to simply throw away. That it might be useful later on seems to have been a bonus, and not the primary keeping incentive.

Rather than a bold step towards modern biobanking, HPI was a continuance of 19<sup>th</sup>-century German scientific medicine and the development of psychiatry as a natural science epitomised in Wilhelm Griesinger's dictum that "all mental illnesses are brain diseases", by which brain pathology became an essential prerequisite for psychiatric knowledge.<sup>28</sup> Histopathology had become so prevalent in German psychiatric research in the 1870s and 1880s that for academic

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<sup>26</sup> Martin Lorentzen, interview, 1:36:45.

<sup>27</sup> For a discussion of the ontological implications of this line of thought, see Bronwyn Parry and Cathy Gere, 'The Flesh Made Word: Banking the Body in the Age of Information', *BioSocieties* 1, no. 1 (B 2006): 41–54, <https://doi.org/10.1017/S1745855205050076>.

<sup>28</sup> Eric J. Engstrom, *Clinical Psychiatry in Imperial Germany: A History of Psychiatric Practice*, 1st ed, Cornell Studies in the History of Psychiatry (Ithaca: Cornell University Press, 2003), 89–90.

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psychiatrists in this period the “ideal was not the practicing alienist, but rather the diligent researcher who spent long hours in front of the microscope and at the autopsy table”.<sup>29</sup> Lorentzen no doubt would have applauded this ideal.

Historian of psychiatry Eric J. Engstrom has identified in German scientific psychiatry in the 1870s and '80s the following features: It relied in great part on the microscope, and therefore the mastery of skills in tissue preparation, handling, and instrument use was crucial, especially to root out concerns of subjective observations; It required a steady supply of cadavers for both research and education, as psychiatrists were learning to see the right way by seeing a lot; practical demonstration became the preferred method of instruction, because it ensured the transfer of technical and practical skills, not least dexterity; its goals were in the fields of diagnosis and physiological explanation, more than in therapy.<sup>30</sup>

By the turn of the century, the prominence of neuropathology in psychiatry had waned due to a stagnation in useful results, the time-consuming nature of microscopic work, and pressure from other professional groups within psychiatry and neurology. However, the preceding decades of intensive lab work had secured psychiatry's status as a legitimate field of medicine, and provided psychiatrists with the empirical ethos, and a reputation for rigour and exactitude, which eased the medical community's acceptance of their psychological explanations of madness. Neuropathology, anatomy, and physiology were thus reduced to “auxiliary sciences to psychiatry”.<sup>31</sup> They remained so when Emil Kraepelin founded the *Deutsche Forschungsanstalt für Psychiatrie* in Munich in 1917. Meant to further promote psychiatry as a modern natural science, the Munich institute included research departments for neuropathology, neurophysiology, serology, genetics, and experimental psychology.

Strömngren had visited the Kraepelin-founded *Forschungsanstalt* in 1935 during the preparation of his thesis, *Beiträge zur psychiatrischen Erblehre auf Grund von Untersuchungen an einer Inselbevölkerung*. When assuming his professoriate in

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<sup>29</sup> Engstrom, 89.

<sup>30</sup> Engstrom, 98–126.

<sup>31</sup> Engstrom, 126.

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Risskov, he intended to model this institution on that in Munich, which meant establishing integrated research institutes in a range of fields including brain pathology.<sup>32</sup> HPI was in its conception a continuance of German scientific psychiatry.

The skills and ideals cultivated and praised by Lorentzen throughout his directorship were broadly the same ones that were prominent in 1870s psychiatry-as-neuropathology. However, in the course of the twentieth century psychiatry and brain research moved further away from classical histopathology, as new therapeutics and investigative methods became available. Lorentzen's institute, initially an indispensable part of scientific psychiatry, gradually became obsolete.

In Britain, neuropathologists recognised this trend, and took steps to maintain their position in the medical sciences. Here, J.A.N. Corsellis held a strikingly similar position to Lorentzen, overseeing from 1947 a neuropathological collection at Runwell Hospital, a major psychiatric hospital east of London. Corsellis' mentor, Alfred Meyer, had trained under Walther Spielmeyer, the brain pathologist at the Munich Forschungsanstalt, and was considered "an emissary of the German model of clinical psychiatry".<sup>33</sup> Not content to remain affiliated with this increasingly outdated model, in the 1970's, he expanded the purview of his collection when he began lending out specimens to researchers from neurochemistry, who were finding it hard to come by *post mortem* material from sufferers of Alzheimer's – something Corsellis had in good supply:

Corsellis and other neuropathologists became the gate-keepers to neurochemical studies. [...] Technical and administrative skills, knowledge and access to material permitted Corsellis and his peers to establish neuropathologists as central figures in neurobiological research on human *post mortem* brains to the degree that late twentieth-century neuropathologists asserted that neuropathologists were "natural" brain bankers'.<sup>34</sup>

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<sup>32</sup> Johan Schioldann, Lizzie Sand Strömngren, and Norman Sartorius, 'ERIK ROBERT VOLTER STRÖMGREN 28 November 1909 — 15 March 1993 A Bio-Bibliography', *Acta Psychiatrica Scandinavica* 94, no. 5 (1 November 1996): 283–302, <https://doi.org/10.1111/j.1600-0447.1996.tb09864.x>.

<sup>33</sup> Schoefert, 'Neither Physicians Nor Surgeons', 408.

<sup>34</sup> Schoefert, 419.

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Although these efforts ultimately did not expand the field of neuropathology in Britain, it did promote the discipline and secured its place firmly in the new neuroscientific paradigm that superseded the scientific psychiatry of people like Kraepelin.

Corsellis' strategy depended on connecting a special kind of neuropathological *knowledge* with the well-established skills of the profession. Familiarity with material pathological markers, with the newest investigative techniques, and with years and years of older cases, all combined into knowledge not available to any other neuro-speciality. This made neuropathologists useful consultants to “neurosurgeons, neurologists, coroners, neurobiologists, as well as neurochemists”.<sup>35</sup> They could see which cases would be fruitful in a certain study, how they might be combined with reference to both similarity and difference, which investigative techniques would yield the most useful results, and they had ample experience connecting *post mortem* findings with knowledge of *in vivo* function. The British neuropathologists fashioned themselves as “custodians of the old and new”,<sup>36</sup> because their collections combined experience with insight, with the past providing the foundation for an informed vision of future directions in science. However, “much of Corsellis' daily practice of brain banking, even in the 1970s, would have been familiar to neuropathologists in earlier decades”.<sup>37</sup> The change was in perception and self-fashioning, more than in a change of practice.

Lorentzen did not attempt such a refashioning, and HPI remained almost the same in both practice and image until its closure in 1982. As I will show in chapter 4, research output dwindled from the early '70s, and the decision to shut the Institute relied explicitly on its lack of knowledge production. It seems Lorentzen suffered the fate that British neuropathologists so feared: being reduced to a ‘skilled technician’.

In chapter 5, I will introduce an analytic distinction first made by Robert Kohler<sup>38</sup> between keepers and finders in scientific practice. Put simply, finders are

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<sup>35</sup> Schoefert, 420.

<sup>36</sup> Schoefert, 420.

<sup>37</sup> Schoefert, 420.

<sup>38</sup> Kohler, ‘Finders, Keepers’.

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more concerned with finding new facts, than they are with preserving existing knowledge, whereas keepers focus on securing the means of reproduction of knowledge claims, to make sure not only the facts, but also their experimental, material substrates and raw data are preserved in some way. Lorentzen was concerned with finding facts, as his pathological practice shows, and he believed that the pathologist could often find the truth that a clinician might have overlooked. However, he was primarily devoted to making sure that the knowledge of *how* to find would not be lost in future generations of doctors, and to standardising procedures in his institute, so that future researchers might come back to the collection and find things easily and readily, precisely as an archive.

## 2 FIXING, REMOVING, COLLECTING BRAINS IN DENMARK'S PSYCHIATRIC HOSPITALS

In March 1945, the Institute was a reality and the work of procuring brains from the other Danish hospitals could begin. As most psychiatrists had very little anatomical training beyond the first years of medical school, this task meant not least issuing clear guides for removal, fixation, and transport of pathological brains. Strömngren sent a letter<sup>39</sup> accompanied by a thorough guide<sup>40</sup> to all chief psychiatrists in the country, detailing all the hospitals' new tasks, along with the purpose of HPL.

Removing, fixing, and preparing the brain for transport proved to be intricate work carried out in many stages requiring varying degrees of precision. First, before the brain was removed, hospital staff had to perform a primary fixation, which “must always take place 12 hours after death and *in situ*”.<sup>41</sup> This initial procedure could be done in two ways: through the nose, or via the carotid arteries. In the first instance, one spinal needle was inserted via the nostrils through the

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<sup>39</sup> Erik Strömngren and Jacobsen to Overlægerne, sindssygehospitalerne, 'Følgrebrev Til Vejledning', 26 March 1945, Direktoratet for Statshospitalerne (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

<sup>40</sup> Erik Strömngren and Jacobsen, 'Vejledning ved Behandling og Forsendelse af Hjerner til histologisk Undersøgelse.', 1945, Direktoratet for Statshospitalerne (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

<sup>41</sup> Strömngren and Jacobsen, 1.

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cribriform plate just behind the nasal bone, and another inserted occipitally (in the back of the head). Then 300 cc (0.3 litres) of fixative was injected with the first syringe, allowed to flow through the cranial cavity, and out through the occipital puncture into a bucket or other container in which the secondary fixation could take place. After this, the occipital needle was removed and a final 50-80 cc injected and allowed to remain in the skull until the time of brain removal.<sup>42</sup>

The alternative method for primary fixation consisted in baring the carotid arteries and jugular veins (the blood's entry and exit points to the brain), inserting needles secured with ligatures in all four blood vessels and connecting rubber tubes to these. Then 3-3.5 litres of fixative was run through the blood vessels of the brain, entering through the carotids and leaving through the jugulars. Because it resulted in a uniform fixation this was the recommended method, except in cases where the blood vessels themselves might be of pathological interest. It was, however, also the more complicated procedure, as it required incisions and precise surgical work compared to the relatively simple nasal injection.<sup>43</sup>

After primary fixation, the brain could be removed. The guide made it very clear that "this must be done absolutely as soon as possible, and if for some reason a primary fixation has been prevented, it is of the utmost importance for the histological examination, that [removal] happens immediately after 12 hours post mortem".<sup>44</sup> It then went on to describe the procedure in detail, noting not only how best to remove the brain, but also how to ensure best replacement of the skullcap afterwards, so the corpse could be buried without any noticeable signs of the operation.

Well removed from the cranial cavity, the brain moved on to secondary fixation, which took place in a container of a suitable size. Here, besides making sure to change and dilute the fixative solution correctly, the most important thing was to ensure that the still malleable brain would not be "deformed through contact with the bottom of the container".<sup>45</sup> To this end, the guide suggested

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<sup>42</sup> Strömngren and Jacobsen, 1-2.

<sup>43</sup> Strömngren and Jacobsen, 2.

<sup>44</sup> Strömngren and Jacobsen, 2.

<sup>45</sup> Strömngren and Jacobsen, 3.

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“leading a piece of string below the basilar artery where it crosses the pons, after which the string is fastened to the sides of the container; in this way the brain will float freely in the fixative solution, lightly supported by the piece of string. A similar effect can be gained by letting the brain rest on a piece of gauze hanging down in the liquid”.<sup>46</sup>

Finally, after 2-3 weeks suspended in fixative, the brain could be sent securely from hospitals throughout the country to the Institute in Risskov. Easily sealable preservative jars were not available in a size that would fit all brains. This was lamented in the letter accompanying the guide. With no such easy solution, packing had to be done creatively but securely with the means at hand:

First, the brain must be draped in a sufficiently thick layer of cotton wool or tissue paper, which has been drenched in the formalin solution. Wrap around this a layer of oilcloth, rubber or in lack of these a double layer of parchment. Round this with a generous layer of newspaper or similar, and place the whole package into a sturdy box of suitable size, which is then stuffed with wood pulp or paper, making sure that the brain is surrounded on all sides by a 4-5 cm thick, firm padding.<sup>47</sup>

That this crucial step in the procedure had to rely on such haphazard methods obviously vexed the people at the Institute, and as I show in section 3.1, Strömngren himself spent much energy on trying to procure the right storage vats.

The skills involved in fixation, removal, and safe transport of the brains soon proved too complicated for textual transfer. In May 1946, Strömngren made a request for a 13-day instructional tour by Lorentzen to all Danish state psychiatric hospitals. Experience from the Institute’s first year had made it clear that “it would be highly desirable if the individual hospitals could, by one of the Institute’s doctors, receive personal instruction with regard to the technique for preparation of brains before shipping to the Institute”.<sup>48</sup> Staff both at the individual hospitals, who found the procedure difficult to grasp without ever having seen it performed

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<sup>46</sup> Strömngren and Jacobsen, 3.

<sup>47</sup> Strömngren and Jacobsen, 4.

<sup>48</sup> Erik Strömngren to Direktoratet for Statens Sindssygehospitaller, ‘Instruktionsrejse 22/5/46’, 22 May 1946, Direktoratet for Statshospitalerne (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

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by someone experienced, and at the Institute, who were dissatisfied with the sub-standard quality of the received preparations, shared this wish. Another hope was that proper instruction would lead to more collected brains, because the procedure itself would then not discourage psychiatrists from making use of the Institute's service.



**Figure 7**  
The autopsy table at Oringe Psychiatric Hospital. The wooden crates in the background were used for transportation of excised brains.

Not exactly tacit knowledge – it could be written down after all – exactly *how* to go about histological preservation was best learned by doing it in a supervised setting where an experienced preserver might correct any mistakes. Danish

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psychiatrists would have received only elementary training in pathology while attending medical school, and had no surgical skills or experience. With ever more complex skillsets and increased knowledge requirements in medical sub-disciplines, the ensuing fragmentation of medicine and the dispute between generalists and specialists has long been a subject for historians of medicine.<sup>49</sup>

Fully aware of growing specialisation, K.A. Lorentzen thought it best to visit each hospital himself and demonstrate the practice. His son, Martin, told me:

It was a skill you had to learn. If you just plop the brain in a bucket and pour over the formalin by eye, the tissue would arrive already destroyed, perhaps even partly decomposed. Then it's really hard to find any distinguishable features, so it is important that fixation happens swiftly and sufficiently. This was common knowledge all around neuropathological departments, but the thing is: if you don't cut into the brain but fixate it whole, it takes more time for the fixative to properly sink in to the tissue. This time could be shortened if the brain was partitioned at once in three or four parts to be fixated independently. But it is very nice for a novice with little prior knowledge of pathology, to see the brain in its entirety, evaluate it and perform a thorough description. To this end, they needed to be fixed whole, 'hanged up' with string in buckets.<sup>50</sup>

This quote is interesting not only in recognising the need for personal instruction, but because it addresses why the brains were collected whole rather than in parts. Novices, which is to say the psychiatrists, but also Lorentzen's students, would benefit from personally seeing the entire brain. In this way, the brain collection functioned not only as a consulting pathology department and research institute, but also as a pedagogical resource ensuring a ready source of fresh, pathological brains for interns to practice on, all the while contributing to neuropathological

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<sup>49</sup> Warwick Anderson, 'The Reasoning of the Strongest: The Polemics of Skill and Science in Medical Diagnosis', *Social Studies of Science* 22, no. 4 (1 November 1992): 653–84, <https://doi.org/10.1177/030631292022004005>; Stephen T. Casper and Rick Welsh, 'British Romantic Generalism in the Age of Specialism, 1870–1990', *Social History of Medicine* 29, no. 1 (February 2016): 154, <https://doi.org/10.1093/shm/hkv103>; Christopher Lawrence, 'Incommunicable Knowledge: Science, Technology and the Clinical Art in Britain 1850–1914', *Journal of Contemporary History* 20, no. 4 (October 1985): 503–20, <https://doi.org/10.1177/002200948502000402>; Schoefert, 'Neither Physicians Nor Surgeons'.

<sup>50</sup> Martin Lorentzen, interview, 57:25.

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and psychiatric research. As a bonus, over time the psychiatrists themselves would develop a more trained eye on post mortem brains, something a person like Lorentzen would have thought a great benefit to the science, in keeping with the neuropathological preference for *autopsia*.

### 2.1 Qualms about primary fixation - Irene

The short post mortem interval (PMI – the time from death to fixation) was key to ensuring optimal histological images. From the moment of death, brain tissues begin to deteriorate, along with alterations of molecular structures. These processes accelerate with time, so it is important to fixate the brains quickly if the goal is to see anything relevant to living brains. Therefore, some hospital intendants narrowed the timeline for primary fixation down to minutes after the patient's death, to the peril of junior employees and locum tenants, who often stood alone with the unpleasant task. I have found no evidence that this was official HPI-policy, and find it more likely that the decision was taken by overzealous consultants, as was the case in at least one example.

I interviewed a doctor, now a GP, who held a locum for a registrar at Risskov in the 1970s, before she had finished her medical studies. While there, she was tasked with primary fixation of a few deceased patients. I call her Irene, a pseudonym, to preserve her anonymity. Irene described to me her arrival at the hospital:

I was just a substitute, and I had very little say in how things were done. Anyway, one of the first things the consultant in my department did, was he said, 'now, here's a job for you: When a patient dies on your shift, we need to collect their brain. What you have to do is inject 150-200 ml' (I don't remember the exact amount. It was a large syringe). He took me aside on my first shift and showed me how to do it, plunge a thick, hefty needle through the nose into the brain, and then inject the formalin. What I remember very clearly was that it had to be done *immediately*, within ten minutes of death, he told me.<sup>51</sup>

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<sup>51</sup> Irene, interview by Thomas Erslev, recording, 4 October 2018, 19:18.

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These ten minutes were not reflected in HPI official guide, which put the maximum time from death to primary fixation at 12 hours. The consultant responsible for Irene's department had clearly made his own rules for the primary fixation, probably with the best intentions. However, for the inexperienced locum, not even a fully qualified doctor yet, the instruction was horrifying. The procedure itself was of course unpleasant, but no more so than other standard procedures in the medical profession. Irene's discomfort with the instruction stemmed more from the fact that she, the most junior employee, was given responsibility beyond her experience.

I could not fit it into what I'd learnt in forensic medicine. That was it had to be at least six hours before you were allowed to even refrigerate the body. And I also did not think they could in good decency ask me, a student, to do it. I wasn't even allowed to write the death certificate. **Cause I wasn't a full doctor.** There couldn't be a death certificate, that was only written after six hours.<sup>52</sup>

Irene puts her finger on a mismatch between her responsibilities and qualifications. She was not allowed to write a death certificate – for that you needed the full medical degree. In spite of this, she was tasked with a procedure that in her view should have followed, not preceded, the certification of death. When she continued her story about the time at Risskov, it became evident that it was the strong hierarchy and obvious patriarchy at the department, which frustrated and angered her, and that her outrage was with the way the consultant handled his employees, rather than with the brain collection itself. As she assured me, “Now, in order for the whole thing to make any sense, they had to inject the formalin within the half hour”.<sup>53</sup> She was on board with the collection initiative; she just did not feel professionally equipped for it at the time.

The procedure itself was legal, even before the production of a death certificate. Denmark passed its first tissue excision act in 1967.<sup>54</sup> Before then,

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<sup>52</sup> Irene, 20:25.

<sup>53</sup> Irene, 59:25.

<sup>54</sup> 'Lov nr. 246 af 9. juni 1967 om udtagelse af menneskeligt væv m.v.', Pub. L. No. 246 (1967), <http://www.logir.fo/Lov/246-af-09-06-1967-om-udtagelse-af-menneskeligt-vaev-mv-som-senest-aendret-ved-L>.

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hospitals were self-regulating with regard to best practice. In the 1967 law, no consent was required from patients who died under hospital admittance. The law stipulated that tissues could not be removed earlier than 6 hours after next of kin had been notified of the death, which could in turn only happen after the signing of a death certificate. However, § 9 excepts from the law small procedures with syringes, under which the primary fixation of brains fell. Irene was right, therefore, to be wary of performing the injection before a death certificate had been signed, but the caveat provided by § 9 placed the procedure within the bounds of law.

Not so much about uncaring psychiatrists or rogue scientists, Irene's lingering qualms about the primary fixation procedure tells the story of a highly hierarchical, gender biased medical profession, in which junior doctors were given unpleasant tasks beyond their purview, and where the objections of women in medicine were overheard. Irene's story ended with her going to the consultant's office, where she was made to feel most unwelcome. She had come to ask one last time to be exempted from the primary fixation. The consultant sat quietly behind his desk listening to Irene as she detailed the teachings of her forensic medicine classes, and informed him that she was not qualified for writing death certificates. Minutes after she had finished her discourse, the consultant still sat quietly with no indication that she should expect a reply. She left his office after some time, bewildered without knowing where she stood. The next day, at the morning conference, the consultant informed very briefly that medical students were no longer allowed to perform these procedures. It would be the responsibility of the senior registrar. That was the end of discussion. While Irene's objection had been heard ultimately, she felt trampled and disregarded both for being a woman, and for being a junior employee. She told me that she remembered the hospitals' patients very fondly, but was very happy to leave the psychiatric system.

### **3 INSTRUMENTATION AND ADMINISTRATIVE WORK**

As was evident in the written guide to fixation and transportation, it had not been possible to find a suitable container that would work both as a transport vessel and storage vat for the brains. This problem refers back to an oft-overlooked kind of work, which nevertheless is a big part of any scientific institution: administration

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and procurement of equipment and technology. Establishing an institution such as HPI entails a long to do list of more or less tedious items. In the months leading up to the official inauguration, the Directorate for Danish State Psychiatric Hospitals corresponded with engineers, architects, and manufacturers of all kinds to make sure the necessary infrastructure would be in place. In the following, I will present two examples of such negotiations: the procurement of storage vats, and of equipment for microphotography. The examples I chose among many others,<sup>55</sup> because they give clear evidence to the care afforded these overlooked practices, and because these seemingly tedious administrative choices reveal an underlying scientific conservatism at HPI.

#### 3.1 Storage vats

On 11 January 1945, the chief psychiatrist of the hospital in Risskov, Erik Strömngren, sent a letter to the manager of finances in the Directorate for Danish State Hospitals, Herr Marker. This letter expressed frustration after long unfruitful attempts to secure glass storage vats of the correct type and size for the soon-to-be-established brain collection. Strömngren wrote:

Normally clay pots are used for this purpose, but it has become evident that the pots produced in recent years are somewhat leaky, so that the formaldehyde in which the brains are stored seeps out over time. For the time being, we must therefore store the brains in glass containers. However, no glasses of the wanted type is currently in trade, and we have therefore engaged in negotiations with Kastrup Glasværk and Holmegaards Glasværk regarding manufacture of a special type suited to our purposes. Despite pertinacious negotiations via telephone and in writing, however, we still cannot achieve a result, given that glass manufactures seem to have a special gift for never answering

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<sup>55</sup> Other examples with a paper trail in the archive include: telephonic infrastructure, architects' drawings of the labs, purchase of preserving alcohol and formalin, and engineers' comments on the suitability of storage and working facilities. All can be found in Rigsarkivet under Direktoratet for Statshospitalerne (1273) - Journalsager 1922-1962 - box 889 and box 1195 - file number H5

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what questions you asked, but answering readily to things not asked.<sup>56</sup>

Strömngren turned to Marker, then, to finalise the negotiations and agree on a price with the chosen manufacturer. Preceding this plea, Strömngren had corresponded for over a month with the two glassware manufacturers.

Strömngren had initially written to Holmegaard on 7 December 1944, to enquire if they were able to deliver the vessels. He acknowledged that it might be difficult to produce the glasses, but encouraged the manufacturer to do their very best as it was “absolutely pivotal for the operation of the planned institute that these glasses are acquired”.<sup>57</sup> Holmegaard replied in the negative six days later; while there would be no problem in manufacturing mouth blown glass vessels to specification, it would not be possible to procure the rubber rings necessary for hermetic sealing of the classes in the required size.<sup>58</sup>

Having seen this offer, Strömngren complained that the price seemed to have risen more than sixfold, and that the custom mouth-blown glasses would not “prevent the evaporation of the formalin to be held in the glasses”. Holmegaard then excused that glasses of the required size could not be supplied with mechanically pressed lids (to seal hermetically), simply because the machines used to this end would not work with those sizes.<sup>59</sup>

Meanwhile, Strömngren had also been in contact with Kastrup Glasværk, who were a little more creative in their approach to the request. Like Holmegaard, they could offer cylindrical glasses of the required size (theirs measured 250x250 mm), but not the lids to go with them, “as we consider it impossible to produce a tightly

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<sup>56</sup> Erik Strömngren to Økonomiinspektør Marker, ‘Strömngren-Marker 11/1-45’, 11 January 1945, Direktoratet for Statshospitalerne (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

<sup>57</sup> Erik Strömngren to Holmegaards Glasværk A/S, ‘Strömngren-Holmegaard 7/12-44’, 7 December 1944, Direktoratet for Statshospitalerne (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

<sup>58</sup> Holmegaards Glasværk A/S to Erik Strömngren, ‘Holmegaard-Strömngren 13/12-44’, 13 December 1944, Direktoratet for Statshospitalerne (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

<sup>59</sup> Holmegaards Glasværk A/S to Erik Strömngren, ‘Holmegaard-Strömngren 6/1-45’, 6 January 1945, Direktoratet for Statshospitalerne (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

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sealing lid of this size. We would suggest procuring plates of window glass, which swabbed with lard would make a decent seal against the rim of the cylinder”.<sup>60</sup> The suitability of this approach, they conceded, would depend on the specific purposes intended for the vats.

The hospital, meanwhile, “would absolutely prefer boil-proof preserving jars with lids, clasps, and rubber ring seals”.<sup>61</sup> Therefore, following exchanges with Holmegaard and Kastrup, they contacted the rubber factory Schiønning & Arvé directly to inquire if they could supply the custom rubber seals. They replied in the positive: By contacting Kastrup directly, they would be able to supply both glasses and rings.<sup>62</sup> They seemed, however, to have misunderstood the hospital’s query, as when Kastrup were presented with this statement, they reiterated their earlier response, that they could offer preserving jars with a diameter 150mm, height 235mm, but would not offer bigger sizes, as they “would not be able to guarantee hermetic sealability of such”.<sup>63</sup>

These negotiations were made harder by the distances, which meant that rather than meet face to face and go over any uncertainties and correct obvious misunderstandings, the parties had to rely on letters and telephonic communication, which lacked the means to show diagrams and drawings of the wares in question. Things became easier, therefore, when Herr Marker took over, as both Holmegaard and Kastrup were within easy driving range of Copenhagen.

Marker’s, first action was to go visit the manufacturing house at Kastrup to clear up remaining questions. He left, however, with the same offer as that factory had issued originally: 15 cm diameter preserving jars with rings and lids, or Ø25 cm

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<sup>60</sup> A/S Kastrup Glasværk to Sindssygehospitalet ved Aarhus, ‘Kastrup-SHA 15/12-44’, 15 December 1944, Direktoratet for Statshospitalet (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

<sup>61</sup> Sindssygehospitalet ved Aarhus to A/S Kastrup Glasværk, ‘SHA-Kastrup 21/12-44’, 21 December 1944, Direktoratet for Statshospitalet (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

<sup>62</sup> Sindssygehospitalet ved Aarhus to A/S Kastrup Glasværk.

<sup>63</sup> A/S Kastrup Glasværk to Erik Strömngren, ‘Kastrup-strömngren 23/12-44’, 23 December 1944, Direktoratet for Statshospitalet (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

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cylinder vessels with window-plate tops and lard seal. In an undated letter<sup>64</sup> sent between 11 January and 22, Marker repeated this offer to Strömrgren, and asked a host of questions in order to find the best course of action.

He asked Strömrgren to describe the intended use precisely: Why the need for boil-proof glass, why the need for secure lids? Would transportation from hospitals to HPI take place in the jars, or in another container? Moreover, if the glass vats were meant for transport, Marker added, would Strömrgren be so kind as to mail a sketch of the transportation box, and how it might be possible to securely fit window-plate sealed cylindrical vats within?

He also wanted to know where the ceramic pots used earlier had come from, as “one might be able to speak to a manufacturer, and hear if they might not be able to produce such vessels without the leakage of formalin”.<sup>65</sup> He ended his letter by unequivocally repeating the statement from Kastrup: Glass preserving jars could not be made in the required size.

Still not satisfied, Strömrgren conceded that while preserving jars would be preferable because they could be used for both transport and storage, plate-sealed glasses were a possibility, because (as evident in the guide), other means of transport were possible. “Though”, he continued, “in other ways they are hard to work with, because the seal could never be complete (unless the temperature is absolutely constant)”.<sup>66</sup> The parenthesis boasted a generalist’s broad knowledge of science. The ceramic pots Strömrgren liked had been produced by Swedish Höganäs, but were no longer available. The new pots from Kastrup Teglværk “are usually permeable, I think because the burning temperature is too low”.<sup>67</sup> However, even good ceramics were not as good as preserving jars of glass, because there was no means of sealing them hermetically. “Why,” he enquired one last time for

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<sup>64</sup> Økonomiinspektør Marker to Erik Strömrgren, ‘Marker-Strömrgren n/d’, January 1945, Direktoratet for Statshospitalerne (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

<sup>65</sup> Økonomiinspektør Marker to Strömrgren.

<sup>66</sup> Erik Strömrgren to Økonomiinspektør Marker, ‘Strömrgren-Marker 22/1-45’, 22 January 1945, Direktoratet for Statshospitalerne (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

<sup>67</sup> Strömrgren to Økonomiinspektør Marker.

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emphasis, “is it not possible to produce bigger cylindrical glasses and lids? If not, we will have to make do with the window-plate sealed cylinders from Kastrup + a smaller number of existing preservative jars”.<sup>68</sup> These smaller jars were meant for transportation of smaller-than-usual brains.<sup>69</sup> Finally, Strömngren encouraged Marker to visit a Dr Neel’s psychiatric laboratory at Righospitalet to inspect their storage solution.

Marker did as suggested and reported: “the storage solution [at Neel’s lab] only fortified my conviction” that Kastrups solution was the best possible one.<sup>70</sup> He had therefore – “to bring the matter to an end” – ordered 150 big and 70 small glass vessels, and had received word from the hospital manager at Risskov that the hospital patients could produce the window plates continuously as they were needed.<sup>71</sup>

Strömngren’s irritation had become more and more evident in the course of the correspondence. In the beginning, he questioned the expertise of glass makers, and he continued to disbelieve their assessment with regard to the feasibility of what he wanted. He also repeatedly turned elsewhere (first from Holmegaard to Kastrup, and later to Schiønning & Arvé) for second opinions. His self-image as a man of science seems to have played a part here, especially in his letters to Marker (references to the effect of room temperature and ceramic burning). However, I would argue his persistence is not only due to his ‘persona’, but also to a committed wish to provide the best possible setting for the new Institute for Brain Pathology. Even tedious work such as this, which was usually beneath Strömngren’s station, he wished to handle himself and ensure its proper completion.

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<sup>68</sup> Strömngren to Økonomiinspektør Marker.

<sup>69</sup> Strömngren and Jacobsen to Overlægerne, sindssygehospitalet, ‘Følgebrev Til Vejledning’, 26 March 1945.

<sup>70</sup> Økonomiinspektør Marker to Erik Strömngren, ‘Marker-Strömngren 31/1-45’, 31 January 1945, Direktoratet for Statshospitalet (1273) - Journalsager 1922-1962 - pakke 889 - journal H5, Rigsarkivet.

<sup>71</sup> Økonomiinspektør Marker to Strömngren.

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### 3.2 Microphotography in war time

The Institute had a slow start, and important elements needed for its daily work were not acquired until years after its initiation. It was inaugurated one month before Denmark's liberation from the German occupation, and important materials and apparatus was hard to come by. This lack of necessities also meant the Institute had to delay its replies to the hospital psychiatrists, who had been keen to receive the pathological reports on their deceased patients.

One example of such necessary apparatus was microphotography. In a letter from March '49, Strömngren remarked:

the Institute have in the last years received offers on English and American instruments; however we had not thought to even consider these offers, as the prices seemed rather high, and we were not convinced about the quality of these products. Over the last months, now, we have seen offers regarding apparatus produced in Germany, and with these there can be no doubt they are indeed quality goods".<sup>72</sup>

This he knew because many of the instruments were already in use other places in the country, where they had proven very satisfactory.

The letter accompanies a request for DKK 11.000 for a purchase of the instruments necessary for microphotography, and a new freezing microtome large enough to produce whole-brain slices, for which the one currently in use was too small. Microphotography, Strömngren wrote, was "an absolute integral part of any institute for brain pathology". In the Institute's first years, the staff at AU's normal pathological institute had been kind enough to allow HPI to use their apparatus for microphotography. However, this solution was not tenable because it was very difficult and bothersome to transport materials from Risskov to the anatomical institute, and because these instruments were needed in the daily work of that unit. This meant that almost no photographs had been taken. The preparations were still at HPI, so with time the backlog of photographs could be sent to the

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<sup>72</sup> Erik Strömngren to Direktoratet for Statens Sindssygehospitaller, 'Mikrofotografi', 3 August 1949, Direktoratet for Statshospitallerne (1273) - Journalsager 1922-1962 - pakke 1195 - journal H5, Rigsarkivet.

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hospitals. However, it would be “of great significance” if in the future the “very instructive microphotographs” could accompany the pathological reports, instead of following years after the initial response.<sup>73</sup> The ministry of finance approved the application 11 April, and granted DKK 11.000 towards the purchase.<sup>74</sup>

A certain traditionalism can be seen in Strömngren’s estimation of the quality of manufacturers. In the case of storage vessels, his preferred suppliers (Holmegaard and Höganäs) were bourgeois household-name brands, and remain so to this day. Add to this his preference for the German instrument makers he had used before the war over American and British newcomers; an image emerges of a leader who trusted his own experience over novelty, and who believed himself an authority even outside his academic specialty. As a professor and chief psychiatrist, Strömngren’s daily work took place far from the storage rooms and labs of HPI, but when negotiating with outside partners, he considered himself an expert. Strömngren’s traditionalism also underlines the brain collection’s awkward position between the past and the future. In chapter 2, we saw Einarson imagine his technique as the culmination of a long tradition, but also capable of crossing the threshold to a future kind of brain science, which would be able to see and measure things no one would have thought possible before. The brain collection, likewise, was both a continuance of pre-war German scientific psychiatry, and a new kind of scientific resource, which given Einarson’s method, would only gain value as technique progressed. The traditionalism present in its instrumentation on the one hand points backwards. However, it also signifies a wish to establish a lasting collection. Strömngren knew that certain instruments and materials had already stood the test of time; if this collection was to last, better to equip with longevity and tradition in mind, than to throw oneself onto the latest novelty. Traditionalism in storage and preservation, but progressiveness in histological technique and theoretical imagination: this was the dual tense that birthed the

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<sup>73</sup> Strömngren to Direktoratet for Statens Sindssygehospitaler.

<sup>74</sup> P.M.V., Finansministeriet to Indenrigsministeriet, ‘Mikrofotografi godkendelse’, 11 April 1949, Direktoratet for Statshospitalerne (1273) - Journalsager 1922-1962 - pakke 1195 - journal H5, Rigsarkivet.

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brain collection. It incarnated, at the same time, “what we’ve always done”, and the promising new hopes for future of brain science.

#### 4 CONCLUDING REMARKS

In this chapter I have shown the quotidian side of HPI’s brain collection: routines were established, which lasted for decades; tedious complications arose, which needed administrative solutions; hundreds of medical students passed through Lorentzen’s lab and learned pathology first hand.

Even though the brain collection was active for 37 years in a period of rapid change and development in the brain sciences, the practices established in its first years remained stable until the end, and makes a collection that could be seen as ahead of its time seem more like a curious remainder of obsolete science. However, the fact that the material is still scientifically useful is worth remembering before deriding formaldehyde-preservation as an outdated technique. “It is still a fine method of preservation”,<sup>75</sup> and even today allows many kinds of examination. The preservation method also meant broadening basic pathological skills to “novices” at Denmark’s psychiatric hospitals, who might otherwise never have seen the physical brains of their patients.

Already at the initial stage of collection, a choice had to be made between two methods of fixation – depending on the nosology of the patient, different kinds of preservation was needed for different kinds of study. Specifically, whether the blood vessels would be of interest in the autopsy, or not. The psychiatrists needed more than basic knowledge of pathology in order to evaluate which kinds of symptoms their patients might show, based on the clinical picture. This stands in contrast to cryogenics in which one preservation method suited a whole range of investigations.<sup>76</sup> When freezing, the clinician needed minimal knowledge of the pathologist’s art and only brief contact with the physical brain, but in the case of formaldehyde-preservation, clinicians had to go through an elaborate process and observe the brain go through stages from almost liquidly malleable to fixed,

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<sup>75</sup> Martin Lorentzen, interview.

<sup>76</sup> Joanna Radin, *Life on Ice: A History of New Uses for Cold Blood* (Chicago: The University of Chicago Press, 2017), 7–8.

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hardened to the point that it could be cut in slices. Whether the fixation procedure and autopsy reports led any psychiatrists to appreciate pathology more than before, I do not know. But I have argued that the wish to cross specialty boundaries was one reason for the existence of HPI.

K.A. Lorentzen must be understood as a pioneer in one respect, for having developed to such a large extent the discipline of neuropathology in psychiatry. However, in contrast to the extremely future-oriented novum of cryopreservation described by Radin,<sup>77</sup> collection practice in the brain collection was not a case of “this will change everything”. Rather, the brains were stored and kept because “it’s what we’ve always done.” The primary goal was to secure reproducibility and safeguard any clinically relevant knowledge.

The Institute for Brain Pathology was not the sole object of Lorentzen’s work effort. He put much energy into education, and in his secondary job as a prosector upon request. As I show in chapter 3, Lorentzen did not publish much research, so perhaps brain pathology in itself was not his first passion, but it was certainly a useful tool in furthering appreciation of his art.

I have also shown that beside the lab work, the infrastructure supporting transport, storage, and documentation of the brain, required both thought and time. While the two cases described here are from the Institute’s first years, new needs arose continually, circumstances changed, and administration was a daily task, as it is in scientific work generally.

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<sup>77</sup> Radin, *Life on Ice*.

## *Chapter 4*

### The active collection: Research and curation

In this chapter, I survey the scientific output from the brain collection. From its conception in the 1940s until the late 2010s, the brain collection has been used in diverse research programs. Thinking of the collection as an archive, as I began to do in chapter 1, reveals that the collection's scientific history is not a continuous one, but one full of junctures, jumps, and hiatuses. What was interesting in the 1940's seems today like strange curiosities; the methods used in recent studies would have been utterly foreign to mid-century researchers. In its history of seven decades, the brain collection has been praised, abandoned, rediscovered, valued and revaluated repeatedly, depending on the scientific interests of the actors that have engaged with it.

The collection is one of very few remaining “premodern” brain banks in the world. I know of one other collection with an almost similar profile: the Corsellis collection from London, now housed in Belgium. Comprised of about 8.600 brains at its largest, and established in the early 1950's under the supervision of Swedish psychiatrist Rolf Ström-Olsen, the likeness to the Danish bank is striking.<sup>1</sup> What makes these banks “premodern” in the scientific jargon is primarily their preservation methods. Most contemporary brain banks use cryopreservation rather than formaldehyde, a technique much better suited to modern genetic research methods. Given the age of the Danish collection, it holds up to three

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<sup>1</sup> Anna Kathryn Schoefert, ‘Research on the Human Brain Post Mortem in Britain, c. 1950-1980: Constituting the Corsellis Collection’ (MPhil thesis, Cambridge University, 2011).

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generations of sufferers from some families, making the prospect of genetic analysis highly interesting for studies of inheritance in mental illness. Therefore, while not optimal for the lab methods of genetic studies, premodern collections are valuable in different ways. Another feature of the premodern banks is that they hold specimens from patients untreated with psychopharmaceutic methods. These specimens represent a valuable cohort for control studies, since it would be both unethical and impractical to let current sufferers go untreated for control purposes.

In section 1, I continue where chapter 2 left off, so to speak. As shown in chapter 2, Einarson chose his close collaborators Erik Krogh and K. A. Lorentzen to staff the Institute of Brain Pathology. In the first decades after its establishment, the Institute's research programme followed Einarson's cue, and section 1 describes Krogh's and Lorentzen's methods and findings. Overall, these first decades of research at HPI describe a trend toward cataloguing histological changes in the brain, and their causes. It seems the aim was a compendium of effects and causes, which would make brain pathology an even sounder discipline.

Section 2 presents the research that has used material from the brain collection in the interval since the closure of HPI in 1982, and the collection's reinstitution as part of the department of psychiatry at Aarhus University in 2006. This includes research on familial Danish dementia (FDD), a vicious single-pedigree disease first observed in 1970, but only named and researched thoroughly in the 2000's. It also includes studies on genetic diseases, in which the brain collection is useful for cross-generational studies, as well as investigations requiring brains from subjects that did not receive psychopharmacological medication.

In section 3, I describe the scientific activities in the collection since 2006, when it was re-instituted with Karl-Anton Dorph-Petersen as its manager. In order to do this, I first provide some analysis of Dorph-Petersen's background in Pittsburgh, as this context is highly relevant for understanding his ambitions with the Danish collection. After this, by way of example, I show how the collection, now fully understood as an archive in actors' terms, became reconstituted to suit new ends, and how in this process both the stuff in the collection was changed,

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and the ways in which it was handled changed with it. I do this by reviewing an article that evaluates the feasibility of DNA methylation analysis of the tissues in the Danish collection. A close reading of this article reveals clearly Rheinberger's insight that epistemic thing and experimental system are co-constitutive, and couples this insight with the concept of the archive. Finally, I continue the ambition from chapter 3 of including quotidian and technical work in our understanding of scientific work. While research output has been limited since 2006, the brain collection has not been idle. Many hours have gone in to the less prestigious, but archivally crucial work of gathering and sorting data, of taking care of the physical specimens, and of making sure the right specimens and experiments are put together in order to ensure maximal output from costly studies.

I THE FIRST DECADES OF HPI: EINARSON'S IMMEDIATE  
LEGACY

As previously noted, the first scientific employees at HPI were Lárus Einarson himself (scientific consultant) and Erik Krogh (assistant researcher). Krogh, however, soon had to recede the post. His replacement, Knud Aage Lorentzen, was Einarson's protégé and had already worked with him for some years. Both Krogh and Lorentzen co-authored articles with Einarson, and they used his method in their own studies. In the following, I present a brief overview of their work at HPI, and how it relates to Einarson's.

Erik Krogh studied the effect of anoxia in cells in the spinal cord in rabbits, cats, and dogs.<sup>2</sup> Anoxia means lack of oxygen, and Krogh achieved this by clamping the aorta, thus restricting blood supply to the brain and spinal cord. As blood carries oxygen, the affected cells will be oxygen depleted after some time. Many others had done this before Krogh. However, their results had been inconclusive. Krogh attributed this primarily to their use of Nissl's staining technique:

A lot of contradictory views are held and many apparently incompatible experiments have been made. The results obtained before the application of the methylene blue stain of Nissl in all

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<sup>2</sup> Erik Krogh, *Effect of Acute Anoxia on the Large Motor Cells in the Spinal Cord*, Acta Jutlandica Supplementum, XVII (København: Universitetsforlaget i Aarhus, Ejnar Munksgaard, 1945).

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probability have to be disregarded as the staining methods formerly used are too uncertain. Even the results obtained with the Nissl stain are not and cannot be conclusive, because this method is often rather capricious and because it rests on a personal judgment how far one will proceed with the differentiation.<sup>3</sup>

Krogh used Einarson's gallocyanin chrome alum stain, which allowed for much greater certainty. He also referred to Einarson's activity stages, as he believed previous uncertainties might have been caused by inducing anoxia in cells in different stages of activity, thus rendering different histological images from the same procedure. By remedying these methodological and theoretical deficiencies, Krogh hoped to improve the knowledge of the effects of anoxia at cell level.

Later, Krogh and Einarson worked together, combining the results of animal studies and post mortem studies of human brain slices, and of different experimentally produced reactions as well as "natural" pathologies. Like in Einarson's own work, the aim of these studies was a better understanding of "nucleic acid metabolism" in the nerve cells. It had become clear that the two nucleic acids (RNA and DNA) were found in different parts of the cell, and likely played different physiological roles. Figuring out in what circumstances these acids remained static in the cell nucleus, and when they entered the Nissl substance, they hoped would further cell-physiological knowledge. In a paper delivered to the Society for Experimental Biology at a Bristol Conference in 1954, Krogh presented a summary of their results so far.<sup>4</sup> When in the 1940's they had considered Einarson's method promising for its determination of physiological activity post mortem, now it was used to determine a quantitative measure of the nucleic acids in different cells under different circumstances, and could also supply an image of the position of the nucleic acids within the cell at different activity stages. Krogh and Einarson concluded, "The nucleic acid of the cytoplasm [the RNA] is formed primarily round the nucleolus inside the nucleus, and then diffuses through the nuclear membrane to become a constituent of the basophilic elements [Nissl

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<sup>3</sup> Krogh, 10.

<sup>4</sup> Krogh and Einarson, 'Nucleic Acid Metabolism in Nerve Cells under Different Forms of Activity and Hyperactivity, Shown by the Gallocyanin-Chromalum Method'.

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bodies] in the cytoplasm”.<sup>5</sup> This knowledge, they hoped, might help construct future experiments, to come closer to a functional understanding of the relationship between RNA and DNA.

As mentioned, Krogh quickly receded to the background of both managerial and scientific work at HPI. His successor, K.A. Lorentzen, had written his doctoral dissertation under Einarson. In its preface, Lorentzen thanked Einarson for supplying “splendid working conditions in the Department of Anatomy”, where he had carried out experiments from 1943-47, and for being “an inspiring teacher to me in the fields of neuroanatomy, neurohistology, and neuropathology”.<sup>6</sup> He also thanked Erik Strömngren “for his great contribution in building up the Institute of Brain Pathology giving me in that way very favourable working conditions”.<sup>7</sup> Strömngren’s administrative contributions to HPI were presented in chapter 3.

The subject of Lorentzen’s thesis was the central nervous system during insulin shock, and specifically changes in Einarson’s structural activity stages from insulin shock. Insulin shock, caused by overdose in a controlled environment, had been introduced as a treatment for schizophrenia in the 1930’s (1937 in Denmark). In his thesis, Lorentzen investigated not the clinical results of this treatment, but the “histological changes of the central nervous system in rabbits treated with insulin shock”.<sup>8</sup> He reported that post mortem studies were underway at HPI on human brains from insulin shock treated patients, to be published later. These human studies were never published. In his animal studies, Lorentzen saw the same histologic changes that Krogh had found in cases of anoxia. He could thus conclude that insulin shock severely reduced the uptake of oxygen in the cells. From the animals that did not die in shock, he could see that:

Some nerve cells, particularly in the fifth layer of the cerebral cortex, become extremely chromophobic during the shock

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<sup>5</sup> Krogh and Einarson, 79.

<sup>6</sup> Knud Aage Lorentzen, *The Central Nervous System during Insulin Shock with Special Reference to Structural Activity Changes of the Nerve Cells. An Experimental Histological Study*, Acta Psychiatrica et Neurologica Scandinavica, Supplementum 64 (Copenhagen: Ejnar Munksgaard, 1950), 5.

<sup>7</sup> Lorentzen, 5.

<sup>8</sup> Lorentzen, 10.

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proper and pass into irreparable cellular atrophy (i.e. develop into cell shadows).

After the cessation of treatment the resynthesized nucleoprotein (the chromatin) will again accumulate in the cells, and in most areas the chromophoby will be replaced by the normal activity picture.<sup>9</sup>

In other words, some cell loss was inevitable with insulin shock-treatment. However, given that treatment did not continue for too long, most of the damage was reversible.

These results were interesting not only as basic science, but also in the clinic and for post mortem analysis of human brains. Villars Lunn, respected professor of psychiatry at the University of Copenhagen and consultant at the National Hospital of Denmark, cited Lorentzen's findings at length in his 1952 review "New Experiences regarding the Somatic Treatment of Endogenous Psychoses".<sup>10</sup> He called Lorentzen's work "a most important contribution" to the knowledge of insulin shock-treatment.

In the morgue, if a post mortem investigator found a histologic image of anoxia in a shock-treated patient before Lorentzen's study, this anomaly might be ascribed to either the schizophrenia itself, or to a secondary pathology. However, Lorentzen's findings clearly linked such a picture to the insulin-induced anoxia, in other words to the treatment not the disease, thereby strengthening diagnostic certainty.

Generally, the studies carried out in the first decade of HPI's existence took Einarson's basic approach and applied it to diverse pathological and experimental changes. While the individual investigations carried some importance, the real aim seems to have been to establish a compendium of histologic changes in pathological brains, which could then elucidate normal function of the cells. This is an old method in medicine: to determine the normal by way of the pathological.

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<sup>9</sup> Lorentzen, 73.

<sup>10</sup> Villars Lunn, 'Nye Erfaringer Ved Den Somatiske Behandling Af Endogene Psykoser', *Nordisk Psykiatrisk Medlemsblad* 6, no. sup1 (January 1952): 117-34, <https://doi.org/10.3109/08039485209130687>.

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The team at HPI hoped to find out the role of the basophilic substances in the normally functioning cell, by seeing how they behaved in abnormal cells.

## 2 RESEARCH IN THE INTERVAL

Over the course of the 1950s and '60s, the research program initiated by Lárus Einarson gradually came to look obsolete. As noted in chapter 2, these two decades saw the rise of the 'synaptic brain', in which connections between cells were more interesting than strictly cytological knowledge of the cells' structure and alterations. Cytology was no longer epitomal. Additionally, the rise in knowledge about the nucleic acids made Einarson's focus on the 'Nissl substance' less meaningful, as RNA and DNA came to be understood as distinct entities not only in chemical structure, but also with regard to cyto-physiological function. Together, these new epistemic things – neurotransmitters and DNA – came with experimental systems that involved living brains or in vitro cell cultures. Einarson's history of truth had been superseded, and his research material had lost its value.

When Einarson's function-from-structure programme ebbed out, so did the scientific output from HPI. From the late 1960s until the Institute's closure in 1982, only five texts were published that used tissue from the collection. Of these, one was a compiled autopsy report from four patients with Klinefelter's syndrome,<sup>11</sup> two were reports on the prevalence and clinical picture of intracranial tumors in the HPI material.<sup>12</sup> Finally, two articles reported the discovery of a new syndrome, later to be dubbed Familial Danish Dementia (FDD).<sup>13</sup>

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<sup>11</sup> Johannes Nielsen and Ove Perbøll, 'Autopsy of Patients with Klinefelter's Syndrome', *Danish Medical Bulletin* 16, no. 1 (1969): 10–15.

<sup>12</sup> Poul Gertz Andersson, 'Intracranial Tumors in a Psychiatric Autopsy Material', *Acta Psychiatrica Scandinavica* 46, no. 3 (1970): 213–24; Poul Gertz Andersson, 'Intracranielle Tumorer Hos Midalrende Psykiatriske Patienter', *Nordisk Psykiatrisk Tidsskrift* 25, no. 3 (1971): 232–36.

<sup>13</sup> Erik Strömngren, 'Heredopathia Ophthalmo-Oto-Encephalica', in *Handbook of Clinical Neurology*, ed. P. J. Vinken and Bruyn, vol. 42 (Amsterdam: North-Holland Publishing Company, 1981), 150–52; Erik Strömngren et al., 'Cataract, Deafness, Cerebellar Ataxia, Psychosis and Dementia - a New Syndrome', *Acta Neurologica Scandinavica* 46, no. Supplementum 43 (1970): 261–62.

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#### 2.1 Familial Danish dementia

Familial Danish Dementia is a progressive disease, beginning with the development of cataracts, followed by deafness, cerebellar ataxia (loss of muscle control initially mimicking symptoms of drunkenness like slurred speech and uncoordinated movement, but gradually tending toward total loss of limb control), psychosis, and finally dementia. The syndrome had in 1981 been observed only in 9 members of the same family, dispersed over 5 generations.<sup>14</sup> Strömngren concluded rather grimly in his 1981-text: “Treatment has been unsuccessful. [...] During their last years these patients are confined to bed and completely dependent on intensive care”.<sup>15</sup>

In 2000, 18 years after the closure of HPI, and 30 years after the first report of the syndrome, a likely cause was discovered for FDD. A team of researchers showed that an alteration in a specific gene was the likely root cause of all the symptoms and clinical changes in sufferers of FDD. The gene was also involved in the disease known as familial British dementia (FBD), although a different mutation was responsible for this disease.<sup>16</sup> While these familial diseases are very rare, the neuropathological lesions evident from histological analysis of post mortem tissue is very similar to those in the much more common Alzheimer’s disease (AD). The confined genetic history of familial dementias, however, made it possible to identify the root cause in a single genetic mutation, and on the same gene in both cases. Therefore, FDD and FBD became interesting as proxy-studies of AD. Researchers are able to model FDD and FBD in mice and control variables closely, in order to follow the specific developments of lesions and symptoms. These models can then be used to test hypotheses regarding the pathogenesis of AD.<sup>17</sup> This work is still ongoing. Most recently, a study published in *Nature* “further

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<sup>14</sup> Strömngren, ‘Heredopathia Opthalmo-Oto-Encephalica’.

<sup>15</sup> Strömngren, 152.

<sup>16</sup> Ruben Vidal et al., ‘A Decamer Duplication in the 3’ Region of the BRI Gene Originates an Amyloid Peptide That Is Associated with Dementia in a Danish Kindred’, *Proceedings of the National Academy of Sciences* 97, no. 9 (25 April 2000): 4920–25, <https://doi.org/10.1073/pnas.080076097>.

<sup>17</sup> Janaky Coomaraswamy et al., ‘Modeling Familial Danish Dementia in Mice Supports the Concept of the Amyloid Hypothesis of Alzheimer’s Disease’, *Proceedings of the National Academy of Sciences* 107, no. 17 (27 April 2010): 7969–74, <https://doi.org/10.1073/pnas.1001056107>.

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support[ed] the hypothesis, put forward several years ago, that familial Danish, British and Alzheimer's dementias share a pathogenic sameness and that *Itm2b* [the "familial dementia gene"] should be recognized as a fourth Familial Alzheimer disease gene".<sup>18</sup> What began as a local rarity from the Danish brain collection now seems to hold relevance for a broader range of pathologies, and might help further therapeutic measures.

### 2.2 Generational studies

Besides FDD, other familial pathologies are present in the brain collection. In 1987, Susanne Gydesen et. al. described a family with three generations of sufferers of a kind of dementia different from Alzheimer's and Pick's disease, the most common forms of dementia.<sup>19</sup> The disease was later identified as a frontotemporal dementia (FTD) and mapped to chromosome 3, giving it the name FTD-3.<sup>20</sup> Gydesen et. al.<sup>21</sup> had found 14 sufferers among 73 members of the same family. They collected clinical data from all the affected, neuropathological reports from the deceased, and performed studies on the living sufferers as well as ten well-functioning first degree relatives. The histological slides used in the study had been prepared at HPI by K. A. Lorentzen and Ove Perbøl.

Sufferers of FTD-3 have a 50% risk of transferring the disease to their children. In 2005, researchers identified the gene responsible for the disease.<sup>22</sup> This afforded

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<sup>18</sup> Wen Yao et al., 'The Familial Dementia Gene ITM2b/BRI2 Facilitates Glutamate Transmission via Both Presynaptic and Postsynaptic Mechanisms', *Scientific Reports* 9, no. 1 (19 March 2019): 4862, <https://doi.org/10.1038/s41598-019-41340-9>; cf. Robert Tamayev et al., 'Danish Dementia Mice Suggest That Loss of Function and Not the Amyloid Cascade Causes Synaptic Plasticity and Memory Deficits', *Proceedings of the National Academy of Sciences* 107, no. 48 (30 November 2010): 20822–27, <https://doi.org/10.1073/pnas.1011689107>.

<sup>19</sup> S Gydesen et al., 'Neuropsychiatric Studies in a Family with Presenile Dementia Different from Alzheimer and Pick Disease', *Acta Psychiatrica Scandinavica* 76 (1987): 276–84.

<sup>20</sup> Jerry Brown et al., 'Familial Non-Specific Dementia Maps to Chromosome 3', *Human Molecular Genetics* 4, no. 9 (1 September 1995): 1625–28, <https://doi.org/10.1093/hmg/4.9.1625>; S. Gydesen et al., 'Chromosome 3 Linked Frontotemporal Dementia (FTD-3)', *Neurology* 59, no. 10 (November 2002): 1585–94, <https://doi.org/10.1212/01.WNL.0000034763.54161.1F>.

<sup>21</sup> Gydesen et al., 'Neuropsychiatric Studies in a Family with Presenile Dementia Different from Alzheimer and Pick Disease'.

<sup>22</sup> Gaia Skibinski et al., 'Mutations in the Endosomal ESCRTIII-Complex Subunit CHMP2B in Frontotemporal Dementia', *Nature Genetics* 37, no. 8 (August 2005): 806, <https://doi.org/10.1038/ng1609>.

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the family members the opportunity to get a test determining whether they would develop the disease later in life. Such knowledge is useful for both the patient and next of kin, for preparing for the onset of disease. It is also a nightmarish dilemma for members of the family, who may have to face the disease decades before they would develop the first symptoms, and even live with the knowledge that if they have the gene, their children are automatically also at risk.<sup>23</sup> However, identification of the responsible gene also allows for targeted therapy, such as transferring healthy genes into pre-diagnosed patients. We may hope that the last at-risk generation of the FTD-3 family has already been born.

A similar case, albeit involving a very different pathology, appeared in an article from 2001.<sup>24</sup> Here, researchers presented a three-generation family suffering from familial Creutzfeldt-Jakob disease (CJD). CJD is a so-called prion disease like mad cow disease and kuru.<sup>25</sup> These diseases are characterised by an abnormal development of a particular protein in the brain, that can cause all manner of degeneration in the brain, including dementia, difficulty walking, confusion, muscle stiffness, and more. At the time, researchers were suspecting one particular mutation of causing familial CJD, but this was the first time said mutation could be shown in three generations of the same family of sufferers, thus providing an important indication of the veracity of this theory.

In Holm et al.<sup>26</sup> the brain of subject II-1 (the first sibling from the second generation) was present in the HPI-collection. This brain provided the material evidence needed to link the three generations, and be sure that they all had suffered from the same disease. Though the brain itself had deteriorated beyond DNA-analysis, and no original diagnosis was given, revision of the microscopic sections proved that II-1 had indeed suffered from CJD of the same kind as I-1 and

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<sup>23</sup> Jon Fiala Bjerre, 'Det arvelige familiedilemma', *Vores liv med demens* (blog), 13 March 2015, <http://www.alzheimer.dk/temaer-om-demens/vores-liv-med-demens/det-arvelige-familiedilemma/>.

<sup>24</sup> I. E. Holm et al., 'Creutzfeldt-Jakob Disease Segregating in a Three Generation Danish Family', *Acta Neurologica Scandinavica* 103 (2001): 139–47, <https://doi.org/10.1034/j.1600-0404.2001.103003139.x>.

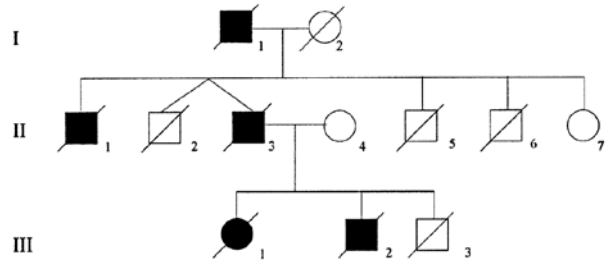
<sup>25</sup> For a history of kuru research, see Anderson, *The Collectors of Lost Souls*.

<sup>26</sup> Holm et al., 'Creutzfeldt-Jakob Disease Segregating in a Three Generation Danish Family'.

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III-2. Further, thanks to the extensive patients records preserved in the HPI-collection, the familial connection could be easily established.

While both these studies could have been conducted without the brains from HPI, the fact that the collection held histological slides from earlier generations made it easier to identify the familial aspect of the diseases, and to follow their generational prevalence. The opportunism of the open-ended archive in



**Figure 8**

Illustration from Holm et al. (2001, p. 140): “Three generation pedigree of a family in which CJD segregates as an autosomal dominant disease”. Filled figures indicate sufferers of CJD, the empty are non-sufferers.

this case meant that brains originally collected for a different purpose, namely the study of function through structure, could be used in a specific pathological investigation of a disease the brain had not previously been diagnosed with. This is a major difference from most modern biobanks, which collect tissue for very specific reasons, and employ rigorous screening processes to avoid wasting time, money, and effort on specimens that will not help address whatever question is their purpose.<sup>27</sup>

It is also worth noting here that both the FTD- and CJD-studies were possible because living people approached the scientists before the study began, thus allowing researchers access to their deceased relatives’ brains. Today, living kin to subjects in biobanks are protected by the so-called ‘right not to know’. This right grants them protection from potentially traumatic knowledge that they would rather have been without. For example, had the researchers identified a living relative of a deceased FTD-3 sufferer, and approached this person with the news that she or he had a 50% risk of developing the disease this might cause undue shock and existential crisis in the approached subject. Just as informed consent is required before any medical or surgical intervention, so a would-be patient has to

<sup>27</sup> More on this in chapter 5

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be aware of such a possibility before any knowledge can be disclosed. Practically, this means a researcher has to be approached and asked directly, before beginning the study of such subjects. This issue, among other bioethical problems related to the brain collection, is discussed in chapters 5 and 6.

#### 2.3 Pre-psycho-pharma controls

In 1990, the Danish neurologist Bente Pakkenberg had observed a 40% reduction in the number of cells, and 25% reduction in volume, in a particular brain region in sufferers of schizophrenia.<sup>28</sup> However, she remained unsure as to the cause of this reduction. All the studied subjects had been treated with psychoactive medicine for an average of 17 years; might this, she wondered, and not the disease be the cause? Consequently, she ran a second study, in which she compared her earlier results with measurements from brains of untreated schizophrenics. Since neuroleptics did not exist before 1950, the brains collected at HPI between 1945 and 1949 were from patients who had received no such drugs. Pakkenberg selected 8 brains from the HPI collection to compare to eight normal Danish brains, and 12 brains from neuroleptic-treated schizophrenics. Because the HPI-brains had been submerged in formalin for a mean period of 39 years, slowly eroding the cell structure in the brains, she was not able to do an actual cell count.<sup>29</sup> She could however compare volumes and extrapolate. She used new methods from stereology to measure the volume of the mediodorsal thalamic nucleus (MD), which is a brain region involved in schizophrenia. Stereology is a field of sampling methods based upon stochastic geometry used to obtain unbiased estimates of structural parameters (such as numbers, volumes, areas, etc.) for three-dimensional objects from two-dimensional samples of those objects. With this method, Pakkenberg was able to sample 18-20 samples of a thickness of about 4

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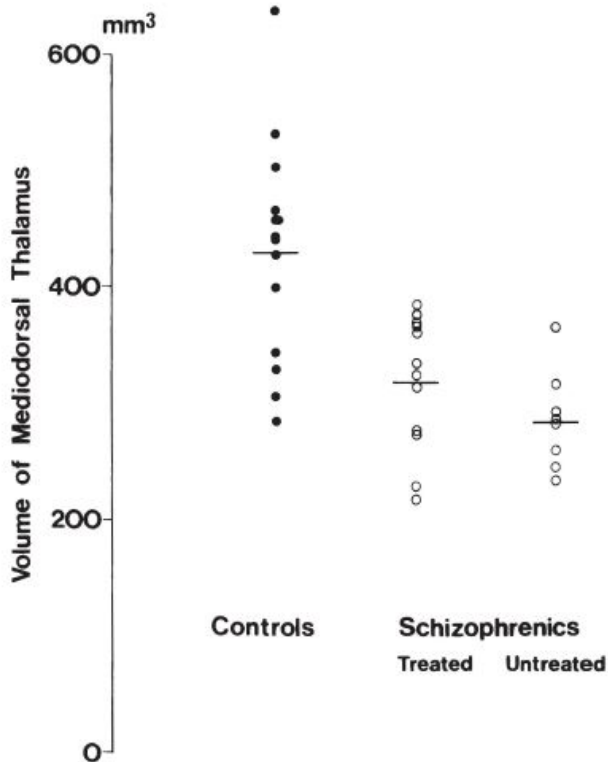
<sup>28</sup> Bente Pakkenberg, 'Pronounced Reduction of Total Neuron Number in Mediodorsal Thalamic Nucleus and Nucleus Accumbens in Schizophrenics', *Archives of General Psychiatry* 47, no. 11 (1 November 1990): 1023-28, <https://doi.org/10.1001/archpsyc.1990.01810230039007>.

<sup>29</sup> This can, however, be done with modern methods for cell counting, which use thicker sections. Thicker sections make it possible to count cells even in long-term preserved tissue.

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micrometres (0.004 mm) from each of the brains, and use these samples to estimate very precisely the total volume of the region.<sup>30</sup>

She found that the untreated patients' brains were 31% smaller than the healthy controls, and the brains from treated patients were 22% smaller. Even taking into account the likely shrinkage from the extended fixation times of the HPI-brains, she concluded that it was most likely that "the smaller total neuron number in MD of schizophrenics (40% less than controls) is unrelated to drug treatment".<sup>31</sup> In this instance, the outcome was the lucky one that neuroleptic



**Figure 9**  
 Illustration from Pakkenberg (1991, p. 97). Shows the volume of the MD in 14 controls, 8 untreated schizophrenics, and 12 neuroleptic-treated schizophrenics. Horizontal lines show the means of groups.

treatment, which is used to reduce delusions and hallucinations, could continue. However, the case shows how valuable it is to be able to compare drug-treated brains with untreated specimens. It would not be possible (or desirable) to let a group of patients today forgo treatment in order to see whether their brains developed differently from those who did receive treatment. Here again, the archive of HPI was used to an end that its institutors could have never foreseen.

<sup>30</sup> Bente Pakkenberg, 'The Volume of the Mediodorsal Thalamic Nucleus in Treated and Untreated Schizophrenics', *Schizophrenia Research* 7 (1992): 95-100.

<sup>31</sup> Pakkenberg, 99.

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**3 RECENT RESEARCH (2006-PRESENT)**

When Karl-Anton Dorph-Petersen began his employment at AU, stewarding the brain collection, it was in a very sorry state. As he phrased it, “I was given a map of the basement and a set of keys – that was it”. Disappointing as this was, he was certain that with time and effort the collection would make a very valuable resource. This conviction was informed by his background in a transdisciplinary research environment, where he had learned the value of highly varied research material of the kind present in the Danish collection. His background also made him certain that the Danish brain collection would only attain its true value if embedded in an interdisciplinary network of neuroscientists, and its tissues put to use in a variety of different fields.

In this section, I describe Dorph-Petersen’s work. I begin before he returned to Denmark, as I present the case of a study on macaque monkeys performed in Pittsburgh. The purpose of this preamble is two-fold. First, it situates Dorph-Petersen within his proper scientific context, which is basic neuroscience more than either neuropathology or psychiatry. Second, and more importantly, the case demonstrates the cornerstone value of integrated transdisciplinary neuroscience and converging studies with diverse methodologies. This value would guide Dorph-Petersen’s work with the Danish collection. While a benefit in some regards, it would also prove a hindrance to quick-turnover publication output. Bridging the gap between neuroscience’s many methodologies, research areas, and disciplinary traditions is hard work.

After the macaque-case, I present the more recent case of an evaluative study of the Danish brain collection. The purpose of this study was to determine whether an epigenetic analytic method was applicable to the tissues in the Danish collection. Specifically, whether the DNA methylation profiles were well enough preserved to extract relevant data. The purpose of this section is once again to demonstrate the archival nature of the Danish brain collection. For one, the authors of the DNA methylation-study repeatedly call the Danish brains “archived” tissue, thus showing that the concept is not merely an analytic or historiographic manufacture, but an actors’ term as well. Interestingly, this also indicates that

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“archived” tissue has a different status from other tissues, collected for a specific purpose. However, the study’s conclusions also add qualitatively to the concept of the archive itself, as it points to a meeting between Rheinberger’s historiographic vocabulary and Daston’s definition of the archive. In this way, I revisit the themes from chapter 1 by way of a concrete case from the Danish brain collection. Finally, before concluding the chapter, I return to the ambition from chapter 3 of including quotidian and technical work in our understanding of scientific work.

### 3.1 Macaques in Pittsburgh: Antipsychotic medicine and brain volume

In his previous appointment, in Pittsburgh, Dorph-Petersen had been affiliated with one of the largest brain banks in the USA, in an integrated neuroscience lab with every method from imaging techniques and endocrinology to animal studies and post mortem analysis. Run by David A. Lewis, MD, this lab is focused on the prefrontal cortex in schizophrenia. In a 2002 publication, Lewis reviewed the “opportunities and challenges in post-mortem studies of psychiatric disorders”.<sup>32</sup> Here, he insisted on the one hand, “postmortem studies provide the essential insights into psychiatric disorders that can only be achieved through the direct investigation of the brain”.<sup>33</sup> On the other hand, he acknowledged the many challenges and necessary caveats involved in post mortem brain studies. In order for post mortem studies to bear fruit, Lewis pointed out several necessities: the need for well-characterised specimens;<sup>34</sup> well-constructed studies that take into account not only the theoretical feasibility and importance of the study, but also the availability of quality tissues necessary for their fruition; and finally thoroughly mapped and controlled confounds of these studies. Mapping of confounds again relies on the quality and quantity of relevant metadata of the specimens, as well as

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<sup>32</sup> David A. Lewis, ‘The Human Brain Revisited: Opportunities and Challenges in Postmortem Studies of Psychiatric Disorders’, *Neuropsychopharmacology* 26, no. 2 (February 2002): 143–54, [https://doi.org/10.1016/S0893-133X\(01\)00393-1](https://doi.org/10.1016/S0893-133X(01)00393-1).

<sup>33</sup> Lewis, 152.

<sup>34</sup> This means accurate and comprehensive clinical diagnoses; knowledge of living conditions including factors like the conditions of death, the use of medication both prescribed and otherwise, as well as alcohol and nicotine intake; detailed patient’s journals; extensive knowledge of post mortem factors such as PMI (the time between death and tissue fixation), storage length, and storage conditions.

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sophisticated use of statistics and converging studies with different methodologies, such as comparisons between human studies and experimental animal models of the phenomena under scrutiny.

Most importantly, according to Lewis, “increasing the number of postmortem brain specimens available for study” is a *sine qua non* for the success of this field.<sup>35</sup> Because psychiatric patients live complicated lives that cannot be reduced to statistical factors, confounds are impossible to rule out. Increasing the volume and diversity of specimens would secure the validity of findings; in a large enough cohort of subjects and controls, it should be possible to find brains that are similar with regard to almost every confounding factor. By making sure that subjects are paired or grouped with controls that share ante- and post-mortem conditions apart from disease and medication, and constructing elaborate databases of these groups, researchers would have a more ready access to comparable and supplementary data than can be achieved through haphazard collection. To put it another way, a great quantity of material might counter the lack of qualitative knowledge of individual specimens, because the knowledge available more easily can be made congruous across a large cohort.

One example of Lewis’ approach, in which Dorph-Petersen was involved, was a study investigating the effects of anti-psychotics on post mortem brain tissue.<sup>36</sup> In this particular example, the research question arose from studies such as Bente Pakkenberg’s 1990-study of the effect of psychopharmaca on the brain, and similar studies conducted both *in vivo* and *post mortem*. In order to attain a clear understanding of post mortem tissue from deceased schizophrenics, researchers need to know which tissue alterations are due to medical drugs, and which are not

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<sup>35</sup> Lewis, ‘The Human Brain Revisited’, 152.

<sup>36</sup> Karl-Anton Dorph-Petersen et al., ‘The Influence of Chronic Exposure to Antipsychotic Medications on Brain Size before and after Tissue Fixation: A Comparison of Haloperidol and Olanzapine in Macaque Monkeys’, *Neuropsychopharmacology* 30, no. 9 (September 2005): 1649–61, <https://doi.org/10.1038/sj.npp.1300710>; Glenn T. Konopaske et al., ‘Effect of Chronic Exposure to Antipsychotic Medication on Cell Numbers in the Parietal Cortex of Macaque Monkeys’, *Neuropsychopharmacology* 32, no. 6 (June 2007): 1216–23, <https://doi.org/10.1038/sj.npp.1301233>; Glenn T. Konopaske et al., ‘Effect of Chronic Antipsychotic Exposure on Astrocyte and Oligodendrocyte Numbers in Macaque Monkeys’, *Biological Psychiatry*, Neural Network Dysfunction in Schizophrenia: From Cells, to Networks, to Behavior, 63, no. 8 (15 April 2008): 759–65, <https://doi.org/10.1016/j.biopsych.2007.08.018>.

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and therefore may relate to the disease itself. One way of estimating the influence of medication, as Lewis noted, is by way of animal experiments.

A research group in Lewis' lab therefore obtained eighteen macaque monkeys, in order to see what would happen if they were given antipsychotic medication. The monkeys were 'experimentally naïve', which means that they had not previously been involved in any kind of scientific experiment. The research group wanted to test two different kinds of antipsychotic – haloperidol and olanzapine – and compare these with a control group who had received sham pills (placebo). Thus, the monkeys were divided into three groups, each of which received a different pill daily for approximately two years. After this time, the monkeys were euthanized and their brains removed for study.

The monkeys' brains were used in three successive studies, which all elucidated the question of antipsychotics' effects on the brain in different ways. In the first study, the whole brains themselves were weighed and measured immediately after excision, as was individual pieces following dissection. These pieces were then weighed and measured at the logical steps in the preparation procedure: "fresh, after 48 h of fixation, after ~ 12 days in sucrose solutions, after ~ 11 days in storing solution at 4°C, and finally after approximately 1 year in storing solution at -30°C".<sup>37</sup> These measurements were complemented by stereological assessments of the grey and white matter volume respectively in the parietal region of the brains. The study concluded that both antipsychotics were associated with marked reductions in both weight and volume, and that said reductions affected both white and grey matter in the monkeys.<sup>38</sup>

Next, in a subsequent study, the group assessed the numbers of different cell types within the grey matter of the brains. The grey matter is made up of both neurons and glial cells, which perform support and protection functions for the neurons. Hence, it was interesting whether the reduced volume from antipsychotics meant less neurons, less glial cells, or fewer of both. The researchers

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<sup>37</sup> Dorph-Petersen et al., 'The Influence of Chronic Exposure to Antipsychotic Medications on Brain Size before and after Tissue Fixation', 1652.

<sup>38</sup> Dorph-Petersen et al., 'The Influence of Chronic Exposure to Antipsychotic Medications on Brain Size before and after Tissue Fixation'.

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found that the density of neurons had become higher, while the total number of neurons was preserved compared to the control group. At the same time, the number of glial cells was reduced to a degree matching the shrinkage of the grey matter. They concluded that the reduction of glial cells and concomitant condensing of neurons might be considered a possible and expected post mortem observation in medicated schizophrenics.

The third study using the monkey tissue achieved yet higher detail, as the group looked at what kinds of glial cells were reduced in number and which kinds remained the same. Glial cells, as mentioned, is an umbrella term for non-neuronal cells in the nervous system. They come in several varieties, with different purposes, and in highly varying amounts. The two most frequent types of glial cells are oligodendrocytes and astrocytes. The oligodendrocytes insulate the axons by coating them in myelin sheaths, thereby increasing the possible travel speed of information. The astrocytes perform a variety of support tasks, which have e.g. metabolic, structural, and regulative functions. Citing previous studies with diverse methodological approaches, which had found less myelination in schizophrenic brains than expected, the group's opening statement was, "these studies suggest that subjects with schizophrenia have fewer oligodendrocytes and that those present are functionally impaired".<sup>39</sup> The question, then, was whether this dearth of oligodendrocytes was caused by the medication, or if it was a symptom of the disease itself. Lewis' group hypothesised that the macaque monkeys would show a comparable decrease in oligodendrocyte-numbers, thus indicating the antipsychotic medication as the cause. Upon investigation, however, the result turned out to be opposite. Their paper concludes:

In contrast to our hypothesis, this study demonstrates that the lower grey matter glial cell number associated with chronic haloperidol and olanzapine exposure in macaque monkeys is due mainly to a lower astrocyte number, whereas oligodendrocyte number appears to be affected to a smaller degree. Thus, these findings provide only weak support of the interpretation that the findings of lower oligodendrocyte

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<sup>39</sup> Konopaske et al., 'Effect of Chronic Antipsychotic Exposure on Astrocyte and Oligodendrocyte Numbers in Macaque Monkeys', 759.

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number in schizophrenia are attributable to antipsychotic medication.<sup>40</sup>

While pathologists should expect to see a reduced grey matter volume, and lower number of overall glial cells in medicated schizophrenics' post mortem tissue, and therefore these features could not be taken as a direct disease symptom, it would seem that the reduction of oligodendrocytes seen in earlier studies of schizophrenia were not caused directly by the medication.

Taken together, these results might prove valuable in future attempts at post mortem (and perhaps even *in vivo* imaging) studies of schizophrenics' brains. Knowing which features are not caused by the disease itself, but by external or associated factors, allows researchers to isolate and focus on the unexplained alterations associated with the pathology. Going back to Lewis' 2002 review, the value of animal studies is that they allow testing for such exogenous effects in non-pathological tissue, thus avoiding confounding results by having too many variables.

What is most important to note for my purpose here, going back to the uses of the Danish brains, is the integration of different disciplines in addressing neuropathological theories. The macaque studies are summarised here because they clearly demonstrate the importance of converging studies. Without previous studies in humans, such as Pakkenberg's 1990 study, the Pittsburgh team would not have known which specific changes to investigate in their macaque monkeys. Indeed, all three macaque articles cite a wide range of post mortem, *in vitro*, and *in vivo* studies of schizophrenia in order to form their hypothesis and interpret their results. Likewise, the importance of their study ultimately relies upon it providing the basis for future studies in humans.

A convergence of different methodologies, research materials, and traditions in the pursuit of a common theoretical insight. Steven Rose pointed with his designation of the "data-rich and theory-poor" neurosciences towards the lack of such an approach. Though limited in this case to schizophrenia, the antipsychotic case provides a nice example of the kind of disciplinary integration necessary to

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<sup>40</sup> Konopaske et al., 762.

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become theory-rich. The key concern is the kind and number of variables affecting the studies: If the same overall result is achieved in studies with highly different confounding factors, the core insight can be expected to hold true.

Coming from this environment, it was clear to Dorph-Petersen that the value of the brain collection lay not in its being used internally by a closed group of local researchers addressing only post mortem pathological issue, but instead in its possibilities as a contributing resource for a wide range of scientific concerns and disciplines. Its strength was that it was able to complement knowledge and insights from diverse fields, by attempting to replicate findings in a unique material with other variables than e.g. fMRI-studies, animal models, and in vitro experiments. A pivotal point, therefore, was the extent of knowledge about the brains and the patients they had come from. As noted previously, the Danish brains had all come from patients dead in hospital care, most of them after chronic hospitalisation. Therefore, the patients' journals were extensive, as was knowledge of patients' nutrition, work, sociality, family, and substance use. The brains themselves were also exceptionally well described and, thanks to Lorentzen, they had been histologically processed in a uniform manner. This would make comparisons and crosschecks easier, and allowed for detailed selection of specimens according to the variables that might confound a given study. An obvious example would be the separation of smokers from non-smokers or, as in the case of Pakkenberg, to select only non-medicated subjects. Before Dorph-Petersen would be able to perform such detailed selections, however, he would have to catalogue and sort the information, spread out in diverse formats and locations, into a database. As noted earlier, he had received only "a map and a set of keys" to a collection that no one had paid any custodial attention to for two decades.

Beginning to work with the collection thus meant first gathering and ordering data and metadata; second, establishing scientific networks across the neuro-disciplines; and third, conducting 'pilot studies' in order to assess the material viability of the tissues. The third point is crucial, not only in order to know what large-scale studies would be likely to pay off, but also in order to generate 'trust' in the collection – to show interested parties that, yes, it can be very useful. For scientists to take an interest, and for funding bodies to support research, hard data

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on the material possibilities was needed. The first point, summarily called data-management, is a long-winded and complex task, which remains uncompleted in 2019, as will be clear from chapter 7.

### **3.2 Reconstituting the archive: DNA methylation**

Establishing networks and collaborations is a central element in heightening the value of a scientific collection. In accordance with the Pittsburgh emphasis on convergence of methodologies and materials in addressing scientific questions, Dorph-Petersen stressed in one of our conversations the importance of what he called “collateral benefits”.<sup>41</sup> Working with the collection is not only about conducting research on the tissue, it is also a matter of establishing and building a milieu of interested and collaborating scientists, who discuss and engage with each other’s fields, methods, and insights. He thought of his first five years at least at Risskov as a matter of building up a “critical mass” of researchers, technicians, and approaches, which together would form a high-volume “pipeline” for the output of results and publications.

To this end, Dorph-Petersen initiated a range of pilot-studies that tested the possible experimental approaches to post mortem tissue in the Danish collection. There was some uncertainty as to the effects of long-term fixation with regard to different processing techniques. For example, what effect did long-term formalin exposure have on the different cell elements? Would it make a difference to the histological picture if staining was carried out 40 years after initial fixation, as opposed to days or weeks? Most of these pilot projects consisted in staining diverse cuts from both fixed and paraffin-embedded tissue, in order to produce sample images for presentation slides, information folders, conference posters, and so on (see Figure 10). To get people interested, showing concrete photos of successful stains was much better than simple verbal insurance of the collection’s quality.

Some questions of technical feasibility also became present later in Dorph-Petersen’s appointment, as theory and technique developed in other strands of neuroscience. For example, the burgeoning field of epigenetics raised the prospect

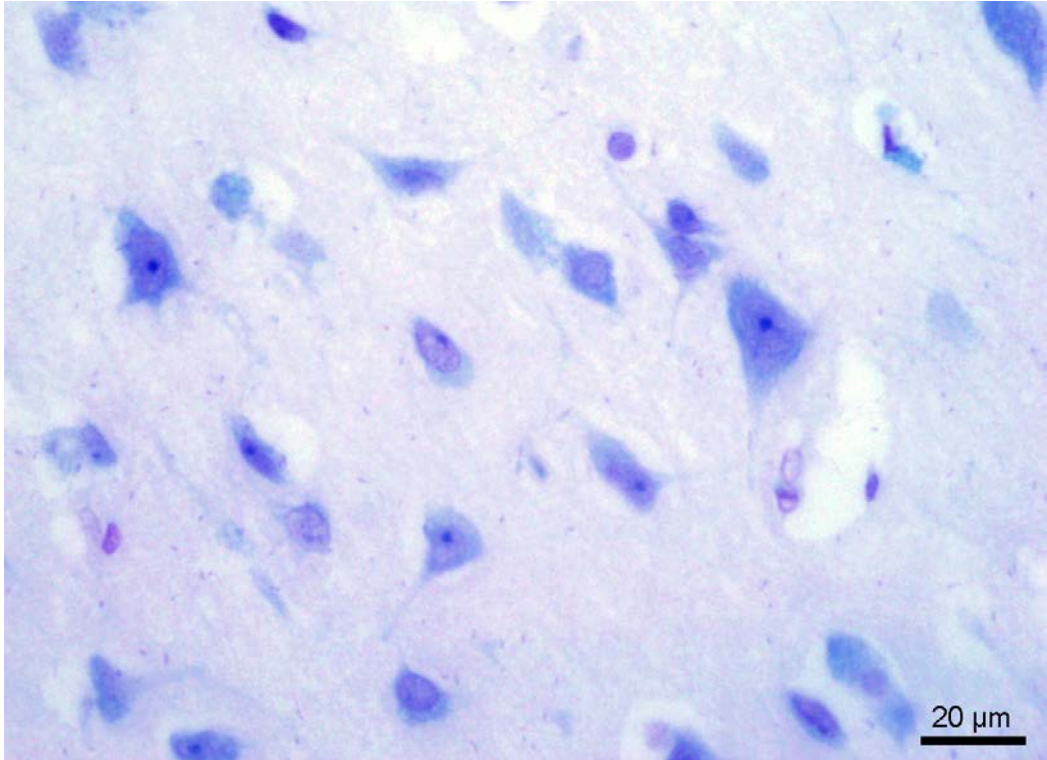
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<sup>41</sup> Karl-Anton Dorph-Petersen, interview by Thomas Erslev, 15 October 2019.

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for a new use of the Danish collection. Briefly, epigenetics is the study of the environment's impact on the phenotype by way of alteration of gene expression, and the inheritance of these impacts. It does not assume that the environment alters the genotype – the DNA itself – but rather that it causes changes in the processes by which the DNA is translated into cellular functions and protein synthesis. One such change is called DNA-methylation.



**Figure 10**  
One of Dorph-Petersen's sample images, which shows that high-quality stains are unproblematic even in the long archived tissues. Pyramidal neurons from a brain slice, which was Giemsa-stained after 53 years in formalin.

Photo: Karl-Anton Dorph-Petersen

Methylation is a chemical process in which a methyl group ( $\text{CH}_3$ ) is either added to a molecule, or substitutes an atom in a molecule, thus effectively creating a new molecule. In human DNA, this occurs in one of DNA's four bases, cytosine (the other three being adenine, guanine, and thymine). Changing one of the bases in a DNA string can result in altered gene expression, by inhibiting transcription from

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DNA to RNA. In this way, a genome can be ‘turned off’, so that while it is still there, it is not ‘read’, and therefore its function ceases. Such an inhibition can have a wide range of consequences, depending on the site in the body, and the DNA sequence that is methylated. Methylation can also coincide with other chemical alterations and thus initiate even larger processes. For our purposes, it is enough to understand that methylation changes the way the genes are read, without changing the genes themselves.

Since ca. 2010, DNA methylation patterns had been observed to correlate with particular psychiatric and neurodegenerative disorders, notably schizophrenia, bipolar disorder, and Alzheimer’s disease.<sup>42</sup> When such discoveries are made, it is important to attempt to replicate the findings in as wide ranging materials as possible. Hence, old brain banks like the Danish one could be a useful resource for further epigenetic studies, especially since the brains are so well documented. The problem, however, was that the quality of formalin-fixed long-term stored paraffin encased tissue was unknown for DNA methylation analyses. Several factors might influence the integrity of both the DNA and its methylation profile: E.g. duration of fixation, storage temperature, and the fixative itself. In other words, it was uncertain whether such studies could even be carried out in the old tissue.

A team of medical researchers, psychiatrists, and mathematicians at Aarhus University therefore conducted a study to evaluate the usability of what they called the “long-term archived brain formalin-fixed paraffin-embedded samples” in the Danish brain collection.<sup>43</sup> They selected 30 samples among the 9479 available brains. The brains were divided between the three pathologies schizophrenia, bipolar disorder, and Alzheimer’s disease. The team proceeded to test the feasibility of the different available methods for DNA-methylation analysis, and list the differences between these results and those from fresh frozen tissue samples. They concluded somewhat ambivalently that while the DNA itself seemed generally to be broken up and fragmented in the formalin-fixed paraffin-

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<sup>42</sup> Stine T. Bak et al., ‘Evaluating the Feasibility of DNA Methylation Analyses Using Long-Term Archived Brain Formalin-Fixed Paraffin-Embedded Samples’, *Molecular Neurobiology* 55, no. 1 (1 January 2018): 669, <https://doi.org/10.1007/s12035-016-0345-x>.

<sup>43</sup> Bak et al., ‘Evaluating the Feasibility of DNA Methylation Analyses Using Long-Term Archived Brain Formalin-Fixed Paraffin-Embedded Samples’.

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embedded samples, the methylation profile itself remained relatively intact. The low quality DNA, however, meant that the input data was substantially lower than from fresh frozen tissue, and this made the use of some techniques almost impossible.

The article is relevant for several reasons. For one, the authors themselves call the Danish brain collection an archive, and indicate that its possible usefulness lies in the age and diversity of samples. These reflections add further proof to Daston's characterisation of the archive as open-ended and opportunistic. Not only that, but upon closer reading the article also supports Rheinberger's account of the interdependency of matter and thought, of theory and practice, of epistemic things and experimental systems.

I discussed Rheinberger's historiographic conception of science in chapter 1, and related it to the concept of the archive, but it would be instructive to see this exemplified in a case from the Danish brain collection. I provide two quotes from the methylation article's conclusion here, which will serve to bolster Rheinberger's claims, and elucidate the material, temporal, and archival nature of the Danish brains more clearly.

[...] obtaining valid data with some of the currently used DNA methylation analytic platforms is not trivial. To this end, the implementation of methodologies designed to work on short single-stranded DNA fragments in low copy numbers should be considered.

DNA methylation analyses in FFPE [formalin-fixed paraffin-embedded] samples are not straightforward using standard experimental protocols. However, since DNA methylation patterns appear intact, our data support the implementation of quality assessment in future studies in order to minimize inclusion of confounding samples.<sup>44</sup>

What stands out to me in these quotes is the fluctuation between established practice on the one hand, and adaptation to new objects on the other. First, we learn that obtaining data from the Danish samples is neither straightforward nor trivial by way of contemporary standard practice. However, the data is there, and

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<sup>44</sup> Bak et al., 679.

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because of the previously mentioned archival features of the brain collection, it is potentially highly valuable data. Therefore, rather than discard the material as unsuited to standard methods, the authors propose the implementation of new methods, and selection of those already available methods that best suit the material under scrutiny.

Looking at this from Rheinberger's perspective, we are reminded that epistemic things & experimental systems are co-constituted. A new epistemic phenomenon (DNA methylation) emerges, which allows for new ways of theorising function and disease. In order to approach this phenomenon, a variety of preparations is needed. The 'normal' preparation is fresh frozen tissue, which after some years of refinement now allows routine analysis. The experimental system has become stable, and with it the epistemic thing more trivial. However, with the introduction of a slightly different preparation, namely FFPE samples, the epistemic thing again becomes unruly. The first question is 'will it be available by way of our current experimental system?' When the answer is a 'yes, but...', attempts are made at adapting and restructuring the experimental system to better suit the new epistemic thing, which simultaneously is more narrowly defined (through quality assessments), and thus excludes part of its own original material (FFPE samples that do not pass initial assessment). Thus, the advent of a new epistemic thing challenges the function of the previously trivial experimental system, and necessitates a not straightforward reconstitution of said system. In this reconstitution, in turn, the epistemic thing is also redefined. The two elements are always co-constitutive.

The case demonstrates clearly the archival interplay between research question and material, between infrastructure and specimen selection, between matter and theory. Not only the material itself takes on new dimensions, which must be accounted for in the selection process, the possible engagements with it are also changed *through* the engagement with the material collection. The archive simultaneously alters and is altered by its uses. The feasibility of DNA methylation analyses on the brain collection has opened up new uses and promises, effectively remaking the collection and giving it new purposes. At the same time, the introduction of FFPE samples to studies of DNA methylation has changed this field

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methodologically and with time perhaps theoretically, as older specimens provide data that are more varied.

It will be interesting to follow, in the coming years, how DNA methylation analysis will be applied to the Danish brains. Not long after the publication of the article read here, the collection was moved to Odense, where it became part of the inter-disciplinary brain research centre BRIDGE at the University of Southern Denmark. Evaluative studies, such as Bak et al. have no doubt been critical for showcasing the collection's possibilities, as their work of updating the archive to modern uses convince others of its value.

### 3.3 Sample selection; salvaging tissue

Beside the DNA methylation evaluation, Dorph-Petersen informed me two forthcoming publications involving tissue from the collection.<sup>45</sup> One is a stereological study of the volume and cell count in the hippocampus of sufferers of depression and schizophrenia, and cases of suicide – some comorbid, some only belonging in one category, to see whether there is any correlation between hippocampal anomalies and these three conditions.<sup>46</sup> This work continues Dorph-Petersen's interest from his PhD-thesis which also investigated brains from cases of depression and suicide, albeit in a different brain region and with a slightly different scope.<sup>47</sup> The other publication is a neuropathological re-classification of cases from the Danish brain collection that show signs of frontotemporal lobar degeneration, which is an umbrella-term for a kind neurodegenerative suffering of which FTD-3 (described above) is a subtype.<sup>48</sup> The Danish brains, like in other instances, provide a valuable resource here, because they are many and well documented, and thus affords a range of analytical entry-points to such diseases.

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<sup>45</sup> Dorph-Petersen, interview, 15 October 2019.

<sup>46</sup> Fenghua Chen et al., 'Hippocampal Volume and Cell Number in Depression, Schizophrenia, and Suicide Subjects', *Brain Research*, 9 November 2019, 146546, <https://doi.org/10.1016/j.brainres.2019.146546>.

<sup>47</sup> Karl-Anton Dorph-Petersen, 'Neuronal Changes in the Dorsal Raphe Nucleus in Depression?' (PhD thesis, Faculty of Health Sciences, University of Aarhus, 2001).

<sup>48</sup> MS Melnikova, K-A Dorph-Petersen, and IE Holm, 'Frontotemporal Lobar Degeneration: Neuropathological Classification of Cases from the Brain Collection at Aarhus University Hospital, Denmark', [in preparation].

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Apart from these three publications, Dorph-Petersen has published a wide range of methodological papers, and collaborated on studies in other labs.

As noted in chapter 3, working with the collection means more than studying the brains and publishing results. I have already described the organisational and administrative work involved in keeping the collection, but my talks with Dorph-Petersen has revealed a range of archival tasks in planning, designing, and successfully carrying out experiments. The most important of these, in archival terms, is selecting tissue samples that match the importance and scope, as well as the scientific requirements of the study in question.

Karl-Anton Dorph-Petersen once described to me the shift in work ethics that followed his move from Pittsburgh to Aarhus. While in Pittsburgh, he had been used to putting in requests for tissue relevant to his concerns, or he had been part of collaborative projects where the tissue was already at hand. In 2006, after his move to Denmark, he was suddenly the one receiving these requests. In Robert Kohler's conceptual frame, Dorph-Petersen had gone from being a 'finder' of scientific facts to being a 'keeper' of information-bearing objects.<sup>49</sup> This shift was most evident in the many new practices he had to learn.

A fictional example will help clarify the keepers' practices and concerns. A group of researchers is interested in diseases X and Y, and they suspect these diseases are affiliated with alterations in brain regions B and R. These brain regions are also known to be affected by external factors S and F, as well as a psychotropic medication M. They put in a request to Dorph-Petersen for specimens from the Danish brain collection. He must now consider several factors: Is there a specific reason for using the archived brains, rather than collect fresh ones? Is the study well thought out with regard to possible confounding factors? Are there enough relevant brains in the collection for the study to be meaningful – if not, have requests for supplementary material been put in elsewhere?

If all these initial concerns are answered satisfactorily, the selection process begins. Here again a host of complications arise. First, because the database for the

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<sup>49</sup> Kohler, 'Finders, Keepers'. I return to Kohler's distinction in section 2 of chapter 5, albeit for a slightly different purpose.

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Danish brains has been incomplete, the logistics of selection are not trivial. Information necessary to select for factors X, Y, S, F, and M may be spread out in different files for each brain, and must be drawn together. The methods of the study must also be taken into account. As we saw in the case of DNA methylation not all brains are suitable for all investigative tools. This adds storage conditions and time, as well as previous engagements with the tissue to the list of selection criteria.

When a suitable number of brains have been found, that represent all the sought after characteristics, and are complementary in terms of confounding factors, there is the matter of excising the relevant brain regions. Here, both anatomical knowledge and surgical skill are paramount. In a study such as the macaque-study described above, where the monkeys were bred for a specific purpose, the relevant tissue is cut out with less regard for the non-relevant parts of the brain. If, in a subsequent study, another part of the brain becomes interesting, it is simply a matter of collecting new tissue, of breeding new monkeys.<sup>50</sup> In a brain collection such as the Danish one, the amount of tissue is fixed, and so it becomes a responsibility to preserve as much brain material as possible, while conducting experiments. Brain region R may be bordering on region T, which would be useful in a later study, and therefore should not be destroyed in the course of excising region R.

Dorph-Petersen told me about the intricate work of cutting out the hippocampus without destroying bordering regions.<sup>51</sup> The cut is laid by hand and scalpel, and in some places there is a mere 5 mm between two regions of interest. In order to judge where to lay the cut, the physician must be well familiarised with the anatomy of the whole brain, as well as the individual regions, and must have a keen sense of the signs that highlight the border (as brains and their anatomical structure varies from individual to individual). If the cut is too wide, future studies

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<sup>50</sup> This point is slightly overemphasised here. In practice, scientists always take care when excising tissue for research. Not least out of respect for the animals, and because such experiments are laborious and expensive.

<sup>51</sup> Dorph-Petersen, interview, 15 October 2019.

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may lack crucial material destroyed in the process. If it is too narrow, you end up damaging the tissue necessary for the current investigation.



**Figure 11**

An excised hippocampus from a brain in the Danish collection. Note the size compared to Dorph-Petersen's fingers. The tissue is cut by hand, which requires extreme precision.

Photo: Karl-Anton Dorph-Petersen.

When selecting tissues for study, the key terms are planning and foresight. Going back to the macaque studies, it was paramount that the three studies were done in the specific sequence chosen. If researchers had first looked at the distribution of different glial cells, and only then thought to enquire about brain

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weight, it would have been too late. In order for the first study to make sense, the brain must be weighed at many different stages of processing, not only at the end. So with all studies in the Danish collection, Dorph-Petersen as its custodian must see ahead at possible future uses of the same brain, or even the same region. This is another reason why a large collaborative network of neuroscientists across disciplines is beneficial. Allowing different methodologies and disciplines access to the same material, diversifying the approaches, solidifies the results when different methods converge in similar results.

#### 4 CONCLUDING REMARKS

This chapter has presented an overview of research conducted on the Danish brains since HPI began collecting in 1945, until today. The first period saw a continuance of Lárus Einarson's scientific project, described in chapter 1. After HPI shut down, the collection was used in several investigations, including studies of multigenerational sufferers of the same diseases, inheritable dementia, and comparison of medicated and unmedicated schizophrenics. Finally, since 2005 the collection has undergone extensive archival and scientific updates, to prepare it for use with modern research methods. Together, this survey of research outputs demonstrates the varied and at times surprising possibilities and uses of the Danish brain collection.

The chapter elaborates on points from previous chapters. I have shown how Einarson continued to be a presence at HPI, even after Lorentzen had taken over the directorship. The quotidian work, which I introduced in chapter 3, continues to be highly important for the collection's use to this day, as exemplified by Dorph-Petersen's work. Finally, the concept of the archive introduced in chapter 1 has been expanded in several ways. The collection has been used to unforeseen ends, in accordance with Daston's determination of archives as both opportunistic and open-ended. When it was left without custodianship, it quickly lost value and relevance, and it took great amounts of work to institute a sufficient logistical and experimental system, in order to make it a viable research resource again. This attests to the important addition from Rheinberger's epistemology of the concrete,

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that also in the archives conceptual and material work are co-dependent, and scientific entities are co-constituted by both.

As a final note, this chapter has shown how different valuable scientific insights have been drawn out of the archive. In the next three chapters, I engage specifically with the concept of value, and show how it connects with the Danish brain archive.



## *Chapter 5*

# Value and waste in the brain collection

In the first part of this thesis, I have investigated the scientific and quotidian brain collection, focusing largely on the time while the HPI was still active. In this and subsequent chapters, I turn my attention to what has happened since the Institute's closure. The brain collection largely ceased to be a scientific resource and a workplace. However, in the course of the 1990s and 2000s it was gradually enrolled in public debates and imaginations as a synecdoche for topics as diverse as ethical travesty, zombie horrors, and a wasteful public sector. At the end of this period, from 2015, the brains were on their way to a waste processing plant or crematorium, though ultimately they would be preserved and archived once more. The preceding chapters have focused on practices and discourses in scientific texts, while the source materials in the last three chapters will be of a more lay and popular kind. My overall concern with materiality, temporality, and the archive remain central, though they will be refracted through different concepts, notably 'value' and 'waste'.

In this chapter, I introduce the concepts 'waste' and 'value' both theoretically and by way of exemplary case studies. I begin, in section 1, by briefly summarising some recent scholarship on 'value' as an analytical as well as an actors' category. Valuations have been a perpetual part of public discussions of the brain collection, so it is necessary to introduce this concept before delving into specific case analyses. Section 2 presents 'waste' as both a theoretical and applied concept. I argue for a temporal understanding of waste, and show how such a concept is

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useful in studying brain collections. I do this first in an analysis of a discussion of the state of brain banking among British experts in the field, and second in reviewing the case of the Danish brain collection and its reinstatement since 2006, as seen through the scope of ‘waste’. In the course of section 2, it will become clear that analysing ‘waste’ is also analysing ‘value’, and that their mingling draws clear strings to the notion of the scientific archive. Finally, in section 3, I expand both concepts, ‘waste’ and ‘value’ with reference to the brain collection, in a reading of a children’s novel about the collection. Here, scientific, ethical, and political concerns are pushed into the background, while gore, shock, and humour highlight a different set of values and ideas in the public discussions of the brain collection. These three sections will help the reader shift focus from past to present, and from a history of scientific practices and discourses to a history of public opinion exchanges and popular imaginaries.

The primary purpose of this chapter is to introduce the coupled concepts of ‘waste’ and ‘value’. While doing this, I also present three empirical cases, two of which revolve around the Danish brains. The three cases rely on widely different kinds of source material. Therefore, a few words on the choice of these materials:

In subsection 2.2, to show the usefulness of a temporal concept of waste in analysing brain collections, I study a Wellcome Witness Seminar entitled “The development of brain banks in the UK c.1970-c.2010”.<sup>1</sup> The Wellcome Witness Seminars are described on their web-page as events where “significant figures in twentieth-century medicine are invited to discuss specific discoveries or events in recent medical history”.<sup>2</sup> The discussions are chaired by a “witness” and subsequently transcribed by historians who are present during but do not participate in the discussion. This format excludes many voices from the debates (e.g. patients’ interest groups, politicians, donors), but it does give participants a

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<sup>1</sup> Caroline Overy and E. M. Tansey, eds., *The Development of Brain Banks in the UK c.1970-c.2010: The Transcript of a Witness Seminar Held by the History of Modern Biomedicine Research Group, Queen Mary University of London, on 12 November 2013*, Wellcome Witnesses to Contemporary Medicine 53 (London: Queen Mary University of London, 2015), <http://www.histmodbiomed.org/witsem/vol53>.

<sup>2</sup> ‘Wellcome Witnesses Volumes | The History of Modern Biomedicine’, accessed 7 March 2017, <http://www.histmodbiomed.org/article/wellcome-witnesses-volumes>.

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sense of belonging and commonality: in this case, all invitees were either researchers or managers at UK based brain banks. This in turn gives room to the actors to delve into more esoteric concerns, and touch on themes and controversies that might not feature in public discussions involving outside perspectives. Because the participants all belong to the same group, they feel comfortable discussing ethically and politically difficult aspects of their practice, and are able to stay with specific and technical issues which would be swiftly dismissed in other settings. The material is also useful because it brings together actors from different collections and different areas of research in a single sitting, making it possible to identify shared concepts and assumptions across the field.

In subsection 2.3, I return my attention to the Danish brains, and to Karl-Anton Dorph-Petersen whose work as the collection's daily manager was discussed in chapter 4. I have in the course of my PhD -studies developed a relationship with Dorph-Petersen. We have met frequently and worked together, for example by conducting a public symposium on the history and current status of the brain bank, and by co-writing a bibliography and meta-analysis of the bank's research output – work that is still in progress. His thoughts on managing a brain bank have naturally entered into our conversations on many occasions, and he has read and commented on some of my drafts. His observations and concerns are at the centre of subsection 2.3. Because of the frequency and nature of our meetings, the information gathered from him does not fit the pattern of a formal interview. Some of it is from conversation, some from email correspondence. Dorph-Petersen has reviewed and accepted the sections in which he is paraphrased or quoted.

In section 3, as noted, the object of study is a children's novel. Fiction is a wonderful resource, in this case because it displays some of the cultural assumptions that I have met also when discussing my project at public events. It does this by relying on well-known tropes, and by radicalising the historical figures into archetypes, that are more easily analysed as proxies for the common ideas that attend the brain collection. The associations in this case are to zombies and ghosts, to being subjected to wrongful medical treatment, and to being buried alive. Through my analysis, I show these associations to be related to temporal and

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material concerns with the meaningfulness of life and death. This is something that is ever present, but rarely expressed, in discussions of the brain collection.

#### I VALUE

Both people and objects can hold values. Often, the values people hold are understood as pre-existing convictions, which form the basis of actions for a historical actor. In such an understanding, value is an explanatory concept. For example, we could say that Einarson initiated the collection because he valued direct observation. Objects, on the other hand, have no such direct cause of action. However, their value can sway a human actor in a certain way. Objects are traded, guarded, and fought over because of value, which can be either practical, monetary, or sentimental, and sometimes all three at once. Whether one understands “value” financially, therapeutically, epistemically, or ethically, it is always contested. Actors will invariably deem different things valuable, and will hold some values dearer than others, leading to disagreement and misunderstanding. Conceptions of value are pivotal in questions of research and collection.

In the volume *Value Practices in the Life Sciences & Medicine*<sup>3</sup>, the editors encourage a pragmatic approach to value as grounded in practice, as the outcome of work. They hold that: “The making of values happens in practice and that therefore a study of values must pay attention to the bricolage of making them”.<sup>4</sup> In this understanding, value(s) is no longer the explanation or underlying reason for an actor’s actions or discourse, but rather is the phenomenon to be explained. Values are not stable *pret-à-porter* things simply adopted, determining behaviour. They are themselves the products of complex social and material practices, as shown through the anthology’s various case-analyses. This pragmatic approach is useful here, because it allows, even encourages, a pluralistic value-concept. Different actors have different values because they engage in different practices in different contexts. Dussauge, Helgesson, and Lee encourage crossing traditionally distinct domains, in order to see how seemingly incommensurable values are

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<sup>3</sup> Isabelle Dussauge, Claes-Fredrik Helgesson, and Francis Lee, eds., *Value Practices in the Life Sciences and Medicine*, First Edition (Oxford, United Kingdom: Oxford University Press, 2015).

<sup>4</sup> Dussauge, Helgesson, and Lee, 6.

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continually compared, conflated, separated, and juxtaposed. This is a welcome encouragement with regard to the Danish brain collection, where discussants come from all sorts of backgrounds, with highly varied agendas and expertise, and where traditional professional boundaries are difficult to uphold, as we shall see.

Studying the value of objects gets more complicated the more stakeholders are involved, because different actors often imbue objects with different values. Both concerning amount (in pecuniary terms), and about the kind of value an object is said to hold, disagreements proliferate. This is especially obvious in medical science, where the value of life itself is necessarily measured against pragmatic concerns of money, time, and resources. Health professionals require wages, instruments, and labs among other things, and proper care of patients requires beds, facilities, medicines, and not least time. All these are more or less scarce, and increasingly so.

In her anthropological MA-dissertation, *Prioritizing Healthcare*, Eva Otto reported on her fieldwork conducted in the Regional Council for Central Jutland, and in a major hospital in the region. Her focus was on “economic and moral value(s) in the Danish healthcare system”.<sup>5</sup> In the region, she identified “efficiency” as the cornerstone value, and she further explained that efficiency in this context was to be understood in a tabular, statistical, numerical, and abstract way. In short: budgetary. Her dissertation presents several examples of council members legitimising decisions with reference to some value, e.g. treatment efficacy, patient autonomy, etc., and then adding in every instance “and also better use of our resources”. Every council decision seems to both improve service *and* cut costs. In this insistence on what she calls perpetual “win-win situations”, Otto sees an indication that cost-efficiency has become a latent but pervasive and ordering cornerstone value in the regional council. It is rhetorically relegated as secondary when describing the decision-making process, but Otto wonders, since it is everywhere present, how secondary it really is. In the case of the brain collection, the same concern was voiced by several stakeholders, who worried exactly that financial motivations had overshadowed scientific and ethical considerations.

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<sup>5</sup> Eva Otto, ‘Prioritizing Healthcare’ (MA, Aarhus University, dept. of anthropology, 2017).

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When I choose to focus this and following chapters on “value”, it is because this concept, understood multiply, makes possible a general understanding of why the brain collection has been so extensively debated. Stripped of meanings and values, it is ‘just’ a pathological collection, and in that sense rather innocuous. Had it been a collection of gangrenous feet or tubercular lungs, it is hard to imagine the same public interest. Two things make the brain collection such a drawing topic of discussion: It consists of brains, and they were collected without consent. These two facts alone carry so many values that almost everyone has an opinion as soon as they hear about it. We are convinced that we are “cerebral subjects”, and that the brain is the site of the self.<sup>6</sup> The collected items are perceived not as body parts, but almost as entire individuals. If the brain is not in the coffin, can we really say the deceased has been buried? This also makes non-consensual collection so much more horrifying. Though it would be no less wrong to collect lungs non-consensually, I doubt it would cause the same outrage.

## 2 WASTE AS AN OPERATIVE TEMPORAL CONCEPT IN BRAIN BANKING<sup>7</sup>

In this section, I will demonstrate how a temporal concept of *waste* may be used analytically to better understand the temporalities at play in contemporary brain banking. By temporalities, I mean the ways in which ideas about pasts and futures subtly structure practices and discourses, and how in turn collection practices inform the future as they (re)construct the past and present. I do this from within the same literature that was at the centre of chapter 1, namely the academic study of collections, especially medical collections and museums.<sup>8</sup>

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<sup>6</sup> Vidal and Ortega, *Being Brains*.

<sup>7</sup> This section is based on the published article: Erslev, ‘A Brain Worth Keeping?’ I have revised and altered the text to make it fit into the broader context of the thesis, but many paragraphs are unaltered, and the overall argument remains the same. A previous version of some parts of the text also appeared in my master’s dissertation; Erslev, ‘Collecting Brains’.

<sup>8</sup> Elizabeth Hallam, *Anatomy Museum: Death and the Body Displayed* (London: Reaktion Books, 2016); Rina Knoeff and Robert Zwijnenberg, eds., *The Fate of Anatomical Collections, The History of Medicine in Context* (Farnham, Surrey; Burlington, VT: Ashgate, 2015); Tybjerg, ‘From Bottled Babies to Biobanks: Medical Collections in the Twenty-First Century’; Karin Tybjerg, ‘From Pathological Collections to Biobanks: Medicine as a Collection Science’, *Medicinsk Museion*, 14 July 2014, <http://www.museion.ku.dk/2014/07/from-pathological-collections-to-biobanks->

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I proceed in three steps. Section 2.1 introduces the theories that inform my understanding of *waste* and *collection*, which are the concepts that will structure the following analyses.

In section 2.2, the concepts are put to work in an analysis of concerns about *keeping* in a British brain banking context. The aim here is twofold: First, to demonstrate the applicability and usefulness of the concepts introduced in section 2.1, and second, to tease out some easily overlooked concepts of temporality and their importance in determining waste and value. This is done with a particular view to materials stored *in* brain collections.

Section 2.3 returns to the Danish brains, as it applies itself to a discussion of the brain collection itself as something more than the physically stored material. Specifically, it points to the importance of infrastructures and experimental systems that also go into constituting a collection. While doing this, it continues the overall interest in the temporalities at play in brain banking, and in the ways in which *waste* is useful for understanding said temporalities.

### 2.1 Theories of waste and collection

Brain banks, I will argue, are explicitly tied in with figurations of pasts and futures through these organising concepts: *waste* and *collection*. In this subsection, I will introduce the key theories and concepts that I make use of in the subsequent case analyses. Section 2.1.1 introduces a temporal concept of *waste*, as opposed to a merely spatial one. Section 2.1.2 further explores this concept by defining three analytical types of waste. In section 2.1.3, I revisit the themes from chapter 1, and expand further my understanding of *collections* and the collecting sciences, as well as the collectors themselves.

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medicine-as-a-collection-science/; Strasser, 'Collecting Nature'; Samuel J M. M. Alberti, *Morbid Curiosities: Medical Museums in Nineteenth-Century Britain* (Oxford, New York: Oxford University Press, 2011); Alberti, 'Objects and the Museum'; Kohler, 'Finders, Keepers'; Parry and Gere, 'The Flesh Made Word'; Parry and Gere, 'Contested Bodies: Property Models and the Commodification of Human Biological Artefacts'.

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### 2.1.1 WASTE AS A TEMPORAL CONCEPT

In a definition that has become commonplace in studies of waste,<sup>9</sup> Mary Douglas has claimed that dirt is “‘matter out of place’ and our attempts to be neat and clean do not relate so much to the things themselves but how we arrange, separate and order our psychophysical environments”.<sup>10</sup> As such, waste and practices of collecting (selecting what is kept and what is not) can be co-constitutive. Waste is that which is not worth keeping. William Viney has complicated Douglas’ spatial understanding of waste by supplementing it with temporality, arguing that “Waste is also (and in both senses of the phrase) matter out of time”.<sup>11</sup>

Viney argues that ‘waste’ as both verb and noun plays a key role in our temporal understanding of the world. He introduces two distinct temporal categories: *use-time*, the time of useful things:

The uses projected and achieved through [things] inform the limits of their time. In this way, use-time is explicitly future orientated, absorbed in the possibility that I might realize some latent or potential utility, but, in being so, [these things] become committed to a future in which a temporal limit remains an immanent part of their use. [...] Use-time is a time of wearing, emptying, digesting, breaking or exhausting – it is a somewhat entropic time, a time of diminishing potential, a time orientated to and by an end.<sup>12</sup>

and *waste-time*, an “anachronistic tense, one that commingles pasts and presents and leaves things lingering, loitering and persisting above and beyond the time of its functionality”.<sup>13</sup> Our lives, he argues, are fundamentally structured by narrative; an order which naturally assumes an end. However, when things end (when they are no longer a meaningful part of a narrative), they do not disappear, but rather *become* waste, which then has to be disposed of, removed. Waste, as Viney

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<sup>9</sup> see William Viney, *Waste: A Philosophy of Things* (Bloomsbury, 2015); Brian Thill, *Waste, Object Lessons* (New York: Bloomsbury Academic, an imprint of Bloomsbury Publishing, Inc, 2015); Greg Kennedy, *An Ontology of Trash: The Disposable and Its Problematic Nature*, SUNY Series in Environmental Philosophy and Ethics (Albany, NY: State University of New York Press, 2007); Waldby and Mitchell, *Tissue Economies*, pt. 2.

<sup>10</sup> Viney, *Waste*, 1.

<sup>11</sup> Viney, 2.

<sup>12</sup> Viney, 7–9.

<sup>13</sup> Viney, 178.

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understands it, is “both a material continuity and a temporal discontinuity”.<sup>14</sup> Waste, then, is matter out of time before it is out of place. Stuff enters the ontological category of waste by being wasted: material and word are co-constitutive, but both depend on temporality.

**2.1.2 ANALYTIC CATEGORIES OF WASTE**

In order to better make use of the category in my analyses, I will now divide it into more specific subcategories: *constitutive waste*, *virtual waste*, and *ontological waste*. They are informed mainly by Catherine Waldby and Robert Mitchell’s analyses of the meanings of waste in tissue economies.<sup>15</sup>

Constitutive waste is a kind of destruction, which is productive – the kind of waste invoked in saying ‘you can’t make omelette without breaking eggs’. Waldby and Mitchell understand it as “waste that is the precondition for producing “the new”. [Waste that] is not in and of [itself] valuable but only manifest[s] *potential value*; [it is] waste, in other words, that promise[s] value”.<sup>16</sup> The kind of value referred to here is called “biovalue”, which “refers not to the stable and known properties of tissues but to the capacity of tissues to lead to new and unexpected forms of value”.<sup>17</sup> In biovalue terms, objects are not valuable in and of themselves, but only as medium of monetary or epistemic value. Biovalue exists in many kinds of waste and non-waste material. The defining property of constitutive waste is that *potential* or *promise* which is invoked in favour of constructive destruction.

Virtual waste refers to some imagined loss, present or future. It is a concept picked up from a set of American congressional hearings addressing that innovation was presumably: “going to waste as a result of bureaucratic red tape and illogical government regulations”.<sup>18</sup> This common trope in American ideology can as easily be applied to the value of tissues as to innovation. Virtual waste in this form is more like a feeling that things are not moving fast enough. It is an annoyance, an impatience with seemingly unnecessary obstacles that waste both

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<sup>14</sup> Viney, 29.

<sup>15</sup> Waldby and Mitchell, *Tissue Economies*, pt. 2.

<sup>16</sup> 109.

<sup>17</sup> 108.

<sup>18</sup> Senator Birch Bayh, quoted from: Waldby and Mitchell, 102.

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time, energy, and resources. However, virtual waste can also conceptualise the fear that using an object now might preclude a more promising use later. Often, when conducting scientific experiments, the objects under study are destroyed or at least fundamentally changed in the course of the experiment, making subsequent studies with other methods difficult. Virtual waste, then, can take place in the same process as constitutive waste. New facts are created now, but at the same time later uses that might have proven even more fruitful are wasted. Virtual waste, it must be specified, “[cannot] be measured directly, of course, and so the specter of the future loss of inventions function[s] as a virtual image”.<sup>19</sup>

If virtual and constitutive forms of waste regards the potential that certain objects may or may not hold, ontological waste is of another, perhaps more primary, kind. As we are dealing in this instance with human tissues, some of them may be inscribed with ontological meaning for a subject. Especially heads, fetuses, and eyes are often imbued with ontological significance beyond the life of the subject. This kind of meaning is not covered by the term biovalue, but is inherent in the objects themselves. Waldby and Mitchell observe that “human tissues are more likely to be classified as waste as they lose ontological significance”.<sup>20</sup> They are thinking here of such neutral kinds of tissue as hair or nail clippings, or stuff that is outright threatening to the subject’s stability, what has been termed ‘the abject’: faeces, puss, and cancerous cells. Such materials must be disposed of, lest the person itself wastes away. What Waldby and Mitchell seem less interested in (though not oblivious to), is the temporal changes objects may undergo *pace* Viney, and the possibility that ontologically significant tissues may *become* waste, given enough time: “waste is every object, plus time”.<sup>21</sup>

#### 2.1.3 COLLECTING SCIENCES: FINDERS AND KEEPERS

A collection, according to John Elsner and Roger Cardinal, “is the unique bastion against the deluge of time”.<sup>22</sup> While this may be true of personal collections

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<sup>19</sup> Waldby and Mitchell, 102.

<sup>20</sup> 84.

<sup>21</sup> Thill, *Waste*, 8.

<sup>22</sup> John Elsner and Roger Cardinal, ‘Introduction’, in *The Cultures of Collecting*, ed. Roger Cardinal and John Elsner, Critical Views (London: Reaktion Books, 1994), 1.

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and some (older) museums, it certainly is not so for medico-scientific research collections; they seem rather to be vehicles for change. As discussed in chapter 1, Bruno Strasser and Karin Tybjerg have argued for a revision of the standard narrative in the history of biology and medicine, respectively, in which experimentalism and the laboratory revolution has been considered the main causes behind scientific development since the late nineteenth century.<sup>23</sup> Strasser and Tybjerg argue on the basis of different historical and contemporary cases that collections (be they natural history collections, medical museums, or modern biobanks) as well as their related “ways of knowing”<sup>24</sup> (collecting and comparing) have played much larger roles in the recent histories of these sciences than they are usually ascribed.

Robert Kohler’s distinction between scientists that are “finders” and those that are “keepers” is useful here.<sup>25</sup> Though Kohler equates all collecting scientists with keepers,<sup>26</sup> I borrow his vocabulary to distinguish between two ideal types of brain collectors. Kohler’s distinction rests on the value of the collected objects: whether they have an “intrinsic value as objects”, or “they are simply unprocessed data, and in yielding up those data they are *used up*”.<sup>27</sup> Note that Kohler’s distinction fits naturally with Viney’s vocabulary of use and waste. *Finders*, in my analysis, will be those actors that see primarily the use-time of an object, or for whom the most immediate form of waste is *constitutive waste* or, as Waldby and Mitchell defined it: destruction for the sake of (scientific) progress. For finders, collections that are not in use will most often constitute an unproductive kind of waste: waste of time, space, or resources. *Keepers*, on the other hand, will be those actors that focus on the *virtual waste* (i.e. those insights that might be lost in the destruction of objects) or who consider collections part of a rich heritage, as necessary elements in an unfolding narrative, and oppose destruction of collected materials as a form of *ontological waste* to be avoided for its own sake.

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<sup>23</sup> Strasser, ‘Collecting Nature’; Tybjerg, ‘From Bottled Babies to Biobanks: Medical Collections in the Twenty-First Century’.

<sup>24</sup> see Pickstone, *Ways of Knowing*.

<sup>25</sup> Kohler, ‘Finders, Keepers’.

<sup>26</sup> Kohler, 432.

<sup>27</sup> Kohler, 432. My cursive.

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If Kohler's vocabulary is useful in distinguishing different types of collectors, the conceptual work by Cathy Gere and Bronwyn Parry is helpful in parsing out different types of objects: "information-containing objects, resplendent in all their three-dimensional uniqueness, and information-bearing surfaces, transmissible in all their two-dimensional replicability".<sup>28</sup> The idea is that certain sciences gather knowledge by making objects readable, but what results from this making-legible is often the destruction of the original object, and with it all the other unrealised potential information it might still contain. This complicates the determination of value in objects, and raises the possibility that using an object *now* may hinder scientific insights *in the future*. Negotiating this chiasm between present future, and future present, brain collectors and researchers have to make daily choices based on their view of the probable value of virtual and constitutive waste forms. Which brain best serves its purpose now, as constitutive of a new research object that may yield important insights; rather than in the future, preserved until neuroscientific research methods have made new unimaginable means of analysis possible? Connecting this pair of concepts with Kohler's, keepers will deem information-containing objects more valuable, whereas finders will try to produce as many information-bearing surfaces as possible.

#### 2.2 Valuable waste? – finding and keeping

In this section I will show how a temporal understanding of waste, along with the ideal types of *finders* and *keepers* can usefully be applied to a discussion about contemporary British brain banking. I show how the analytical categories highlight otherwise easily overlooked concepts of temporality. The aim is primarily to test the applicability of the conceptual tools, and to bring attention to the temporal logics that structure internal debates in the field of brain banking.

Though the discussions repeatedly return to issues of donation and exchange of material, I have focused my analysis in section 1.2.1 on those passages where temporalities of *keeping* are central to the arguments in line with the overall scope of the paper with its focus on kept material. It turns out that disputes about what

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<sup>28</sup> Parry and Gere, 'The Flesh Made Word', 47.

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to keep revolve around the equally contentious definition of *value* in research material, which is the focus of section 1.2.2.

**2.2.1 WASTING THE FUTURE OR KEEPING THE PAST?**

Early in the Wellcome Witnesses discussion, two of the brain bankers took opposite positions with regard to the temporal criteria for accepting donations. Dr. Djordje Gveric, manager of the MS and Parkinson's Tissue Bank at Imperial College, worried that he and his team "have far too many brains for our own capacity",<sup>29</sup> and he wondered how appropriate it would be to turn away tissue donations based on their likely lack of usefulness. He mentioned patients who have been registered as donors *initially* because their symptoms were relevant to research at the institution, but who "are *becoming* cases that are not really interesting to researchers".<sup>30</sup> Note that the use-time of diminishing potentiality is central to this argument. The longer these brains are 'in use', the less is their value when considering different uses.

This view, that brains should be turned away according to their loss of usefulness was opposed by Professor Margaret Esiri, an Emeritus neuropathologist at Oxford. She countered: "I think it can be difficult to predict what people are going to be interested in [... and] it's hard to predict exactly what could be valuable in the future".<sup>31</sup> She then described her own research on Alzheimer's and multiple sclerosis in which she uses brain tissue that is usually 'low priority'.<sup>32</sup> In Esiri we find a clear exponent for the 'keeper' approach. She argued for maintaining collections of a wide variety of brain types, pathological and otherwise, in case they become 'valuable in the future' – it might be a waste to throw them away. Gveric, on the other hand, was clearly of the 'finder'-opinion that collected tissues should be used, and even noted the temporal limit of usefulness, as cases are 'becoming less interesting' as they age. They 'waste' whatever biovalue might have been

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<sup>29</sup> Gveric in: Overy and Tansey, *The Development of Brain Banks in the UK c.1970-c.2010*, 41.

<sup>30</sup> Gveric in: Overy and Tansey, 42. [my cursive]. The issue is specific to multiple sclerosis, in which the pathology is most easily observed in younger brains as the disease gradually 'destroys' its own lesions, leaving 'blanks' which tell very little of the aetiology or pathogenesis.

<sup>31</sup> Esiri in: Overy and Tansey, 43.

<sup>32</sup> Compare with the cases of Familial Danish Dementia and FTD-3 presented in chapter 4. Here also, "low priority" tissue became useful in studies of very rare diseases.

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present in the tissue at an earlier stage (this is of course a most happy form of waste, as it means patients live much longer than expected). However, he had ethical qualms over turning donors away:

**Gveric:** It's just about people who were registered at some point and who have lived happily for about 20 years, occasionally ask: 'I'm in my seventies now or eighties; am I really useful to you as a donor?'

**Shaw:**<sup>33</sup> And do you feel able to say 'no'?

**Gveric:** No, I don't feel able to say 'no'. You have to really judge the character over the phone and say: 'How do you feel about it?' and then it's a discussion. We have quite a few of these cases.

**Shaw:** We have enquiries in the same way but we are able to say 'no' in a nice way. It's explaining to people that actually it's not going to be valuable for research.

**Gveric:** It's more about the actual active approach, not just waiting for people to come into you, because we probably have more than 10,000 people on the donor scheme. Out of those 10,000 I'd say maybe 2,000 really shouldn't be on the donor scheme for all sorts of reasons.

**Shaw:** But you could, even at the time of death, say to the relatives, 'Actually...'

**Gveric:** It's very, very difficult. It's very difficult because then it's the lasting gift.

**Shaw:** I know, and you're disappointing them. Yes, I understand.<sup>34</sup>

This exchange introduced the issue of meaning for the donors' personal integrity. The last will of the deceased must be honoured, or it may be traumatic for their next of kin, even if this means 'wasting' resources on keeping material that is not useful for research. There is a double responsibility at work here, and a double ontology in which 'waste' for Gveric cannot be considered 'waste' for donors and next of kin, and it would be disrespectful to even raise the issue after the time of

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<sup>33</sup> Ms Karen Shaw is research/ brain donation nurse at the Queen Square Brain Bank for Neurological Disorders at UCL Institute of Neurology.

<sup>34</sup> Overy and Tansey, *The Development of Brain Banks in the UK c.1970-c.2010*, 43.

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death. Professor Seth Love, Director of the South West Dementia Brain Bank, Director of the UK Brain Banks Network and President of the British Neuropathological Society, seconded this stance: “if people have registered with brain banks and they’ve been on the donor register for several years it would be wrong not to accept their brains”.<sup>35</sup> Love and Gveric were of the view that respect for personal choice and the donors’ right to believe in the meaningfulness of their ‘lasting gift’ should trump professional valuation of the brains, at least at the time of donation.

Professor Paul Francis, Director of Brains for Dementia Research and member of the MRC UK Brain Banks steering committee, argued for a different, perhaps more pragmatic, ethical codex, in which the value of the tissue directly correlated with the ethics of keeping. Rather than the eye-to-eye ethics between donor and receiver, he highlighted the ethical responsibility to secure the usefulness of collections:

It would be unethical for us to keep brains in our collection that are never going to be used [and the public needs to be made aware] that we will then dispose of the tissue if it is not being used or it’s not being requested, I think that’s something that we’ve got to move to: that we’re not going to keep these collections forever; people understand that they will have – I don’t want to use the phrase in the way that it’s used in a supermarket – a shelf life; these brains will be in the bank for a certain amount of time and then they will be replaced by other brains.<sup>36</sup>

Again, as with Gveric, *time* is key in Francis’ idea of wasting space and resources on what is to him essentially waste material. His is an understanding of waste as fundamentally temporal as Viney also suggested. Even though he denied the supermarket metaphor, it plays out not only in the limited shelf life of the brains, but also in the use of the word ‘requested’ – there is a supply-demand logic here, which plays badly with the ‘keeper’ frame of mind in which demand does not have primacy. Even though Francis and Esiri both agreed that demands may change, they posited opposite solutions, supported by different temporal attitudes. Esiri

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<sup>35</sup> Love in: Overy and Tansey, 45.

<sup>36</sup> Francis in: Overy and Tansey, 44–45.

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focused on storing material for the future as they may yet gain usefulness and thus still hold some potential, whereas for Francis unused materials are simply remnants of a past that has outlived itself, they have definitively lost their usefulness and entered waste-time.

Esiri is one of the only exponents for the ‘keeper’ approach of safeguarding objects for the future. It seems that many of the other discussants connect *usage* with *value*. Gveric believed that the public will become more interested in brain banking if “there’s something coming out of it, something valuable”.<sup>37</sup> Love called for more interdisciplinary uses to “maximise the value of the tissue that we have”,<sup>38</sup> and Professor David Mann, coordinator of the Manchester Alzheimer’s Disease Research Centre, summed up the discussion nicely:

I thought it was really important to document that we’re not just setting up a network that acquires, but we’re also setting up a network which utilizes, so that the rate of utilization at least matches or outweighs the rate of acquisition; that we’re just not stamp collecting, because that’s always the danger, isn’t it – that you feel you’re just collecting stamps at the end of the day?<sup>39</sup>

This choice of metaphor is an interesting one, as the hope for stamp collections usually is that they rise in value the longer they are kept, though for Mann clearly value comes from usage. I now return my attention to the question of value explicitly, as I continue my analysis of the Wellcome seminar.

#### 2.2.2 BIOVALUE IN BRAIN COLLECTIONS

Who is producing the value, who is benefitting from it, who is it potentially denied to, what form does it take, what counts as valuable, and who has the right to define it as such? These questions are touched upon several times in the witness seminar. Take for example Professor Hugh Perry, Chair of the MRC Neuroscience and Mental Health Board, who says:

Every day all over the UK there are dozens of neurosurgical units *discarding* bits of human brain tissue. You were saying earlier,

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<sup>37</sup> Gveric in: Overy and Tansey, 62.

<sup>38</sup> Love in: Overy and Tansey, 64.

<sup>39</sup> Mann in: Overy and Tansey, 65.

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David [Mann], how your early studies had benefited from taking tiny samples of fresh human tissues with which you can do all sorts of things that you might not otherwise do. It has always puzzled me that there isn't a part of brain banking which involves the capture of small half-centimetre cubes of post-surgical tissue, which I would have thought *invaluable*. You could learn an enormous amount.<sup>40</sup>

Being able to 'do all sorts of things' is always of interest to scientists, but the 'enormous amount' of knowledge that is going to waste is obviously imagined in this instance (that is not to say there is no wastage, but as with all virtual waste, it cannot be measured directly). What is deemed invaluable by Perry here is not so much the amount of tissue, though, as it is the *freshness* of it. This is discussed elsewhere in the seminar, by Esiri who complains that the 'death to post mortem' intervals are too long for brain removal:

So the value of the tissue is greatly reduced. I say greatly, there's still a lot you can do with it, but certainly it would be more valuable if we could bring those times down, and there seems to be no understanding of the importance of this.<sup>41</sup>

Note the key importance of time once again. There seems to be agreement about the cause of these prolonged intervals: bureaucracy.<sup>42</sup> Mann notes that mortuary staff members, for example, "are quite happy to participate and reduce post mortem times, but they're hide-bound by Trust bureaucracy".<sup>43</sup> Francis seconds, and adds that it is in everyone's best interest to lower waiting times and raise the quality of the collected brains:

We can engage with GPs and persuade them that this is an important part of eventually helping patients – okay, it will be a generation further on – and the more we find out about research now, in the long term if we can improve and develop new treatments, it's actually going to reduce the load. [...] The other people, and this is bureaucracy, are the R&D departments and the chief executives who control mortuaries. R&D departments in hospitals or Trusts gladly sign off projects, such as Brains for Dementia Research, and from the lack of communication with

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<sup>40</sup> Perry in: Overy and Tansey, 73. My cursive.

<sup>41</sup> Esiri in: Overy and Tansey, 50.

<sup>42</sup> Overy and Tansey, 50–55.

<sup>43</sup> Man in: Overy and Tansey, 50.

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other people involved, mortuaries and chief executives, you'd think they weren't on the same planet.<sup>44</sup>

So as it was with American innovation, important and valuable information is literally wasting away, decaying, because of bureaucratic red tape holding up the smooth flow of tissue.<sup>45</sup>

As the discussion turns to another kind of regulation: intellectual property rights and the relationship with industry, Perry asks: "How do we feel that industry should use our brain banks? Djordje [Gveric], you've got an excess of brains; are industry clamouring to use your excess of brains?"<sup>46</sup> It turns out that Gveric's brains are much sought after, but not so easy to get to:

The issue is that there's obviously a line referring to intellectual property in terms and conditions for every single tissue request, saying that they need to inform us of something that might be commercialized, something they might be making profit from, and so on. That's the real problem: whether someone who is signing the contract in a pharmaceutical company will agree to it. There's a lot of negotiation and renegotiation going on, obviously very complex if it starts involving the company, Imperial College lawyers, and charity lawyers as well.<sup>47</sup>

The real issue, though, turns out to be the question of ownership. This is much too big a question to explore here in its entirety.<sup>48</sup> Suffice it to say that donated tissue is placed into the custodianship of the collection in question, and that in principle human tissue cannot be owned (according to British law). This is a problem in for-profit industry, as it is hard to claim intellectual property in non-private materials:

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<sup>44</sup> Francis in: Overy and Tansey, 54.

<sup>45</sup> for a discussion of tissue regulations in the British context, see Bronwyn Parry, 'The New Human Tissue Bill: Categorization and Definitional Issues and Their Implication', *Genomics, Society and Policy* 1, no. 1 (2005): 74–85.

<sup>46</sup> Perry in: Overy and Tansey, *The Development of Brain Banks in the UK c.1970-c.2010*, 75. Note in this quote the very explicit ideas about excess, waste, and use.

<sup>47</sup> Gveric in: Overy and Tansey, 75.

<sup>48</sup> but do see Parry and Gere, 'Contested Bodies: Property Models and the Commodification of Human Biological Artefacts'.

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**Brayne:**<sup>49</sup> The reason they want to own the tissue is that they will then be able to make profit on what they've discovered, is that right?

**Jenkinson:**<sup>50</sup> It's the specification [from industry] that they actually own the tissue that causes problems because it's a donation.

**Brayne:** [...] The problem is that the pharmaceutical approach doesn't fit that stewardship on behalf of humankind.<sup>51</sup>

There is general agreement that research results should benefit all of humanity, and that this should be the first concern before profits. This rhymes well with something Kohler describes as typical of the 'keepers' of science: that they are more prone to consider their data communal and make it public. For 'finders' "raw data are regarded as private: because, I think, they are in principle unlimited"<sup>52</sup> in contrast to collections with their 'thing-y' materiality, "resplendent in all their three-dimensional uniqueness".<sup>53</sup>

Perry underscores that the main value of data is not financial but scientific, and that for science to progress, data must be shared and connected. This means both making it publicly accessible, 'legible' in Parry & Gere's terms, and upholding its material specificity, its connection to a particular collected brain:

I think it is a hugely important issue that the data that is collected from any type of analysis from a particular brain comes back to a database relating to that brain; this is the most useful way forward. [...] If all this information came back to a source we would have a richness that would be truly valuable.<sup>54</sup>

This quote sums up the different perspectives on value nicely, arguing for a 'keepers' approach not only to objects, but also to the data of the finders, making a subtle synthesis between past insights and future ones, turning future knowledge

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<sup>49</sup> Professor Carol Brayne is professor of Public Health Medicine and Director of the Cambridge Institute of Public Health and the University of Cambridge.

<sup>50</sup> Dr Joanna Jenkinson is part of the Medical Research Council where she manages the mental health and addiction portfolios.

<sup>51</sup> Overy and Tansey, *The Development of Brain Banks in the UK c.1970-c.2010*, 77.

<sup>52</sup> Kohler, 'Finders, Keepers', 449.

<sup>53</sup> Parry and Gere, 'The Flesh Made Word', 47.

<sup>54</sup> Perry in: Overy and Tansey, *The Development of Brain Banks in the UK c.1970-c.2010*, 80.

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into the facts of the past, which again become foundations of new theories. Data must be found, but not without keepers of both material objects and immaterial information about those objects. Only if kept is it truly valuable. Collections thus, are not only important as reservoirs of empirical material for researchers. They also serve the vital role of scientific archives that facilitate comparisons and meta-studies, guarantee verifiability, and preserve even seemingly irrelevant data in the hopes that it might be of use to future scientists.

### **2.3 Wasting the bank? – Problems of inheritance**

In subsection 2.2, we saw understandings of temporality tied up with ideas about waste, utility and value. Of special concern was the longevity of research material (how long is its “shelf life”? will it become valuable in the future?), the PMI and the freshness of tissue (how quickly can decay be stopped?), the ‘becoming-useless’ of cases as patients grow older, and therapeutic hopes for the future. Most if not all of these concerns are about the tissues *in* the collection and, though temporal in nature, they are tied up in the *now* of acquisition and research. Further, the discussions were mostly concerned with brain banking in general, not with a particular bank or practice.

In this subsection, I will continue the analysis of time, waste and value, as I now turn my attention to the Danish brain collection. This move makes it clear what happens when we consider the collection *itself* as an object of interest, not just the tissues in it. Our focus thus shifts from the temporality of tissues to the timeframe of the bank itself, in this case spanning seven decades. It makes possible a closer look on the importance of practice in brain banking, and highlights once again the intricate interplay between material and conceptual work.

First, I note a dichotomy between two perspectives, one concerned with the singularity of the collected brains, the other with the conglomerated entity of the collection as a whole. I then move on in section 2.3.1 to discuss a particular predicament of Dorph-Petersen, who experienced troubles with “inheriting” a collection stripped of its surrounding infrastructures; his predicament gives evidence to all that makes up a collection besides its materials. This discussion

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leads me to propose that we distinguish between the use-time of the materials in a tissue bank, and of the bank itself.

In 2005, the Danish brain collection was brought to new life with the proposition of several new research projects involving tissues from the collection (see chapter 4). Between 1982 and 2005, the bank had been left without any maintenance or supervision. Although it had been used in a few research projects, it stood neglected and without purpose. The new research proposals, however, brought funding and personnel to the collection, and also sparked popular interest. Stories about the brain collection had been circulating irregularly in the press since the early 1990s, as is the subject of chapter 6. Here I will just review one response to the non-consensual collection after the news that the collection was to be brought back into use.

In a newspaper article couplet devoted to the brain collection's revival, and its contested ethical status, one woman suspecting her deceased sister's brain to be part of the collection, commented: "it seems a tad grotesque that your "heirloom" may have been made into preservative without you knowing".<sup>55</sup> Central in this quote is the strange choice of the word "heirloom [arvegods]". The word is indicative of the complex metonymical meanings that can be attributed to deceased bodily material.<sup>56</sup> It invokes a sentimental value, because while heirlooms can be of high monetary value, they are more often defined by a sentimental value in the form of familial narrative, generational ties, and identity building. The word also signifies a focus on the singularity of collected materials; one brain becomes different from the rest, because it is spun into one person's personal narrative.

In the original article, the above quote was juxtaposed with the position of Professor Raben Rosenberg MD, DMSc, the director of the collection. In this

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<sup>55</sup> quote from Jørgen Bollerup Hansen, 'Pårørende kræver besked om hjerner', *Århus Stiftstidende* 8. maj 2005, 8 May 2005.

<sup>56</sup> see Sarah Tarlow, 'Curious Afterlives: The Enduring Appeal of the Criminal Corpse', *Mortality (Abingdon, England)* 21, no. 3 (2 July 2016): 210–28, <https://doi.org/10.1080/13576275.2016.1181328>; and Susan C. Lawrence, 'Beyond the Grave - The Use and Meaning of Human Body Parts: A Historical Introduction', in *Stored Tissue Samples: Ethical, Legal, and Public Policy Implications*, ed. Robert F. Weir (Iowa City: University of Iowa Press, 1998), 111–42 for two very different, though not opposing, treatments of the subject.

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juxtaposition, a dichotomy emerges between the singular object implied by the woman's use of such signifiers as "sister", "mother", and "heirloom", and the more abstract conglomerated entity that is the entire collection, implied when Rosenberg refers only to "research material" in his answers, stressing the collection as the object of concern rather than the individual materials in it.<sup>57</sup>

Though not as singularly focused as the Danish woman, the British bankers in section 2.2 were predominantly concerned with the materials *in* their collections: the quality and characteristics of the material, the relationship to donors, &c. When the issue is collection practices, such concerns must enter into the discussions. The Danish collection, however, invites a different perspective because active collection ended in the 1980s. It can be viewed, in this regard, as a contained entity, stable in its entirety, in contrast to actively collecting banks, which are harder to conceptualise as *objects* because of their continued expansion and alteration.

#### 2.3.1 INHERITING THE BANK WITHOUT INFRASTRUCTURE

Following Rosenberg, we now shift our focus from the individual brain to the entirety of the collection. Strasser<sup>58</sup> and Tybjerg<sup>59</sup> have convincingly argued (as argued in chapter 1) that medicine relies as much on collections as it does on laboratories. Further, they both stress the longevity of collection practices in medical research. Strasser underlines, however, that historical similarity is not the same as continuity. Commenting on recent decades' increase in data-driven research, he calls modern databases *analogous* to earlier natural history collections, that is they share a "common structure and function", but he is wary of calling them *homologous*, that is of a common descent.<sup>60</sup> By modern databases here, we are to understand post-1980 scientific large-scale data collection. Considering the quotidian practices of the Danish collection presented in chapter 3, and the likeness there identified between the HPI and 19<sup>th</sup> century German scientific

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<sup>57</sup> Hansen, 'Pårørende kræver besked om hjerner'; Jørgen Bollerup Hansen, 'Sygehus vil gerne give pårørende vished', *Århus Stiftstidende* 8. maj 2005, 8 May 2005.

<sup>58</sup> Strasser, 'Collecting Nature'.

<sup>59</sup> Tybjerg, 'From Bottled Babies to Biobanks: Medical Collections in the Twenty-First Century'.

<sup>60</sup> Strasser, 'Collecting Nature', 336–37.

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psychiatry, this distinction between homology and analogy might hold an important lead. If the Danish collection was effectively ‘pre-modern’, how was it to become a modern research bank in 2006? <sup>61</sup>

When discussing an earlier draft of this text with Karl-Anton Dorph-Petersen, he mused on the fluidity of the *keeper/finder* categories. He thought it funny that he had been ‘brought up a finder’, but had seen himself ‘become a keeper’. In other words, he was educated in a system that placed great emphasis on experimental practice and on discovering new facts. As discussed in chapter 4, he had then spent his formative years as a *finder* in an American brain bank, before coming to Risskov in 2006 where the work with the HPI-collection required a shift in work ethics and practices, as he spent more and more time on management and maintenance of the collection. <sup>62</sup>

Continuing on this, he speculated that it is probably easier to start up a new brain bank than to inherit one. Having done research in modern brain banks before, he was surprised by how much work would be required before even basic research could begin. There are several reasons for this, the main one being a lack of maintenance between 1982 and 2006. When Dorph-Petersen took over the collection, it was in a very sorry state, having been left to itself in remote basement rooms for 24 years. The buckets, some of which had begun deteriorating, spilling formaldehyde, had to be replaced. The patients’ records had to be updated with modern diagnostic terminology, and preferably be digitised. The collection itself had to be organised, making it easy to identify which brain was where, and allowing for cross-searches among the metadata of both patient and tissue. Finally, but not negligibly, the rooms had to be cleaned of dust, spider-webs, and rodent droppings.

In chapter 1, I argued with Tybjerg and Strasser that tissue collections are as much a part of biomedical research as laboratories, and that it made sense to think of them in terms of experimental systems, which Hans-Jörg Rheinberger has defined as “not simply experimental devices that generate answers; experimental systems are vehicles for materializing questions. They inextricably cogenerate the

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<sup>61</sup> Though the research proposals are from 2005, funding and research did not commence until 2006.

<sup>62</sup> Karl-Anton Dorph-Petersen, interview by Thomas Erslev, 25 November 2016.

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phenomena or material entities and the concepts they come to embody”.<sup>63</sup> By extension, as described in chapter 1, experimental systems themselves are co-constituted with the epistemic things of the day. Moreover, epistemic things are unstable. They exist with the express purpose of giving way to facts. Once a fact has been produced, what happens to the experimental system that corresponded with its epistemic thing?

Tobias Rees has recently shown the myriad kinds of painstaking work required in maintaining and developing an experimental system, and the fluctuations such systems see as, for example, post-doctoral researchers come and go, leaving unfinished research projects for the next in line.<sup>64</sup> As an anthropologist in Alain Prochiantz’ lab, Rees followed and contributed to the lab’s scientific work on the ‘plastic brain’ for the duration of his fieldwork. His book ends on a nostalgic note, as Rees realised he had to “let go” of the plastic brain which was no longer an epistemic thing, but had become fact: “the plastic brain elaborated in Alain’s lab no longer has a center. It has become globally dispersed. The experimental system had itself become a black box, a building block of other experimental systems”.<sup>65</sup>

What Dorph-Petersen effectively had to do in 2006, when he was appointed daily director of a brain collection which had stood unsupervised for 24 years, and which was epistemically geared towards decades old research questions, was pry open just such a black box. When Prochiantz’s experimental system ceased to generate questions, once its epistemic thing became fact, it had to be developed and reconstituted around new epistemic things and concepts, in order for new questions to materialise. This did not happen at HPI. Dorph-Petersen, in subsequent discussion, clarified his statement on inheriting a collection: to him, the difficulty resulted from a lack of infrastructure. When he began working with the Danish collection, there was no lab dedicated to it, no staff, and no logistics. In other words: no experimental system. The problem with this, in his words, was

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<sup>63</sup> Rheinberger, *Toward a History of Epistemic Things*, 28.

<sup>64</sup> Rees, *Plastic Reason*.

<sup>65</sup> Rees, 279n1.

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that “the organically symbiotic growth of the collection and its infrastructure is lost when only the physical collection remains”.<sup>66</sup>

Tybjerg has argued convincingly that the similarities between pathological collections and biobanks far outnumber the differences. According to her, the main difference “is one of methods – lab tests rather than vision, hearing and touch”.<sup>67</sup> If we follow Rheinberger’s definition of the experimental system, then new methods also generate new epistemic things, and new concepts for these things. We might understand the “symbiosis” between collection and infrastructure as a metaphor for this relationship, in which case the problem of inheritance is, at least partly, that the inherited material does not fit the experimental system. The tissue in the bank is geared towards generating research questions of a different time.

Indeed, no small effort has been put into “updating” the data and metadata pertaining to the material: the case files of each brain have been combed through for outdated diagnoses, in which case a re-diagnosis has been attempted, estimating a suitable modern substitution based on noted symptoms, pathological examination, and other available data and tools. In other words, the concepts are actively changed in order to enrol the collection in a modern experimental system.

The epistemic things have undergone a similar refashioning, to better suit the framework of a modern biobank in which “diseases are now understood as vast causal webs with nodes both at the molecular scale of the body and in the environment from the womb onwards”.<sup>68</sup> In addition to physically altering the objects in new ways (for example by utilising new histological stains not available to the original researchers at HPI), modern research is making new epistemic things from the collection by novel methods. As shown in chapter 4, this happens for example by way of stereology, and by introducing the archived tissue samples in epigenetic studies of DNA methylation.

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<sup>66</sup> Karl-Anton Dorph-Petersen to Thomas Erslev, ‘RE: Citat’, 21 February 2017.

<sup>67</sup> Tybjerg, ‘From Bottled Babies to Biobanks: Medical Collections in the Twenty-First Century’, 274.

<sup>68</sup> Tybjerg, 276.

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All this work, which is both conceptual and material, is coupled with a time consuming expansion and restructuring of the collection's infrastructure. What emerges is a new biobank, one which is homologous, in Strasser's terms, with modern biomedical science but whose materials, the brains collected between 1945 and '82, are not. They have been given a new use-time by becoming new objects both materially and conceptually.

The continuing co-development of research questions and practices keep biobanks from outliving themselves; keep them in use-time. Recall that use-time is not so much defined by the present as it is by the future. If experimental systems are "vehicles for materializing questions", so are collections, and only so long as they generate new and intriguing possibilities for research do they have a future. However, we must not be fooled and think of the collection only as the materials it holds. It was not primarily because the specimens were old or decayed that HPI was shut down, but rather because the institute still belonged to a bygone research tradition homologous with the 19<sup>th</sup> century. Similarly, it was not new materials that allowed it to re-enter use-time, but novel methods, and visions of a new experimental system generating new questions, new things, and new concepts.

Viney's temporal vocabulary of waste makes a case for expanding how we think of the time of collections. Not defined by the longevity of the material within them but by their narratively and practically constructed potential for the future, the use-time of brain banks becomes more a question of the experimental systems, the infrastructures, the metadata, and the discourses pertaining to them, than a question of material integrity and subsistence.

In subsection 2.2, we heard British brain bankers lament different types of waste in collection and research practices. A main issue was the material specificity of scientific data. Devoid of their material substrates, findings lose scientific validity, usefulness, and value. In Dorph-Petersen's case, it was not the facts, but the material that had become stale. Going back to the dispute between Esiri and Francis, we have in the HPI-bank a case where Esiri's future-oriented stance – that kept materials may always regain usefulness – is vindicated, but which also echoes parts of Francis' supermarket logic: if the material is not "requested" to be enrolled

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into dynamic experimental systems, it truly does become waste. When *merely* kept, without attending to the infrastructures (material as well as conceptual) which uphold an experimental system, collections simply waste away. These infrastructures include the archived data from previous studies in the collection, but they also include: curation, scholarly networks, technical staff, instruments, metadata, and much more.

### 3 THE BRAINLESS CEMETERY

In Nykøbing Sjælland, Denmark, an old hospital's cemetery and adjoining chapel has been dubbed "the brainless cemetery".<sup>69</sup> Buried here are some 1900 human bodies without brains. The bodies are those of mental patients who died while submitted to the State Mental Hospital of Nykøbing Sjælland. One telling of the story unfolds in a horror book for children aged 10-15, in which two boys discover an undead wandering the cemetery in search of his wrongfully displaced brain, and decide to help him find it.<sup>70</sup> The novel is based on the real practices at HPI,<sup>71</sup> and it highlights affective value as an important aspect of the brain collection's social significance.

In this section, I will analyse some passages from the book, to show the tropes and associations it draws upon. While doing so, I investigate further two of these associative elements: the cemetery and the undead. The cemetery invites thinking about time and its value in a certain manner, while the second, the idea of coming back from the dead, raises questions of body/spirit-interaction. Both the cemetery and the undead invites reflection on what it means to live meaningfully, and thus introduces values that are rarely discussed yet often present in medical science.

Michael Næsted Nielsen's *De Hjerneløses Kirkegård* opens with a prequel chapter, in which a routine autopsy including brain removal goes wrong: the body on the autopsy table is not dead but merely unconscious. When the doctor

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<sup>69</sup> Mette Hansen, 'De Hjerneløses Kirkegård', *Religionslæreren*, no. 4 (2013): 6-7; Michael Næsted Nielsen, *De hjerneløses kirkegård* (Rørvig: Facet, 2010).

<sup>70</sup> Næsted Nielsen, *De hjerneløses kirkegård*.

<sup>71</sup> Næsted Nielsen, 129-30. In a brief afterword, the author accounts for the real HPI, and informs his readers both of the ongoing research on AD and schizophrenia, and of the ethical debates.

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performing the autopsy discovers this, his hacksaw is already halfway through the skull of the man, and struck by panic he continues sawing until the man is dead. He then removes the brain:

There was a silent suction noise as the cavity let go of the brain. It was still warm and gleaming with moisture. It was bleeding from where the veins had been attached. Perhaps it still had some life in it. The brain cells might not all be dead. He placed it in a plastic bucket with formalin, which would prevent it from rotting. The corpse remained, now without scalp and brain.<sup>72</sup>

Already, we sense the disruption in time: the hectic, premature death of the patient, who in the accompanying illustration (Figure 12) is as shocked as the doctor, is followed by the calm of death. The brain passes unceremoniously from use to waste. First, it issues qualities of life – heat, moisture, bleeding – and the narrator wonders if the brain cells were still functioning, still useful? Until it is placed in preservative fluid to prevent rot, a definite inhabitant of waste-time. The corpse, we are told, would be buried the following day in the hospital's cemetery, where of course it would rot.

There is no return to life from the vat of formalin: The alternative is decay. Decay, at least, is some form of life, of time. Formalin, however, all but stops time. In his popular science book *Postcards from the Brain Museum*, Brian Burrell warned the reader of the affective impact of anatomical museums:

It is not so much the sight of it, the shock of the uncanny (as Freud described the contemplation of familiar objects in unnatural settings), as the smell of it. Not the smell of death, as you might expect, or even the reassuring whiff of decay (which at least promises an eventual end to things), but a smell that reneges all hope of oblivion. It is formaldehyde, a fixative so powerful that it does to living cells what **the pause button** on the remote control does to pixels.<sup>73</sup>

Formaldehyde is a biological pause button, Burrell wrote. Except you cannot resume life at the press of a button. As will be made clear below, the initial

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<sup>72</sup> Næsted Nielsen, 17.

<sup>73</sup> Brian Burrell, *Postcards from the Brain Museum: The Improbable Search for Meaning in the Matter of Famous Minds*, 1st ed (New York: Broadway Books, 2004), xi. my emphasis.

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divorcing of the brain from its body is the central concern throughout the novel, and the temporal differentiation between formalin-preserved brain and decaying body will be replayed throughout the book.



**Figure 12**

Illustration from Næsted Nielsen (2010, p. 15)  
Drawing: Christian Guldager.

The skeleton story of the book is as follows: the brothers Ask (11) and Rune (14) spend their autumn holiday with their aunt Oda, whose house is adjacent to a psychiatric hospital and its cemetery. Ask hears a terrible voice calling from the cemetery at night and decides to go investigate. He finds the zombie-ghost of the murdered patient, now a decaying, disintegrated, rotting corpse whose rasping

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voice calls out “where is my brain?”<sup>74</sup> The zombie continues to haunt Ask until the brothers decide to spend their holiday finding and returning the man’s brain. Their quest leads them to the adjacent psychiatric hospital, where Rune sneaks into an unlocked building and in the basement finds the right bucket. The grave is dug up and the bucket placed in the coffin, and the hauntings end.

In this simple tween horror story, the times of living and dying and of material decay are muddled and mingled in more than one instance. First, there is the juxtaposition of the grimy zombie body, “the rotting fleshing hanging from the bones and in several places falling off in flakes, scattering all over the floor”,<sup>75</sup> and the unaltered state of the brain in formalin, its time arrested by the fixative. It is almost as if the brain’s fixation in the still-living moment just after removal is keeping the body of the man alive, in a haunting attempt to right not his wrongful murder, but the displacement of his ‘organ of the self’.

The brainless corpse is sometimes called a zombie, sometimes called a ghost. Interestingly, he shares characteristics with both undead figures. Stories of hauntings usually revolve around some unfinished business. Ghosts come back in order to obtain absolution, vengeance, or some other meaningful closure. To be undead is not to belong to waste-time, is not “lingering, loitering and persisting above and beyond the time of [ones] functionality”.<sup>76</sup> It is, rather, to extend that reach of functionality or purpose even beyond the grave. In a way, ghost stories are comforting tales of incomplete lives given meaning and totality even after a premature death had declared their contingency.

This is true for most undead. One particular kind of haunting seems different: the zombie. At least in its modern, popularised form, removed from its Haitian-African origins, the zombie is characterised by a complete lack of purpose. While mummies, vampires and ghosts all retain some or all of their personal identity, the zombie is decidedly nobody, everybody. It is not surprising that they are almost always represented in hordes. According to Kevin Alexander Boon, absence of self is the defining quality of a zombie: “it is more central to the zombie myth than

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<sup>74</sup> Næsted Nielsen, *De hjerneløses kirkegård*, 43.

<sup>75</sup> Næsted Nielsen, 78.

<sup>76</sup> Viney, *Waste*, 178.

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death, as you can have a zombie who is not actually dead, but you cannot have a zombie that retains its sense of identity”.<sup>77</sup> A zombie is a mere material lingering, whereas for most other forms of undead the important aspect is the spiritual remnant.

In mingling the figures of the zombie (pure body) and the ghost (pure spirit), the reader is left uncertain as to the subjective status of the risen body: does he have a sense of identity, or is he rather searching for one? Is his purpose in alignment with that of his living body, or has he become other in death? His psychiatric diagnosis itself makes the boys wonder: “is having been mentally ill enough to make you a ghost?”<sup>78</sup> The internal logic of the story is that the wrongful murder of the man caused his spirit to linger and attempt to bring brain and body back together. However, the fact that the missing organ is the brain makes the story even more unnerving. It is a special organ, which in Danish culture (and most others) is imbued with both affective and intellectual value. The zombie needs his brain, not for use in this life, but to achieve peace in the afterlife. The brain is both body and spirit, and the quote above clearly indicates the blurriness of the “mental”, which is at once physiological and spiritual. It is in the brain, but also drives the corpse to search for his brain.

The time of nature is also at play in the novel. In their search for the zombie’s brain, the boys encounter two adults: a gravedigger pushing a wheelbarrow full of dirt, and a gardener tending the hospital grounds and cemetery. Both of these characters highlight in their own way the juxtaposition of natural and cultural time. The gravedigger tells them about the psychiatric patients in the cemetery. More importantly, however, his presence reminds the reader of the usual progress of death. When the boys encounter him, he is about to lower a coffin into a grave, and dump the wheelbarrow-full of dirt on top. This reminds the reader of the divine composting act of “dust to dust, earth to earth”. The grave-digger’s job is to

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<sup>77</sup> Kevin Alexander Boon, ‘Ontological Anxiety Made Flesh: The Zombie in Literature, Film and Culture’, in *Monsters and the Monstrous: Myths and Metaphors of Enduring Evil*, ed. Niall Scott (Amsterdam - New York: Rodopi, 2007), 36.

<sup>78</sup> Næsted Nielsen, *De hjerneløses kirkegård*, 61–62.

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facilitate the natural course of nature, in which dead bodies decay and reintegrate with the earth.

The cemetery, embodied in the gravedigger, is a powerful cultural sign. Johan Fjord Jensen, a Danish scholar of literature, visited 1129 cemeteries in 40 different countries, and attempted a grand-scale analysis in his book *Vest for paradis: begravelsespladsernes natur*.<sup>79</sup> The central *motif* in all his analyses is what he calls the “conflict of secularisation”, a concept that tries to capture a characteristic suspension between celebrating and respecting this earthly life, and hoping and praying for a worthy afterlife. He finds this conflict in the practices and objects of cemeteries: The headstones which are built to last for eternity, but which sooner rather than later crumble under the influence of the weather; the cut flowers left on graves, which must be changed often, giving an impression of eternal freshness, or rot and signify negligence on the part of kin. The temporalities of nature and of man are at odds in cemeteries, as “the elements” at once uphold our visions of religious meaning in the world and simultaneously wreak havoc on our attempts at permanence and serenity.

In terms of use- and waste-time, cemeteries seem to share many likenesses with ruins, which William Viney has characterised as engaging “precisely because of an absence they make present, or a presence enlivened by absence” as they “have both withstood time and fallen prey to time’s relentless wearing and wasting, meaning that ruins figure both transience and durability, the entropic dissolution of all material things and the survival of remainders”.<sup>80</sup> This temporal confusion or equivocality is what makes ruins so easy to engage with narratively. They tell of things past and future, without really telling anything. They become a temporally dispersed skeleton which can be fleshed out to suit the narrative needs of the day.

Cemeteries, as opposed to ruins, are built with narrative in mind: religious narratives abound in iconographies, traditions and customs; remembrance is a form of narrative recall of loved ones, and in retelling their stories, the kin left behind are also fashioning their own narratives of personal identity; and finally,

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<sup>79</sup> Johan Fjord Jensen, *Vest for Paradis: Begravelsespladsernes Natur* (Copenhagen: Gyldendal, 2002).

<sup>80</sup> Viney, *Waste*, 129–30.

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the individual headstones carry the most basic form of narrative to the unconcerned passer-by: A name and two dates, and sometimes a specification of the deceased's vocation or a small epitaph. Like the crumbling stones of ruins, these facts are all that is needed to conjure a plethora of possible lived lives, inspiring hope and meaning or dread and despair dependent on the reader, or on the facts on the stone.

Later in the book, the two brothers meet the gardener at the hospital, who tells them the location of the brains: "over there! He said, down in the basement. They are almost all there. I don't think the researchers have begun looking at them, even though it's been many years".<sup>81</sup> In the gardeners understanding, the brains are outside of time. The perpetual business of a gardener tending to live vegetation stands in stark contrast to a basement full of forgotten brains that the researchers have not even begun to look at. The brains, in this passage, are waste: A waste of space, of resources, of human dignity, left over by researchers who have long since moved on. They belong to the anachronic tense of waste-time. This, crucially, is a narrative invention to make the horror story more horrible. In HPI, as shown in previous chapters, research did take place from the beginning and the brains are still actively researched today. Narratively, in order for the vision to be truly terrifying, the brains have to be waste; they must point to the possibility of meaninglessness.

The brainless cemetery holds such sway because it articulates fears about our time. The time of our lives in imagined retrospectivity are meaningful, self-contained totalities, but the fates of the brainless buried psychiatric patients spurs fears of wasted lives and of becoming waste in death. The idea of a subject-become-object for the benefit of brain research might not be so appalling in itself. For a subject, even after death, to become a *waste* object, that is the source of the horror.

#### 4 CONCLUDING REMARKS

The purpose of this chapter was to introduce, unfold, and apply the concepts of value and waste on the Danish brain collection. By way of theoretical introduction

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<sup>81</sup> Næsted Nielsen, *De hjerneløses kirkegård*, 65.

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of the concepts, and three case analyses I have shown that waste, as a temporalized and narrative concept, figures prominently in discussions of value in the brain collection. The chapter has focused more on identifying the use of ‘waste’ as an exclusionary device through which value has been determined. In subsequent chapters (especially chapter 7), my analyses will emphasise the positive construction of value more, as I look at the most recent discussion of the brain collection.

Through my analyses in this chapter, it has become clear, and this is in accord with the framework in chapter 1, that brain collections, and indeed biobanks in general, are constituted by much more than the materials in them. Professor Hugh Perry concluded the Witness Seminar warning that what makes collections truly valuable is the data about the objects, and the *keeping* of data and objects in the same collections, knowing that the data would very soon become meaningless without the objects themselves. In the case of the Danish brain collection, even more poignantly, it became clear that a non-trivial infrastructure around the physical collection is necessary to maintain its usefulness. In line with the general argument of the thesis, I equalled this infrastructure with the experimental systems that others have identified in laboratory sciences, and in the process showed how collection work requires not a small portion of manipulation and ‘tinkering’ in addition to acquisition.

This identification of infrastructure, continuing the interest of chapter 3, led me to propose that the use-time of a collection and that of the materials in it must be considered distinctly. The latter is reliant mainly on storage technologies and the ability to prolong the longevity of the material, as well as on the specific characteristics of the individual objects: what may *this* brain tell us about *x*? The former is determined partly by the usefulness and availability of materials, but just as much, or more, by the conceptual and practical infrastructures that surround the materials, and crucially on the continued development of such infrastructures, a development that must be ‘organically symbiotic’ with the kept material and epistemic things.

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Summarily, in section 2, I showed that the value of what is kept is highly contingent on its use either imagined or realised. One brain-banker used the metaphor of “shelf-life” to denote that ideally brains should only be kept so long, before they were put to use in a research project. I argued that temporality and narrative play key roles in valuation practices, and determining what is waste. Following from Will Viney’s definition of waste as “matter out of time” rather than “out of place”, I argued that value is ascribed through a temporalized narrative constructed around more or less likely uses of the material. I coupled this with Robert Kohler’s distinction between “finders and keepers” in scientific practice to better analyse the very different attitudes to contemporary brain collection, even among brain bankers.

Section 3 shifted the focus from the scientists’ own perspective, to a lay representation of the brain collection, and investigated the use of literary tropes, cultural assumptions, and shock effect to bring out ideas and conceptions of brain collection practices that the professional brain bankers do not engage with. These include anxieties about death and the loss of meaning, as well as grand philosophical distinctions, such as culture/nature, body/spirit. These are not of immediate concern for the people on the ‘receiving end’ of tissue collections, but they demand attention in a broader public discussion. The analysis here has introduced these themes through the dual perspective of waste/value, but in chapter 6 these questions will continue to be central, as I engage with bioethical discussions of the brain collection. Finally, the analysis of *De Hjerneløses Kirkegård* included the jokingly grotesque manner of science and ethics communication that allows teenagers to grapple with these questions in a non-serious manner. While some may think it puts science in an unflattering light, or is too sensationalist to be taken seriously, I think it is important to engage with such popular engagements, especially if we consider scientific practice as cultural practice.



## *Chapter 6*

# The Bioethical Collection

In this chapter, my main aim is to demonstrate changes in bioethical attitudes towards the brain collection. I do this by analysing three public debates in 1991-92, 2005-06, and 2017. A secondary aim is to lay the ground for a deeper account of the emergence of bioethics in Denmark. As the brain collection has featured prominently in foundational debates in the early years of Danish bioethics, it provides a fruitful localised nexus for outlining the principal stances towards bioethical committees, their function, and the scope of their power.

The chapter begins, in section 1, with a brief introduction of the key bioethical actors in Denmark. Then, I delve into the bioethical debates spurred by the brain collection. These debates are treated in five subsections (2.1-2.5), which proceed more or less chronologically from the first debates in 1991 to the most recent in 2017. I will argue from this account that there has been a temporal shift regarding the way the collection is approached bioethically, so that the actions of past actors, which were most pressing in the early discussions, gradually receded and gave way to, first, concerns about the collection's future and, second, concerns about present spending and financial viability.

### **I BIOETHICAL ACTORS**

The collection's institution had been the result of a closed biomedical discussion internal to the psychiatric and neuropathological professions. The practice of collection was never public knowledge, and even hospital staff not directly

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involved with collection practices were largely unaware of their magnitude. Only a small number of people knew exactly what collection meant, and how often it took place. This knowledge certainly never left the professionally closed space of psychiatric hospitals.

By the early 1990's, however, every new research project had to go through external review in committees made up of laypeople, lawyers, ethicists, church representatives &c. This meant, among other things, that controversial research proposals quickly became heated topics of discussion in the public sphere, and that judgement in individual cases was no longer the prerogative of doctors exclusively. This new state of things, beginning in the 1980s, is called bioethics. This has been defined as the involvement of actors outside the biomedical sector in professional medical ethics.<sup>1</sup> Previously, medical professionals had largely dictated their professions' ethical guidelines themselves, but by the late 20<sup>th</sup> century, both political pressure and internal voices from the medical community demanded heightened accountability, and the public credibility that came with it. Below I introduce the three most important actors in bioethical debates about the Danish brains: The Ethical Council, the Research Ethics Committees, and SIND – The National Association for Mental Health.

#### 1.1 The Ethical Council

Following American, British and Swedish reports on novel techniques in genetics and fertility<sup>2</sup> the Danish Interior Ministry commissioned a report on “the ethical problems regarding gene splicing, embryo transfer, artificial fertilisation and prenatal diagnostics”. The report, published 1984, proposed establishing a new

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<sup>1</sup> Duncan Wilson, *The Making of British Bioethics* (Manchester: Manchester University Press, 2014).

<sup>2</sup> Department of Health and Social Security and Mary Warnock, eds., *Report of the Committee of Inquiry into Human Fertilisation and Embryology, Department of Health and Social Security* (London: Her Majesty's Stationery Office, 1984); Gen-etikkommittén and Göte Ekström, *Genteknikens tillämpning på människa: diskussionspromemoria*. (Stockholm: Socialdepartementet, 1984); President's Commission for the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research, *Splicing Life: A Report on the Social and Ethical Issues of Genetic Engineering with Human Beings* (Washington DC: President's Commission for the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research, 1982).

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independent organ devoted to ethical questions in the Danish society.<sup>3</sup> This organ, named The Ethical Council, would keep up with new developments in biotech and continually identify, discuss and publicise possible ethical problems in research and therapy. Law<sup>4</sup> established the council in June 1987 and specified its guiding principles as follows: It must assume that life begins at the moment of conception. It is comprised of 17 members. Eight of these were appointed by the minister for internal affairs on the basis of their publicly documented insight into ethical, cultural, and societal questions relevant to the Council's work. The remaining nine were appointed by a separate committee constituted at the beginning of each political year, to represent the proportion of the political parties in government. These nine members must not be elected politicians. The council must be balanced regarding gender, with a maximum of 9 members of the same gender. Finally, Members would be appointed for a three-year period, with the possibility of one reappointment.

The law also named 4 specific areas to which the Council must attend within its first year: regulations for the use and protection of fertilised human eggs and living embryos, the prospect of genetic manipulations of human reproductive cells, the prospect of prenatal diagnostics, and regulations for cryopreservation of human reproductive cells. For each area, the council had to present a detailed account of the scientific and technological state of the art, a qualified proposal of developments in the foreseeable future, and an evaluation and presentation of possible ethical and judicial dilemmas now and in the future, as well as proposals for solutions to said dilemmas. The Council had purview to advise the pre-existing Research Ethics Committees, the health department, and other public authorities in all ethical matters, especially in relation to medicine, science, and (genetic) data management. Further, the Council was legally required to keep itself updated with regard to new developments in all aforementioned areas, to assume a proactive

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<sup>3</sup> Udvalget om Ethiske Problemer ved Ægtransplantation, Kunstig Befrugtning og Fosterdiagnostik, *Fremskridtets Pris: Rapport* (Kbh.: Indenrigsministeriet, 1984).

<sup>4</sup> 'Lov Om Oprettelse Af et Etisk Råd Og Regulering Af Visse Biomedicinske Forsøg', Pub. L. No. LOV nr 353 (1987), <https://www.retsinformation.dk/Forms/R0710.aspx?id=46927>.

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role in treating ethical dilemmas and questions as they arose, and to further public discussion of said questions when they felt it necessary.

In the most recent edition of the law regarding the ethical council from 2004, its areas of responsibility have been broadened to include “nature and the environment”, beyond “the respectful treatment of humans as well as the integrity and dignity of future generations”.<sup>5</sup> Accordingly, only four of its members are appointed by the minister for health and internal affairs today. The remaining four are appointed, respectively, by the minister for the environment, the minister for food and agriculture, the minister for science, technology, and innovation, and the minister of finance and business. Neither in the original law, or in any of the later editions is mentioned the need for medical professionals in the Council – a requirement in the research committees established some years earlier.

#### 1.2 The Research Ethics Committees

Before the Ethical Council was established, Denmark had followed the international trend and formed internal research ethics committees. Such committees had already been established in the 1950s and 1960s in the UK,<sup>6</sup> Sweden,<sup>7</sup> and the Netherlands.<sup>8</sup> In 1979, responding to the Helsinki Declaration II from 1975, Denmark followed suit, and by 1982 a national committee system had been established.<sup>9</sup> A central committee for ethical research conduct and seven regional subcommittees were to ensure that all biomedical research in Denmark was carried out in accordance with the Helsinki Declaration and followed commonly accepted ethics. The subcommittees were constituted of 6-10 members, of which half were biomedical professionals (doctors, dentists, pharmacists)

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<sup>5</sup> ‘Lov Om Det Ethiske Råd’, Pub. L. No. LOV nr 440 (2004), <https://www.retsinformation.dk/Forms/R0710.aspx?id=9909>.

<sup>6</sup> Adam Hedgecoe, ‘“A Form of Practical Machinery”: The Origins of Research Ethics Committees in the UK, 1967–1972’, *Medical History* 53, no. 3 (July 2009): 331–50, <https://doi.org/10.1017/S0025727300000211>.

<sup>7</sup> Helena Tinnerholm Ljungberg, ‘Regulating Research: The Origins and Development of Research Ethics Committees in Sweden’ (Research Ethics Committees: Origins, development, and beyond, Uppsala University, Sweden, 2019).

<sup>8</sup> Noortje Jacobs, ‘Ethics by Committee: Governing Human Experimentation in the Netherlands, 1945–2000’ (PhD, Maastricht university, 2018), <https://doi.org/10.26481/dis.20180620jn>.

<sup>9</sup> Udvalget om Ethiske Problemer ved Ægtransplantation, Kunstig Befrugning og Fosterdiagnostik, *Fremskridtets Pris: Rapport*, chap. 5.

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appointed by the state or by research institutions, while the other half were laypeople appointed by the regional councils. The central committee consisted of two members (one layperson and one medical professional) from each regional committee, as well as a chair and vice chair appointed by the State council for medical research. The main principle behind the parity of lay and professional representation was that researchers and non-scientists have “equal entitlement and competences to value the ethical aspects of a given project”.<sup>10</sup> The inclusion of lay members was generally welcomed, though one commentator was wary that the laypersons would probably be politicians, and not “ordinary people”, and that their personal ethical concerns would be too easily swept aside by the jargon of the doctors.<sup>11</sup> The purpose of the committees was to safeguard the public against unethical practice, but in contrast to the Ethical Council their deliberations were not meant to be public. As will be clear, an exception was made in the case of the brain collection.

### 1.3 SIND

SIND – The National Association for Mental Health was established in 1960, and membership then as now is open to all. According to its website, the NGO “works to bridge the gap between daily experiences in the psychiatric services, and the information available for decision-making civil servants and politicians. SIND also offers help to individual families and disease sufferers in urgent hardship.”<sup>12</sup> Their services include dialogue, education, legal aid, facilitating human contact, as well as disseminating information to the wider public, and political lobbying for rights and welfare for sufferers of mental illness. SIND is contacted by governing bodies whenever they consider decisions that will affect the psychiatric services available through public healthcare. They are often consulted before new laws are proposed, and their influence is noticeable both in the broad public and in local political arenas.

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<sup>10</sup> Udvalget om Ethiske Problemer ved Ægtransplantation, Kunstig Befrugtning og Fosterdiagnostik, 80.

<sup>11</sup> Peter Rossel, *Medicinsk etik: en idéhistorisk analyse af lægestandens dokumenter* (København: Gad, 1979).

<sup>12</sup> ‘Om SIND | SIND’, accessed 3 October 2019, <https://sind.dk/om-sind>.

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### 2 BIOETHICAL DEBATES

In this section, I trace the public debates about the ethical status of the brain collection. I first describe the initial reactions to the collection's becoming public knowledge in 1991, and some of the ensuing foundational debates about the interplay between science and ethics. In the following subsection (2.2), I identify a strategy of synecdoche on the part of the actors involved, as they all tried mobilising the collection for their respective purposes. Following this (in 2.3), I narrow my focus on one actor in particular, Raben Rosenberg, who until 2015 was head of Translational Neuropsychiatry Unit (TNU), the department that housed the collection. Rosenberg has been deeply engaged in all the debates here analysed. He is especially interesting because he has written several books on ethics and psychiatry, from which I unfurl his ethical principles before returning to the brain collection. I then (2.4) proceed to 2005-06, when a new research project received funding to reactivate the collection, thus spurring new public interest. Finally, in section 2.5, I account for the most recent debate in 2017, which almost resulted in the collection's destruction, but ended with its moving to Odense.

#### 2.1 1991: Initial reactions

Knowledge of the brain collection became public 20 June 1991. SIND had sent a letter to the health minister, demanding a statement regarding the brain collection.<sup>13</sup> The health minister, Ester Larsen, forwarded the request to the relatively new advisory body, the Danish Ethical Council, as well as the National Research Ethics Committee (REC). This initial event prompted only a short notice in the press, which reported mainly on the stance of the psychiatric researchers who stated, "We must be allowed to employ material collected under circumstances we would not accept today if indeed this can possibly provide us with a broader understanding of diseases that cause immense suffering".<sup>14</sup>

Days later, the newspaper *Berlingske Tidende* could report the conclusion of the REC. They argued for the retention of the brain collection with the caveat that

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<sup>13</sup>I have not been able to find this letter in the archives, so here I rely on press coverage.

<sup>14</sup>Ritzaus bureau, 'Etisk Råd Skal Tage Stilling Til Hjernesamling På Hospital', *Ritzaus*, 19 June 1991.

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next of kin should have the opportunity to contact the collection manager and request a brain removed. The chair said the committee's decision rested on two decisive points: First that the brains were collected lawfully, because "no guidelines for proper practice or oversight were in place at the time of collection", and second that all future research would require pre-approval by the RECs. This, to him, made future use of the collection ethically sound.<sup>15</sup>

SIND responded to the national REC's statement with another letter demanding the brains buried. They decried the collection as "a monument to the power held by biological psychiatry over its patients", and raised the question of "who owns the patient's body?" They also expressed concern that continued research on the brains would lead to stigma towards the sufferers of psychiatric illness. Finally, they questioned the epistemological claim that it would be possible to identify direct correlation between structural features of the brain and psychiatric illness.<sup>16</sup>

By the end of the year the Ethical Council had finished its deliberations, agreeing neither with SIND nor with the REC. The latter had recently approved a research project into the heredity of Alzheimer's disease to investigate 326 tissue samples from the collection in Risskov.<sup>17</sup> The problem for the Ethical Council was not, as for SIND, the use of these brains samples - the council supported retention and scientific use. The council took issue, however, with part of the Alzheimer's project, namely that researchers would identify living at-risk families from the material - descendants of the unwitting donors - and request their participation in the study, in order better to follow the inherited features.

To approach descendants of Alzheimer's sufferers unexpectedly, the Ethical Council argued, was an indefensible breach of privacy and the requirement of informed consent. Given the age of the specimens in the collection, the descendants would likely be at least two generations removed from the original sufferers, and considering the stigma and taboo surrounding psychiatric illness at

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<sup>15</sup> RB, 'Komite: Bevar Hjerne- samling', *Berlingske Tidende*, 22 June 1991, sec. sektion 1.

<sup>16</sup> 'Krav Om at Gamle Hjerner Begraves', *Politiken*, 5 August 1991, sec. sektion 1.

<sup>17</sup> Henning Ziebe, 'Langsomt Ind i Mørket', *Berlingske Tidende*, 11 September 1991, sec. Sektion 3, magasin.

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the time of collection, might be unaware of any Alzheimer's in the family. To inform them suddenly that their ancestors' brains had been kept and researched for several decades, and that they themselves lived with a heightened risk of inherited dementia, this constituted a severe breach of patients' right to decide how much they want to know. Some people might prefer to live oblivious to hereditary disease risk, and could be shocked and traumatised to receive this information without explicitly asking for it.<sup>18</sup>

The Ethical Council was generally for research on the brains in the collection, but only so long as it did not involve the lineal descendants in any way. They proposed instead mobilising the mass media to inform the public about the collection, and about the possibility of removal of specimens if so wished by next of kin. This indirect approach would mean that only those descendants who actively pursued information would get it, and that those who wished to object to continued use of material could do so.<sup>19</sup>

In response to this statement, *Politiken* quoted Raben Rosenberg, PhD DMSc, professor at the psychiatric hospital and, until recently, director of the brain collection. He would figure prominently in the debates to come as a hard line supporter of the autonomy of science. He called the Ethical Council "too restrictive" because it potentially hindered cross-generational studies with living participants.<sup>20</sup>

These initial responses to the brain bank's entry into the public sphere demonstrate the key issues in the bioethical discussions of the time: Paternalism, self-determination, scientific autonomy, and information. SIND was worried the biomedical sector would have the paternalistic power to make life-altering decisions for the patients, without consulting them or their next of kin, and worked to safeguard the individual rights to informed consent and ownership of the physical body. Rosenberg, on the other hand, was worried that such rights,

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<sup>18</sup> 'Uenighed Om Hjerner Fra Sindslidende', *Politiken*, 19 December 1991, sec. Sektion 1.

<sup>19</sup> Det Ethiske Råd, 'Bilag 6: Det Ethiske Råds udtalelse om depotet af hjernepreparater på Psykiatrisk Hospital i Århus', in *Public discussion about bioethics: activities initiated by The Danish Council of Ethics*, Annual report of The Danish Council of Ethics 4 (Copenhagen: The Council of Ethics, 1992), 86–95.

<sup>20</sup> 'Uenighed Om Hjerner Fra Sindslidende'.

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when upheld without caveat, would restrict science and ultimately hinder therapeutic progress, and the ethical council was mostly concerned with the possible trauma that comes with sudden unwanted exposure to existential questions which might radically alter the life course of an individual.

## 2.2 The Danish brains mobilised as a synecdoche

After becoming public knowledge, the brain collection was not only discussed in its own right, but would soon feature as an exemplar case in more broadly bioethical debates. One such debate began with an interview with Rosenberg in *Politiken*, a national newspaper, in January 1992.<sup>21</sup> In it, Rosenberg began with the statement “mental illness is caused by chemical failures in the brain”, and argued that biological medicine is simply “better than talk”. He was fashioned in the article as a clear-sighted visionary who for years had been vilified as part of a “powerful, biomedically fixated elite unwilling to recognise mental illness as the sensitive soul’s healthy reaction to a sick world”.

However, with the 1990’s being hailed as the decade of the brain, he felt he could finally speak his mind: “A few years ago, people were too wussy to say these things aloud. These years brain research is growing explosively, and now we can say it as it is.” He rejoiced in the promises made by brain scanners and genetic technologies to develop “more precise types of medicine” and by identifying genetic markers, catching diseases earlier and take more preventive steps. This, he argued, was far preferable to psychotherapeutic methods. He acknowledged their efficacy in “personal crises, for example following divorce”, but contended that “it is deeply injuring and brutish to tell the mother of a young schizophrenic man who is always on about his ears and nose falling off, that it is her fault and the boy is reacting to a lack of love in the childhood.”

Research on the brain collection was presented by Rosenberg as one of the promising new investigations of the brain decade. The article mentioned the “ethical dispute”, but the collection primarily functioned as an example of the promises of genetic research: “Once we know these genes [underlying

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<sup>21</sup> ‘Medicin Er Bedre End Snak’, *Politiken*, 5 January 1992, sec. PS.

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Alzheimer's], we can begin uncovering what triggers psychiatric suffering in those with the hereditary predisposition. It may be birth trauma such as lack of oxygen, it may be environmental factors such as nutrition, addiction, vira, or upbringing. Or it may be a combination". According to Rosenberg, the key to solving these questions was extensive genetic research on as broad a material as possible.

4 days later the secretary of SIND, Peter Storm, wrote a critical reply to Rosenberg. He began his piece thus: "One must assume that psychiatry professor Raben Rosenberg believes his own theory that mental illness is due to genetic errors that result in pathologies of the brain. It is, to my mind, a very simplistic and scientifically unfounded claim".<sup>22</sup> Storm called it a "huge ethical and democratic problem" that "psychiatric researchers in closed milieus produce inventions that are then, at a very late stage, presented to the public as inevitable facts - as trains that have already left the station." Storm gave two examples of this tendency: the use of frontal lobotomy, and the "collection of almost 10.000 brains from psychiatric patients without asking permission."

Storm's article was published along with two other replies to Rosenberg.<sup>23</sup> All three claimed that psychiatry as a profession was stuck in a paternalistic, closed-system understanding of professional ethics, and that it was time for a broader public to be involved in the discussion. Storm concluded: "We must demand democratic research councils and independent, critical institutions to evaluate and control psychiatric research. We must have an ongoing, open debate about the priorities in research."

Peter Storm's rhetoric clearly shows an attempt to make the brain collection a short-hand for bad practice in science, and enrol it as an argument-by-example in the broader discussion about bioethics and the scope of different actors' purview in medical ethical questions. The example of frontal lobotomy was a strong one, but might be (and was) dismissed as a folly of the past, when researchers did not know better. The brain collection, however, was still fresh in public memory, and

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<sup>22</sup> Peter Storm, 'Lyt Og Lær', *Politiken*, 9 January 1992, sec. 2. sektion.

<sup>23</sup> Birgitte Brun, 'Kunsten at Hjælpe Det Syge Sind', *Politiken*, 9 January 1992, sec. Sektion 2;

Birgitte Husmark, 'Glemt Er al Viden Om et Samspil', *Politiken*, 9 January 1992, sec. Sektion 2.

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it would provide a strong argument if successfully made into a synecdoche for ruthless, calculating science.

Storm was not the only actor trying to enrol the collection in the bioethical debate. It would prove a recurrent strategy for both parties in the debate, to try and present the brain collection as an obvious example of their side's veracity. Later that year, 1992, Rosenberg attempted just such a mobilisation of the collection. In response to a recent statement by the Ethical Council regarding consent and the protection of sensitive information, he expressed "a certain doubt regarding the value of informed consent".<sup>24</sup> Rosenberg stressed the importance of statistically reliable knowledge in modern medical research, for example in clinical trials, and of thorough registers of the population for broad correlation studies, which could uncover new perspectives on psychiatric disorders among other things. Finally, he turned to biobanking:

Collected tissues represent a substantial value, but the full value of such banks typically becomes evident only years after collection. Many diseases are expressed only after a latency period, and new advances in research and technology can make a biobank especially valuable. It is worth noting that recent developments in molecular genetics allows investigating hereditary factors in Alzheimer's disease on the basis of the brain collection in Risskov. New cell counting techniques have also found application on tissues in the collection and has contributed new knowledge about schizophrenia. Just a few years ago, this would have been considered very unlikely. I am surprised, therefore, by the Ethical Council's consideration of allowing only very short retention of tissues in biobanks.<sup>25</sup>

Rather than denying or silencing the non-consensual collection practice that Storm opposed so fervently, Rosenberg made it his strongest claim that such autonomy in research generated invaluable data, which could never have been realised under the stricter regulations of the 1990s.

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<sup>24</sup> Raben Rosenberg, 'Etikkens pyrrhusejrer over videnskaben?', *Berlingske Tidende*, 17 September 1992, sec. Sektion 2.

<sup>25</sup> Rosenberg.

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Peter Storm's reply instantly attempted to recapture the collection as a clear breach of individual rights to self-determination, and an abuse of power on the side of the psychiatrists:

Personally I gained interest in these issues when, through the organisation SIND, I raised questions concerning the psychiatric brain depot in Risskov. Here, in the years 1947-1980 [sic], was collected some 10.000 brains from psychiatric patients across the country. Consent was not asked (rather researchers took advantage of weak legislation). Neither did they ask permission to store the brains; they wrote themselves a blank check for future research. Sadly, this is how it was done back then. And from this we must learn, so we can prevent such violations in the future.<sup>26</sup>

The first sentence is revealing, as it highlights Storm's wish to portray the collection as a self-evident example of immoral scientific practice. He suggests here that it was the brain collection that opened up to him the entire field of bioethics, and made him gain interest in broader issues of consent, protection of patients' rights, and non-medical supervision of the field.

The very same feature of open-ended futurity<sup>27</sup> that Rosenberg made the crux of his argument, was here made obviously reprehensible. The metaphor of a blank check to be cashed in the future with unknown, perhaps devastating consequences relies on the public's outrage by invoking tax evasion, irresponsibility, and disloyalty towards society.

Only a year after becoming public knowledge, the collection was mobilised by both sides in an extensive bioethical debate. Rosenberg congratulated his predecessors for their contribution to the bank of knowledge, in epistemological terms, while Storm condemned the same actions as a clear violation of ethical principles. Both Storm and Rosenberg simply assumed the readers' agreement that the bank was either good or bad, and attempted to make it a synecdoche for much more foundational convictions.

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<sup>26</sup> Peter Storm, 'Individets selvbestemmelse eller videnskabelige fremskridt?', *Berlingske Tidende*, 15 October 1992, sec. 2.

<sup>27</sup> Daston, 'Introduction'; Daston, 'Epilogue: The Time of the Archive'.

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### 2.3 Rosenberg's ethics

Here, I would like to provide a closer look at Rosenberg's ethical position. A medical doctor since 1983, and psychiatrist 1987, he became the chief physician at Risskov in 1991. Beside his scientific output, Rosenberg has written several textbooks for students of medicine and psychiatry, as well as more popular accounts of the history and philosophy of psychiatry. While his statements in the press give an indication of his position on the brain collection, a closer look at these more extensive writings will provide a better understanding of his reasoning. This will serve to highlight the differences between internal, professional ethics, and external lay ethics-perspectives, and the different values practiced in each.

In the following, I summarise Rosenberg's position from his 2016 book *Psykiatriens Grundlag* [The Foundations of Psychiatry], which is a comprehensive tome of history, philosophy, and scientific theory.<sup>28</sup> Though written much later than the discussions of the brain collection, his position in this book is not different from that in earlier books,<sup>29</sup> only more expressly formulated.

The first thing to note is that Rosenberg overall is concerned with medical ethics as doctors' ethics towards patients. His focus is on treatment, therapy, and providing the best possible care. His ethical review centres on the meaning of the Hippocratic Oath, as well as the largest normative ethical philosophies, and what they mean for doctor-patient interaction. Besides this, he spends much time on the value of evidence-based practice, arguing that the best safeguard towards bad treatment is well-documented experimental knowledge obtained via RCTs (randomised controlled trials) and meta-reviews. His historical overview is balanced, as he maintains on the one hand that we must not judge past actors by modern standards: More often than not, he argues, doctors have acted from the wish to help above all other concerns. Sometimes this has led to genuinely helpful

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<sup>28</sup> Raben Rosenberg, *Psykiatriens Grundlag: Historie, Filosofi Og Videnskab* (Aarhus: Aarhus Universitetsforlag, 2016).

<sup>29</sup> Henrik R Wulff, Stig Andur Pedersen, and Raben Rosenberg, *Medicinsk filosofi* (Kbh.: Munksgaard, 2007); Per Jørgensen, Jan Mainz, and Raben Rosenberg, *Psykiatri: forskning, teknologivurdering og kvalitetsudvikling* (København: Munksgaard Danmark, 2002); Henrik Ramsing Wulff, Stig Andur Pedersen, and Raben Rosenberg, *Philosophy of Medicine: An Introduction*, First publ. 1986. Repr (Oxford u.a: Blackwell, 1988).

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innovations (he mentions electroconvulsive therapy and psychopharmaca, both of which were introduced without RCTs, but with their efficacy subsequently confirmed by this method), at other times to harmful and even cruel techniques, such as lobotomy and sterilisation. On the other hand, modern psychiatrists should take advantage of all the knowledge at hand. It is their duty, Rosenberg urges, to pursue continual postgraduate studies to keep up to date; never to disregard scientific evidence; and to proceed with the utmost caution when attempting therapy where no evidence-based knowledge is yet available.<sup>30</sup> Evidence-based patient care is at the centre of Rosenberg's psychiatric ethics, and with this in mind, we begin to get a better understanding of his position against Storm.

Further clues are gained from the subsection of Rosenberg's ethics chapter called "policy – medical ethics in society".<sup>31</sup> Here, he discusses the prevalence of "values" in psychiatric policymaking, and documents such as declarations of intent, guidelines, political party programmes, and so forth. Rosenberg argues that these vaguely formulated values "must undergo systematic philosophical analyses in order to command respect and bind actions. As accepted political-administrative values, they are too loosely defined to achieve the character of an ethics of duty within the concrete clinical everyday".<sup>32</sup> If this does not happen, Rosenberg fears a shift will occur away from professionalism towards administrative governance. In other words, the medical personnel must continually engage in considerations of ethical principles, lest ethical behaviour be reduced to following externally formulated guidelines. In this light, his stance towards the brain collection gains another dimension. As someone devoted to philosophically principled ethics rather than discussions of personal or interpersonal values, he cannot take seriously what he considers to be mere chock-value and personal outrage from his opposition. As evident from the January 1992 interview, Rosenberg fashioned himself as the voice of reason and science, against an unreasonable and ill-founded critique of biological neuroscience.

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<sup>30</sup> Rosenberg, *Psykiatriens Grundlag*.

<sup>31</sup> Rosenberg, 509ff.

<sup>32</sup> Rosenberg, 511.

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In short, Rosenberg argues that ethical behaviour in psychiatry equals scientifically sound behaviour. Treating patients requires knowledge from well-conducted studies; lack of research, especially biological research, means sub-standard and therefore unethical treatment. At no point in his discussion of medical ethics does he devalue personal autonomy, or defend paternalism in any way. However, when it comes to the collected brains, the question of autonomy and consent have long passed. To him, the Danish brains constitute valuable research material; the unethical thing would be to prevent research that might help living or future patients.

### 2.4 The 2000's: Use-value and novelty

After the initial debates in 1991-92, the brain collection lived a relatively quiet life. A few studies were published, but overall, the collection was forgotten and left to itself in remote basement rooms until 2005, when the initiation of a new research project reignited public interest in the collection. In April 2005, Danish media reported the news that Karl-Anton Dorph-Petersen from the University of Pittsburgh was to lead an extensive evaluation of the research potentials of the brain collection.<sup>33</sup> Novel imaging techniques, the newspapers reported, had driven psychiatric research (especially into depression) to new heights the preceding decade, but classical microscopy was a crucial addition in order to progress further. Rosenberg explained that “a range of studies are only possible when looking at brain tissue in great detail, which is not possible when working with scanning images of living brains”.<sup>34</sup>

In May, the local newspaper Århus Stiftstidende brought a double feature on a woman who, prompted by the notice from April, wanted to know if her sister's brain was held in the collection. She found “grotesque” the notion that a family “heirloom” (her sister's brain) might have been preserved without her knowledge.<sup>35</sup> However, her inquiry was from curiosity only. As she put it, “I cannot exactly be

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<sup>33</sup> Ritzau, ‘Unik dansk hjernesamling kan give ny viden om depression’, *Ritzaus Bureau*, 7 April 2005; Ritzaus Bureau, ‘Hjerner kan give ny viden om depression’, *Berlingske Tidende*, 8 April 2005, sec. 1. These articles were also cited in chapter 5.

<sup>34</sup> Ritzau, ‘Unik dansk hjernesamling kan give ny viden om depression’.

<sup>35</sup> Hansen, ‘Pårørende kræver besked om hjerner’.

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sad or angry if her brain might help other people”.<sup>36</sup> Rosenberg, as the representative for the collection, said he was more than willing to provide information to any living relatives with kin in the collection. However, he said, this was the first time they had been approached with such a question since the collection became public knowledge. Finally, he said that if someone expressed a wish to stop research on the brain of a relative, the hospital would most likely acquiesce.<sup>37</sup>

The first debates in 1991-92 had revolved around the scope of the ethical council and the autonomy of psychiatry with the brain collection performing only a metonymical role, standing in either for all kinds of horrors, or for unimagined progress. This new debate would place the brain collection in its centre. With a concrete research project on the horizon, the question was no longer simply “should we keep these brains” but rather “should we *use* them?” The episode with the woman seeking knowledge of her deceased sister’s brain would prove to foreshadow a development throughout the ‘00s, where feelings of the “grotesqueness” of the collection would be pursued increasingly separate from the more determining question of their use-value, and whether they might “help other people”.

Also in the question of use-value, Rosenberg was at the forefront of the debate, this time praising the (unwitting) visionary foresight of Einarson, Lorentzen, and colleagues, and reminding readers that the collection was unique:

In popular terms, they hadn’t a clue what to do with the brains. Their collection and registration took place simply with the expectation that the future would provide new possibilities for extracting knowledge from the brains. [...] The fact that the collection practices today would be subject to ethical reproach, is beside the point. Today, a similar collection would not be possible.<sup>38</sup>

Rosenberg did not initially meet much opposition. In an article from *Samvirke*, Frede Budolfsen the chair of SIND stated “I am a bit ambivalent about this

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<sup>36</sup> Hansen, ‘Sygehus vil gerne give pårørende vished’.

<sup>37</sup> Hansen.

<sup>38</sup> Jan Sisseck, ‘10.000 Hjernner På Lager’, *Samvirke Nr. 10*, October 2005.

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question. From a purely ethical standpoint I still think the brains ought to be buried. However, in SIND we also wish to support all research that might help make life more agreeable to sufferers of mental illness”.<sup>39</sup> Budolfson, it seemed, was willing to let the ends, namely further insight into the basics of depression, justify the means – an unethically obtained brain collection. The head of next-of-kin-relations within SIND, Sanne Lauridsen went further yet, calling it unethical to discard the brains: “I have previously stated that it would be OK to use the brains, but not without obtaining explicit consent from next of kin. At that time I thought, wrongly, that the brains were illegally collected. According to the laws at the time, that is not the case, so now I think it would be catastrophic not to use them for research. Incidentally I am an organ donor myself”.<sup>40</sup> SIND would not stand in the way of using the collection. However, going forward would not prove frictionless.

On December 8 2005 a story broke across Danish media that a political majority demanded consent from next of kin before allowing new research. “Originally, no consent was given either for removal of the brains or their use in research. We now demand therefore that next of kin be contacted and asked for permission”, said Helle Sjelle from the conservative party.<sup>41</sup> She was backed up by health spokespersons from the Danish people’s party, the social democrats, and the liberal democrats. Only Preben Rudiengaard from the liberal party Venstre offered a differing opinion: “Whether or not it was justified to not inform anyone of the brains back then, it would be entirely wrong to contact next of kin now. These are people who have already gone through the hardships of losing a loved one under very difficult circumstances, and it is not our place to make them relive those memories”.<sup>42</sup> His position was akin to the 1991 proposal from the Ethical Council, in which they deemed it unkind to cold call people regarding long dead relatives and possible genetic diseases. The 2005 head of the Ethical council, Ole Hartling, however, expressed his assent with the political majority: “We have to bite the bullet now. Otherwise the clandestine parts of this case will continue to cause

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<sup>39</sup> Sisseck, 52.

<sup>40</sup> Sisseck, 52.

<sup>41</sup> Leif Kiil Sørensen, ‘Politisk krav om samtykke før der forskes i gamle hjerner’, *Kristeligt Dagblad*, 8 December 2005.

<sup>42</sup> Sørensen.

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problems, and the malpractice cases will multiply”.<sup>43</sup> All interviewed parties expressed their support for the research itself, hoping it would lead to new insights regarding terrible mental conditions.

Inside that day’s paper paper, *Kristeligt Dagblad* brought two background articles, extending coverage of the story from the front page. The first reported from a visit to a hospital chapel and cemetery in Nykøbing Sjælland. Written to shock and terrify the reader, it began with a highly evocative description of the idyllic grounds, “but the idyll hides a horrific story, the story of a macabre violation of unoffending sufferers of mental illness”.<sup>44</sup> In the hind room of the chapel, colloquially known as “the abattoir”, “the white tiles on the walls and the large drains in the floor bear witness of the operations performed in the so-called dissection room. Kin to the deceased patients were not informed of these operations, which always ended with old newspapers being stuffed into the patients’ heads”.<sup>45</sup> The article proceeded to describe the non-ceremonious transport of the brains, the neglected gravesites, and finally informed readers that around 1900 brains overall had been collected from the hospital in Nykøbing.

The other article was an extensive interview with Rosenberg describing the new “battle against depression by way of research”. In this battle, the brain collection was to play an important role: “The hope for the collection was that it would in the future help to solve the most pressing problems for sufferers of psychiatric diseases. [...] We still have the original journals and can make modern diagnoses. It is very interesting to uncover how the hippocampus looks in these people. The collection is topical again because it can supplement the newest research with knowledge at the level of cells”.<sup>46</sup> The basic argument was that new scanning techniques were not enough to answer fundamental questions and that research on real tissue was necessary.

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<sup>43</sup> Sørensen.

<sup>44</sup> Leif Kiil Sørensen, ‘Idyllen i skyggen af mørket’, *Kristeligt Dagblad*, 8 December 2005.

<sup>45</sup> Sørensen.

<sup>46</sup> Leif Kiil Sørensen, ‘Til kamp mod depression gennem hjerneforskning’, *Kristeligt Dagblad*, 8 December 2005.

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Regarding the question of consent, Rosenberg dismissed any claim to limit the extent of research. “My task as its leader is to say that when we have such a collection, we must try and maximise our gains from it research-wise. That is our fundamental ethical obligation. If this new project can contribute towards solving a severe problem, it would be unethical not to make use of the collection”, he said, turning the ethical dilemma on its head. He and his colleagues, he assured, would show the utmost respect and heed all wishes from next of kin, but in the preceding 15 years, only a single person had called in to enquire about a deceased relative, and that person’s brain was not even in the collection. Finally, he said, “seen with past eyes, nothing was done that was considered wrong or against common practice. I for one am against holding the past to present standards”.<sup>47</sup>

The following days the story unfurled as bishops, journalists, and politicians proclaimed their positions, and finally the Ethical Council was mobilized to arbitrate on the issue. *Kristeligt Dagblad* ran an editorial on December 9, saying on the one hand that it was not fair to “judge the past by today’s ethical standards”, thus agreeing with Rosenberg that it would be unethical not to use the collection. On the other hand, they wrote, if a solution addressing contemporary ethical demands could not be found, it would send the undesirable signal that the medical community did not take ethical standards seriously. They concluded with the hope that most if not all remaining next of kin would give their consent to using the brains in research.<sup>48</sup>

Religious voices also joined in, as the bishops of the Danish national church backed the political demand for consent. Five bishops proclaimed their support, echoing previous statements from politicians that consent was necessary to remedy the transgressions of the past, which they all objected to, all the while welcoming the prospect of new research on depression.<sup>49</sup>

Jyllands-Posten, on the same day, ran a very different perspective on the story. Their article attributed all ethical concerns to conservative health spokesperson

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<sup>47</sup> Sørensen.

<sup>48</sup> KD, ‘Leder: Hjerneforskning’, *Kristeligt Dagblad*, 9 December 2005.

<sup>49</sup> Leif Kiil Sørensen, ‘Biskopper støtter etiske krav til hjernesamling’, *Kristeligt Dagblad*, 9 December 2005.

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Helle Sjelle, and in a stroke dismissed them by letting Rosenberg state, “all necessary permits are sorted” with reference to the 1991 ruling of the national REC, which had deemed the brain collection worthy of preservation, but which had also been ambivalent about initiating new research without consent. The article concluded by highlighting the collection’s status as “highly valuable research material”.<sup>50</sup>

The 2005-debate concluded 7 March 2006 with a statement from the Ethical Council<sup>51</sup> who had been asked to deliberate on the subject by the minister for health and the interior. Their conclusions were very similar to those from 1991. They opposed contacting living relatives for consent (with reference to the right not to know); they allowed research on the collection, given that all proposals were submitted to RECs for approval; and they noted that the collection had been established in accordance with the laws and ethical standards of the time, even if these were different from modern sentiments. However, they spent considerably more time on the current research perspectives than they had in 1991, and they stressed the collection’s potential to “heighten the life quality of living people”, and added that “the collection’s value is continually increased” as a result of new knowledge and new investigative techniques.<sup>52</sup> Additionally, they judged it very unlikely that a similar collection could be established today, which further heightened the value of the collection. The national REC also issued a statement overall agreeing with the Ethical Council, including the problem of contacting next of kin.<sup>53</sup>

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<sup>50</sup> Jens Kurt Jørgensen, ‘Politisk blæst om hjernesamling i Risskov’, *Jyllands-Posten*, 9 December 2005.

<sup>51</sup> Det Etske Råd, ‘Høringssvar, 7. Marts 2006’, March 2006, <http://perma.cc/7DUJ-WXMC>.

<sup>52</sup> Det Etske Råd, 2.

<sup>53</sup> Den Centrale Videnskabsetiske Komité, ‘Vedr. Forskning På Hjerner Fra Hjernesamlingen På Psykiatrisk Hospital i Risskov, Århus’, 22 February 2006, <http://cvk.sum.dk/omDNVK/~media/Files/cvk/omDNVK/Hoeringssvar/EpostsvarSUM.ashx>; Den Centrale Videnskabsetiske Komité, ‘Hjernesamlingen på Psykiatrisk Hospital, Risskov’, in *Årsberetning 2006*, Annual report of The Danish National Committee on Biomedical Research Ethics (Copenhagen: The Danish National Committee on Biomedical Research Ethics, 2007), 22, <http://www.dnvk.dk/publikationer/~media/Files/cvk/publikationer/aarsberetninger/CVK%20orsberetning%20til%20web%202006.ashx>.

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In the following decade, the brain collection made continuous brief appearances in the press. Some articles were meant to communicate the science coming from use of the brains. These usually followed the publication of scientific results and relied on interviews with experts and staff.<sup>54</sup> Others followed the trope of the “brainless cemetery” and used the brain collection to give a “strange” and “mystical” lustre to filler-stuff in the news’ silly season.<sup>55</sup> Such articles underplayed the scientific value of the collection, opting instead to present it as a curiosity. One even let its readers know that “the brains were collected in Risskov where there is still 10.000 brains, which no one knows what to do with”.<sup>56</sup> The “brainless cemetery” entered the realm of fiction, as we saw in chapter 5, with a tween zombie-novel from 2010.<sup>57</sup> It even made it into school curricula, when a periodical for religion-teachers brought an example teaching case based on the “brainless cemetery”.<sup>58</sup>

In the second half of the 2000s, the brain collection became a truly public phenomenon: the subject of political and religious debates, of fiction, and its example even used in school teaching. The case was no longer confined to the past, or simply a segue to principle discussions, but became a question of what (or, rather, how much) to do and, especially, how to secure its future use. Past transgressions were largely dismissed to consciously exaggerated fictions, or else discussions were about how to get past them for the sake of the collections’ future viability.

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<sup>54</sup> ‘Gamle hjerner skal opklare depression’, *DR.dk*, 18 February 2009; ‘Gemte hjerner skal løse en gåde’, *Stiften.dk (Århus Stiftstidende)*, 18 February 2009; Jørgen Bollerup Hansen, ‘Hjerner kan give svar på depression’, *Århus Stiftstidende*, 19 February 2009; Steffen Bang Nielsen, ‘Raben Rosenberg’, *Dagens Medicin*, 20 February 2009, sec. 1; ‘Ny viden om depression fra gamle hjerner’, *Midt i psykiatrien - regionspsykiatriens blad*, February 2009; Line Randrup, ‘Den smukke, komplekse hjerne’, [8240] - *Magasinet for beboere og erhvervsdrivende i 8240-området*, November 2008.

<sup>55</sup> Judith Betak, ‘Det mærkelige og mystiske Danmark’, *Århus Onsdag*, 25 June 2008; ‘De hjerneløses kirkegård’, *Metro Express*, 5 May 2009.

<sup>56</sup> Betak, ‘Det mærkelige og mystiske Danmark’.

<sup>57</sup> Næsted Nielsen, *De hjerneløses kirkegård*.

<sup>58</sup> Hansen, ‘De Hjerneløses Kirkegård’.

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### 2.5 2017: Scientific and monetary value

In 2017, prompted by the psychiatric hospital's imminent move to a new location in Skejby, the brain collection's owners, the Regional Council for Central Jutland and AU, jointly announced that they had decided to shut down the brain collection. The scientific and therapeutic potential of the collection, they argued, did not measure up to the costs of moving it to the new location.<sup>59</sup> This decision spurred criticism from local and international brain bankers alike, and the collection's uncertain future was in the national news cycle for a few months. The critics were enraged that base economic concern could mean the end of a unique collection with a, in their view, untapped scientific potential, while the Regional Council maintained that economic considerations had not influenced their decision directly, but that it was rather based on scientific judgement. The ensuing developments, which are ongoing, is the subject of chapter 7, but the political controversy is worth including in this chapter, because it has been completely lacking of the bioethical perspectives present in the preceding two decades.

In previous instances, the calls for ethical deliberation had come from external parties, in response to new research developments. In 1991, SIND had successfully prompted the minister for health to put the Alzheimer's research project's use of the brain collection to the Ethical Council and in 2005, the same ministry had asked the Council to take up the question once more following months of public discussion involving a broad spectrum of actors. In 2017, however, two things were different. First, the occasion was not a new research project and second, the Ethical Council was asked to consult by the initiating party, namely the Regional Council for Central Jutland.

The Regional Council sent out the press release announcing their plans to close the collection on 19 April 2017. They argued for their decision because the money that would go to moving and securing the collection for future use at Skejby would be better spent elsewhere in the health research budget, citing the limited

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<sup>59</sup> Ole Steen Nielsen and Kristjar Skajaa to Region Midtjylland, 'Nedlæggelse af hjernesamlingen ved AUH, Risskov', 30 March 2017.

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scientific output from the collection.<sup>60</sup> However, they did not consider their decision financially motivated, given that closing the collection would not mean withdrawing money from the psychiatric research department that housed it.<sup>61</sup> On the same day, the Council sent letters to the Ethical Council, the national REC, SIND, and a host of other interested parties, requesting comments and concerns regarding the proposal to close the collection.<sup>62</sup> All these actors acknowledged the difficulty of balancing a regional health budget, and accepted the Council's decision to close the collection. However, all but the Ethical Council and the national REC encouraged the Regional Council to consult with these before making any final decision. Having done this already, the Regional Council could be relatively certain that its decision would not be challenged on bioethical grounds.

After the press release, many critical voices also joined the discussion. In a letter from 3 May, the new director of the brain collection, Gregers Wegener, and the head of his department, Dorthe Eggertsen, expressed deep concern and frustration with the closure. They argued the decision was “void of scientific vision”, as it was based on “narrow economic considerations, and a lack of insight

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<sup>60</sup> Nanna Jespersgård, ‘Hjernesamlingen På Risskov Nedlægges’, medarbejderportal - Institut for Klinisk Medicin, 19 April 2017, <http://clin.medarbejdere.au.dk/#news-10388>.

<sup>61</sup> Nielsen and Skajaa to Region Midtjylland, ‘Nedlæggelse af hjernesamlingen ved AUH, Risskov’, 30 March 2017.

<sup>62</sup> Det Etske Råd to Christian Boel, Region Midtjylland, ‘Høringssvar: Det Etske Råds besvarelse af henvendelse fra Region Midtjylland om evt. nedlæggelse af Hjernesamlingen, Psykiatrisk Hospital i Risskov + bilag’, 19 May 2017, <http://www.etiskraad.dk/etske-temaer/neuroetik/hoeringssvar/2017-05-17>; National Videnskabsetisk Komité to Region Midtjylland, ‘Henvendelse fra Region Midtjylland: Beder om National Videnskabsetisk Komité's bemærkninger til evt. nedlæggelse af Hjernesamlingen, Risskov’, 22 May 2017; Psykiatri- og socialledelsen, Region Midtjylland to Region Midtjylland, ‘Bemærkninger Fra Psykiatri Og Social Til Nedlæggelse Af Hjernesamlingen’, 22 May 2017; Region Midtjylland, ‘Dagsorden 21. Juni 2017, Pkt. 5: Psykiatriens Forskningsenhed TNU’, 21 June 2017; Regionernes Bio- og Genombank to Region Midtjylland, ‘Bemærkning Til Evt. Nedlæggelse Af Hjernesamlingen’, 24 May 2017; SIND - Landsforeningen for psykisk sundhed to Region Midtjylland, ‘Bemærkninger til evt. nedlæggelse af Hjernesamlingen, Aarhus Universitetshospital, Risskov’, 9 May 2017; STARS\* (Strategisk Alliance for Register- og Sundhedsdata) to Region Midtjylland, ‘Vedrørende Bemærkninger Fra STARS\* Om Evt. Nedlæggelse Af Hjernesamlingen, Psykiatrisk Hospital i Risskov’, 30 May 2017; Statens Serum Institut to Region Midtjylland, ‘Bemærkninger til evt. nedlæggelse af Hjernesamlingen, Psykiatrisk Hospital Risskov’, 1 May 2017.

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into the facts of the case, and knowledge about the resource and those research perspectives that are wasted”.<sup>63</sup>

Attached to the letter was 10 letters of support for the collection from internationally renowned brain bankers and pathologists, all of whom praised the collection as a unique and very valuable scientific resource, and called the decision to close it lacking in vision and dedication to important basic research.<sup>64</sup> I analyse these support letters in detail in chapter 7.

SIND, also, was not completely convinced. Although they did not challenge the decision directly, they were sceptical of two claims in the Council’s presentation. First, they wondered if it were true that the collection was of poor scientific value, given that both the REC and the Ethical Council had previously underlined the high scientific potential of the collection. Similarly, they drew attention to the many international letters praising the collection exactly because of its unique scientific value. Second, they questioned the contention that financial considerations had not played a role in the decision: “We find this hard to believe. The letter from Aarhus University mentions both directly and indirectly that it is precisely the costs of moving the collection that has led to the proposal of destruction”.<sup>65</sup>

While the bioethical questions of the past and the future that had been prevalent in previous debates were largely absent in 2017, the discussion turned to questions of value both scientific and monetary, and how to align the two. The

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<sup>63</sup> Gregers Wegener and Dorthe Eggertsen to Rektor Brain Bech Nielsen, Aarhus Universitet et al., ‘Nedlæggelse af hjernesamlingen ved Aarhus Universitetshospital, Risskov (med bilag)’, 3 May 2017.

<sup>64</sup> David A. Lewis, MD to Dean Ole Sten Nielsen, MD, DMSci, 22 November 2016; Goran Šimić, MD, PhD to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016; H. Ronald Zielke, Ph.D. to Dean Ole Sten Nielsen, MD, DMSci, n.d.; Maree J. Webster, PhD to Dean Ole Sten Nielsen, MD, DMSci, 21 November 2016; Naguib Mechawar, PhD to Dean Ole Sten Nielsen, MD, DMSci, ‘Re : Aarhus Brain Collection’, 13 December 2016; Sabina Berretta, M.D. to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016; Bente Finsen, Professor, dr. med. to Professor, MD, PhD, Dr.med., Gregers Wegener, 22 December 2016; Bente Pakkenberg and Susana Aznar to Læge, PhD, Dr.med., Gregers Wegener, November 2016; Mads Nielsen and Datalogisk Institut, Københavns Universitet to Læge, PhD, Dr.med., Gregers Wegener, 21 November 2016; Patrick R. Hof to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016.

<sup>65</sup> SIND - Landsforeningen for psykisk sundhed to Region Midtjylland, ‘Bemærkninger til evt. nedlæggelse af Hjernesamlingen, Aarhus Universitetshospital, Risskov’, 9 May 2017.

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University and Regional Council on their side argued that the low research output did not warrant further spending on the collection. All the while, biobankers and SIND questioned the validity of this measure as an indication of the scientific value of the collection, referring instead to its untapped potential as measured by experts in the field, and decrying the Council's decision as a shortsighted attempt to save money.

The ensuing developments, and a more detailed account of the events preceding the closure, are presented in the final chapter of the thesis. Here I will simply stress once more the shift in concern discernible in the debate's lack of the bioethical questions that were at the centre of previous debates. This change is most fruitfully understood as a change in temporal alignment. With the sudden necessity to act before the planned move to Skejby, and a construction budget that was persistently strained, the most pressing questions were neither those of the past, nor of the future. The present, with all its immediacy, made the question of financial value more pressing than it had ever been before. While it is not foregrounded by any of the actors (save perhaps SIND), the budgetary logic of the Regional Council, with the pervasive value of "efficiency", became much more guiding than any other values. Hardly present or far in the background are the keepers' desire to safeguard potentiality, or the patients' rights activists' calls for respect and dignity, both of which had been determining values in previous discussions.

### **3 CONCLUDING REMARKS**

In this chapter, I have followed bioethical discussions about and surrounding the brain collection since its introduction to a wide public. There are two major analytical insights from the material. First, I see a clear shift in the register of the discussions from the 1990's until today. When the first debates broke, the key questions concerned responsibility, ethics, and human rights. At issue was patients' (and next of kin's) rights to their own bodies, the autonomy of medical science, and jurisdictional boundaries. The focus was mostly on what had been done, how to characterise these actions, who was responsible, and how to remedy the wrongs. As later debates arose, the focus of the stories changed, and the

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rhetorical register of the actors shifted with it. In 2005, the key concern was securing the collection's future when moving forward with research. While some actors called for the same retroactive consent as proposed in 1991, the reason was no longer the past and its transgressions, but rather the future – to ensure the ethical sustainability of the collection by making sure all necessary precautions were taken. Simultaneously, the topos of the “brainless cemetery” was introduced, giving the story elements of horror and shock alongside ethical considerations. This would continue to develop as a parallel trope in all future coverage, from zombie-metaphors and visions of ‘mad science’ to the intense focus of the physical otherness of the brains suspended in preservative fluid – never to decay or otherwise show evidence of time passing. The further we move away from the initial debates, the more the key debates centre on the *value* of the collection, rather than the wrongs in its inception. At first, the key term is scientific value, as measured in the imagined benefits to future generations of the mentally ill. Against the transgressions of the past, this promised value repeatedly outweighed the calls for disposal or reburial. Latest, however, in 2017 politicians began to question whether the collection was worth keeping not ethically but in monetary terms. Would the returns be enough to justify decades of public spending on maintenance, infrastructure, and research on the collection?

In broad strokes, we sense a shift in temporal alignment in the discussions. From the beginning, the past held the most power of argument. Whether styled as wrongs committed or as visionary contributions to the present day, it was the actions of earlier researchers that kept the debates going. Then, in the 00's, the future took prominence, as hopes and dreams of future insight and benefits needed to be secured, so the past would not get in the way. In this period, the past was dismissed to horror stories and colourful imaginaries. And finally in 2017, the present became the determining tense, as financial savings in the now were deemed more important than either the possibilities of the future, or the transgressions of the past.

The other point I would make concerns an interesting temporal distinction between ethics and epistemology in the position of Raben Rosenberg. He has repeatedly made the point that we must not judge past actors by modern ethical

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standards. However, on several occasions he has also proposed we can go back to old journals (some as old as a century) and re-diagnose the patients in accordance with modern diagnostics, thus back-tracing cases of diseases that have only been acknowledged as such long after the patients' deaths. It would seem that while ethics are historical, epistemology is not. Scientific knowledge can be applied retrospectively, but ethical judgments remain in their original context.

In a similar move, Rosenberg along with other actors make the violations of the past something to be remedied – not to judge past actors, but to make sure to not compound the damage. The non-consensual practices of the past give rise to the wish that consent be obtained now, from whoever is in a position to give it. Once given, however, consent would seem to be everlasting. It is implicit in the very idea of an open-ended archive (and for Rosenberg the brain collection is just that), that future researchers can forever rely on the original consent to validate research aims. While ethics are recognised to be historical, our current standards are never questioned, nor does Rosenberg imagine they can be challenged in the future.



## *Chapter 7*

# Moving the collection: recent developments

Except for a few paragraphs in the preceding chapter, this thesis has been about the brain collection in the past. This, however, has not been the reality as the project unfurled. One of the first things I heard when I began asking around, while I was still writing my project proposal, was that there was talk of ending the collection. Two years after I began, the collection was officially terminated, and destruction seemed imminent. In the end, it was transferred to a new research institute in Odense, BRIDGE, which hopes to actualise some of its scientific potential in tandem with other kinds of brain research.

In this chapter, I reflect upon these developments, all the while continuing the overall research ambition of the thesis, which focuses on the temporal and material meanings of the brain collection as an archive. I begin by recounting the process of closure in 2016-17. I briefly summarised these events in subsection 2.5 of chapter 6, but go into more detail here. The focus also shifts somewhat, as I move away from the vocabulary of bioethics to that of value explicitly. Section 1 thus establishes the timeline, and also investigates the different registers of valuation that were employed at different times in discussions of the collection's future and present circumstances.

Section 2 revisits the theme of chapter 3, as I show that the quotidian administrative and practical work has continued to this day, and plays an often overlooked role in valuation of the collection. This section also reflects on the

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concepts of potentiality and actuality in the archive, something I believe explains the different and often contradictive valuation practices in this chapter. As will be clear, the Region of Central Denmark and AU were forced by the actuality of budgetary restraints to dispose of the collection, even if it held high scientific potential, not least because the region's budgetary prioritisation was on clinical care and therapy. BRIDGE, on the other hand, saw more than potential in the brain collection. They had wished to establish a brain collection of their own, but abandoned the idea due to the large investments and extensive infrastructure necessary for such an undertaking. For them, therefore, the brain collection represented the actualisation of an ambition they had thought out of reach.

The timeline and details of the following have been established from documents received from the Regional Council of Central Jutland, after I submitted a request for access to records pertinent to the brain collection. I also rely to some degree on press coverage and publicised minutes from council meetings with attachments. Several caveats with regard to the source material are warranted. The written sources do not cover all the negotiations and internal reflections in the decision-making. For example, subcommittees in the Regional Council will have weighed finances and priorities in much more detail than the publicly available records reflect. The documents are sufficient for the scope of this chapter, however.

The purpose of this chapter, to be clear, is not to determine whether a political decision was right or wrong, or whether faults were committed in the process. The purpose, in line with the thesis generally, is to parse out and display the multiplicity in perspectives on the brain collection. Here, they take the form of valuations, but as I make clear once more in the concluding remarks, they relate to the core analytical concepts of the thesis, namely the temporality and materiality of the archive.

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FINANCIAL COSTS

On 14 November 2016, the acting Dean of the Faculty of Medicine at AU, Ole Sten Nielsen, sent an email to the deans of medicine at the University of Aalborg, and the University of Southern Denmark. The email was short, a follow up from a previous meeting. After the meeting, he wrote, he had been in contact with the Regional Council of the Capital Region, and had learned that they did not wish to help finance the brain collection. “Just to be sure”, he continued, “What I heard you say at the meeting was that your universities also do not want to get involved?” Both recipients replied in the negative; they had no available funds to put to such use.<sup>1</sup> In December, the Dean and the head of the Department of Clinical Medicine, Kristjar Skajaa, met with CEO Christian Boel, and Medical Director for psychiatric and social medicine Per Jørgensen, both from the Regional Council of Central Jutland, to discuss the collection’s future. Four months later, in April 2017, the Region and AU jointly announced their decision to shut down the collection.<sup>2</sup>

These exchanges in late 2016 was the culmination of a three-year long deliberation of the future of the brain collection. In the final months of 2013, a committee was constituted to manage the brain collection. Its mandate was the following.

The committee’s assignment is to consider the future use of the brain bank, including approvals of any tissue requests, etc. for

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<sup>1</sup> Lars Hvilsted Rasmussen to Ole Skøtt and Ole Steen Nielsen, ‘SV: Dekanmøde’, 14 November 2016.

<sup>2</sup> Jespersgård, ‘Hjernesamlingen På Risskov Nedlægges’; Marie-Louise Krejler Andersen, ‘Flytteoprydning: 9479 Hjerner Skal Smides Ud, Når Psykiatrien Flytter Til Skejby’, *Horsens Folkeblad*, 19 April 2017, <http://hsfo.dk/artikel/104228>; Rikke Danielsen, ‘9479 Hjerner Må Ikke Flytte Med Til Skejby - TV 2’, *TV2 Østjylland*, 19 April 2017, <http://nyheder.tv2.dk/lokalt/2017-04-19-9479-hjerner-maa-ikke-flytte-med-til-skejby>; Morten Nystrup, ‘Hvad Stiller Man Op Med 9.479 Hjerner, Som Man Ikke Kan Bruge Mere?’, *JP Aarhus*, 19 April 2017, <http://jyllands-posten.dk/aarhus/ECE9514176/hvad-stiller-man-op-med-9479-hjerner-som-man-ikke-kan-bruge-mere/>; Region Midtjylland, ‘PRESSEMEDDELELSE: Hjernesamling i Risskov foreslås nedlagt’, Press Release, 19 April 2017, <http://www.rm.dk/om-os/aktuelt/nyheder/nyheder-2017/april-17/hjernesamling-i-risskov-foreslas-nedlagt/>; Ritzau, ‘Samling Af 9479 Patienthjerner Står Til at Blive Skrottet’, *Kristeligt Dagblad*, 19 April 2017, <https://www.kristeligt-dagblad.dk/danmark/samling-af-9479-patienthjerner-staar-til-blive-skrottet>; Helle Torpegaard, ‘Region Planlægger at Destruere Unik Samling Af 9.479 Hjerner’, *Dagens Medicin*, 19 April 2017.

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use in specific research projects. The committee is also tasked with producing a full account of the scientific value of the collection, which will be the basis on which to determine the necessary investment to maintain the brain bank. Therefore, the committee will have a successive task, which is to prepare an estimate of the necessary costs involved in upkeep and transport of the brain bank in connection with the planned move of the Psychiatric Hospital to AUH in Skejby, given that a scientific foundation for its continued existence is reached.<sup>3</sup>

The first thing to notice in this mandate is the subtle shift from future use in the first sentence, to scientific value in the next, and finally to financial costs in the last. While these are presented as three different tasks, it is clear that they are also interdependent. Use and value, I showed in chapter 5, are very often coupled in tissue banking, and as briefly presented in chapter 6, the decision to close the collection depended upon actors' understandings of both scientific value and financial cost. The specific process of valuation – the practicalities of the committee's work – is therefore worth pursuing.

The committee's first decisions, 18 December 2013, was to digitise as many neuropathological reports as possible, and to investigate how many patients' journals could be retrieved from hospitals and archives around the country, along with the cost of retrieving them. This clearly indicates what I also noted in chapter 4 and 5, that data and metadata, both historical and current, is at the heart of tissue banking. On the second meeting, 1 May 2014, they reinforced these decisions, and contacted institutions that might hold pertinent documents. They also hired a student assistant who would copy all the reports to pdf-format. These initial practices pertain to the value and use of the collection, because availability of data is crucial for both, and to estimating cost by seeking offers for the work involved in digitising, updating, and running the database, and upkeep of the collection itself, such as maintaining and changing the storage buckets and preservative liquid.

Finally, the second meeting's minutes refer to a document by Karl-Anton Dorph-Petersen, which listed all ongoing and planned research projects involving

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<sup>3</sup> Aarhus University, HEALTH, 'Notat: Kronologisk oprids af forløbet vedr. Hjernesamlingen', 5 December 2016, 1.

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the collection. A summary of the process notes, with reference to the second committee meeting: “On the basis of the attached list of current and future projects, the committee put forward that the collection is worthy of preservation”.<sup>4</sup> This was the committee’s first engagement with active scientific projects related to the collection, and their conclusion was unequivocal: there was a scientific foundation for maintaining the collection.

The third meeting, 1 July 2014, introduced the notion of disposal. The committee had estimated the cost of securing the brain collection to be 6.2 million Danish kroner, based on incoming offers of services. This included repacking the brains and outfitting the storage rooms. The committee then decided to try raising these funds from the Regional Council, from the University’s internal funding schemes, and from external private foundations. They also decided to gather price offers for the disposal of the collection. The funds for securing the collection depended on fundraising, as the brain collection itself had no dedicated budget. Hence, a contingency plan was necessary.

That the concrete estimate of securing the collection was followed by the question of disposal indicates very clearly that cost was not a secondary, but a continually present element of the deliberations. This is further strengthened by Eva Otto’s anthropological fieldwork inside the Regional Council (introduced in chapter 5), where she identified cost-efficiency as a pervasive and determining value in the Region’s affairs. The brain collection’s committee did not include representatives from the Region, but the mandate was clear that the committee’s work should include a thorough account of any affiliated costs.

By the time of the fourth meeting, 9 April 2015, it was certain that the brain collection would be maintained, and would move along with the rest of TNU to the new hospital in Skejby. The committee members all expressed their satisfaction with this decision. Besides this, the meeting focused on the continued practice of digitisation and retrieval of patients’ journals.

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<sup>4</sup> Aarhus University, HEALTH, 2.

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The fifth meeting, 25 June 2015, centred on this same question, as the committee had received an updated offer from the State's Archives on identification, retrieval, and digitisation of patients' journals and neuropathological reports. These services together would amount to between DKK 3.236.000-5.736.000, for which the committee decided to submit an application to a private fund. The question of data law was also raised, with regard to best practice for storing the data securely.

The sixth and final meeting, 25 October 2016, more than a year later, had a less optimistic agenda. Several things had complicated the planned move to Skejby in 2018. First, the collection's approval from the Danish Data Protection Agency had expired, and would have to be renewed. Second, the dean had been unsuccessful in securing financial support from other universities' health faculties (the final plea would come in the form of the email quoted above). Third, construction of the new hospital had been delayed, which meant that the storage rooms for the collection would not be ready in time for the moving date. This meant extra funds were needed, in order to move the collection into an external depot, and from there into Skejby when the storage rooms were ready.

At this final meeting, all possible ways of proceeding and their estimated costs were drawn up. I quote from the internal case summary:

- Retention of the collection at Aarhus University at the University's own expense would amount to DKK 7.560.000. Since neither the deanship nor the Department of Clinical Medicine has available funds, this possibility is unrealistic.
- Retention at Aarhus University with funds from the other three Danish universities. Since no positive replies have come in with regard to co-financing, this possibility is unrealistic.
- Disposal of the brain collection (wet preparations). It would cost between DKK 1.800.000 (internally at Risskov), and 2.300.000 (externally) to repack and dispose of the brains.

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- Handing over the brain collection to the University of Copenhagen. This is an option only if we receive a positive response through official channels, along with a concrete plan for transfer, storage, and financing. In that scenario, AU can contribute to the moving costs with an amount equal to the cost of disposal.<sup>5</sup>

It is a radical and surprising shift in tone from previous meetings to the final meeting, in which the only viable option would seem to be disposal. Before, the focus of the committee had been on securing, updating, and digitising the textual and contextual data related to the collection, and the physical brains themselves had only briefly been discussed with regard to the maintenance and update of preservative measures. Now, suddenly, the wet preparations had become a costly and problematic affair, principally due to the issue of space.

In March 2017, the dean Ole Steen Nielsen, and the head of the Department of Clinical Medicine Kristjar Skajaa sent a letter to the region with the heading “Termination of the brain collection at AUH, Risskov”.<sup>6</sup> Here, they wrote:

Aarhus University and the Region of Central Jutland have jointly evaluated the possibilities for both keeping and terminating the collection. The conclusion is that the collection’s scientific value does not measure up to the costs of moving and storing the collection. On this basis, the regional administration has decided to recommend the collection’s closure, and Aarhus University backs this decision. The university has no intention to cut funds from TNU as a consequence of the collection’s closure, and so financial factors have not been decisive in our evaluation.<sup>7</sup>

While this seems like a straight-forward statement, the quote requires some unpacking. The first sentence likely refers to the committee established late 2013, which was tasked with accounting for both the scientific value and monetary costs of the collection. However, as per the minutes from the second committee meeting, 1 May 2014, they had concluded that the collection did hold scientific

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<sup>5</sup> Aarhus University, HEALTH, 3.

<sup>6</sup> Nielsen and Skajaa to Region Midtjylland, ‘Nedlæggelse af hjernesamlingen ved AUH, Risskov’, 30 March 2017.

<sup>7</sup> Nielsen and Skajaa to Region Midtjylland.

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value and was “worthy of preservation”.<sup>8</sup> As I described in chapter 6, both the Ethical Council and the National Research Ethics Committee had stressed the collection’s scientific value as an important argument for suspending ethical qualms, and keeping the collection for research.<sup>9</sup> Therefore, the next sentence is surprising: “the collection’s scientific value does not measure up to the costs”. As I showed from the committee meeting minutes, the initial cost of the move to the new Skejby premises was not considered a problem, and just a year earlier everyone had considered this move a certainty. What had happened in the meantime was delays in the new hospital’s construction, which also meant that the project exceeded its budget.

As mentioned in chapter 6, several stakeholders commented on this change in the collection’s status. SIND was most vocal. In a letter from May 2017, the chairperson Knud Kristensen raised several issues with regard to the Region’s decision.<sup>10</sup> He began by stressing that the original non-consensual removal of the brains was a violation of the rights of patients’ and their next of kin. As a minimal consequence of this wrongdoing, he wrote, “we owe it to those affected” to consider the issue carefully. Kristensen here echoes a sentiment from the previous bioethical debates; that decisions about the brain collection should not only take account of contemporary and future scientific concerns, monetary concerns, and ethical concern for living stakeholders. They should also, and perhaps principally, refer back to an ethical debt to the unwitting donors whose sacrifice would be further disgraced if valuable material was thrown away for the wrong reasons. Using the material for as many purposes as possible, such is the argument, is the only way to make sure the ethical violation was not committed in vain.

The principal question for SIND, therefore was “Is the collection without scientific value?” Knud Kristensen was not convinced this was the case. His letter

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<sup>8</sup> Aarhus University, HEALTH, ‘Notat: Kronologisk oprids af forløbet vedr. Hjernesamlingen’, 2.

<sup>9</sup> Det Etske Råd, ‘Bilag 6: Det Etske Råds udtalelse om depotet af hjernepræparater på Psykiatrisk Hospital i Århus’; Det Etske Råd, ‘Høringssvar, 7. Marts 2006’; Den Centrale Videnskabetiske Komité, ‘Vedr. Forskning På Hjerner Fra Hjernesamlingen På Psykiatrisk Hospital i Risskov, Århus’, 22 February 2006.

<sup>10</sup> SIND - Landsforeningen for psykisk sundhed to Region Midtjylland, ‘Bemærkninger til evt. nedlæggelse af Hjernesamlingen, Aarhus Universitetshospital, Risskov’, 9 May 2017.

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mentions the statements from the National REC, in which the collection was called a valuable resource for research; it also mentions that many medical researchers had voiced publicly the collection's "unique scientific value".<sup>11</sup> His letter ends, as also cited in chapter 6, with the issue of money. I quote in full:

The letter from Aarhus University points out that economic factors have not been decisive in their evaluation. We find this hard to believe.

It is put forward both directly and indirectly that it is precisely the costs of moving the collection that has led to the proposal of destruction.

SIND must encourage that no decisions are made solely on short-sighted financial grounds.<sup>12</sup>

Based on the reasons presented, SIND were not convinced that destruction was the right way forward. In terms of concrete proposals, they recommended that AU and the Regional Council made inquiries at other research institutions to find out if they would be interested in taking over the collection if there was no way to keep it in Aarhus.

The letters of support that Knud Kristensen referred to had come in during November and December 2016, after the brain collection's committee had discussed the prospect of disposal. It was Gregers Wegener, the head of TNU after Raben Rosenberg, as well as Dorph-Petersen, who had encouraged their collaborators from around the world to express their views on the collection and its scientific value. In May 2017, Wegener and the head of his department, Dorthe Eggertsen, sent a response to the press releases that had announced the collection's closure. As noted in chapter 6, Eggertsen and Wegener expressed their "concern and bewilderment" of the press releases, and called the decision "thoughtless and devoid of scientific vision".<sup>13</sup> They feared that the decision was founded on "narrow economic considerations, and a lack of insight into the facts of the case, and

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<sup>11</sup> SIND - Landsforeningen for psykisk sundhed to Region Midtjylland.

<sup>12</sup> SIND - Landsforeningen for psykisk sundhed to Region Midtjylland, 2.

<sup>13</sup> Wegener and Eggertsen to Rektor Brain Bech Nielsen, Aarhus Universitet et al., 'Nedlæggelse af hjernesamlingen ved Aarhus Universitetshospital, Risskov (med bilag)', 3 May 2017.

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knowledge about the resource and those research perspectives that are wasted”.<sup>14</sup> To remedy this lack of insight and knowledge, Wegener and Eggertsen had attached the 10 international letters of support to their own letter.

Reading these letters considerably reinforced my conclusions from chapter 5, namely that ideas about waste and value are closely tied together in tissue collections. Wegener and Eggertsen themselves, in the quote above, speak of the virtual waste of a unique and scientific resource with important research perspectives. This sentiment was echoed in almost all the support letters. Often, a value claim is directly attendant on such references to waste. Here follow a few examples:

“It would be a significant scientific and ethical mistake to **waste the opportunities** that could be derived from this **valuable material**”.<sup>15</sup>

“The decision to discard such a **precious resource** would represent for neuropathologists and neuroscientists a terrible and sad mistake, as well as **a loss that could never be repaired**”.<sup>16</sup>

“A **significant investment** and **irreplaceable resource** will be **lost** if it is not preserved now”.<sup>17</sup>

“The **loss** of the Aarhus Brain Collection would represent nothing less than a tragedy. I thus truly hope that solutions will be found to protect this **invaluable and irreplaceable resource**”.<sup>18</sup>

A closer reading of the letters reveals some of the grounds for these value claims. One factor stands out as common in all letters: The excellence of the material itself. Four qualities contribute to the excellence of the collection over similar collections. The first is the well-documented clinical information available because of the patients’ hospital stays. The second is the very low post-mortem interval. Readers will recall from chapter 3 the standard collection practice

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<sup>14</sup> Wegener and Eggertsen to Rektor Brain Bech Nielsen, Aarhus Universitet et al.

<sup>15</sup> Maree J. Webster, PhD to Dean Ole Sten Nielsen, MD, DMSci, 21 November 2016.

<sup>16</sup> Hof to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016.

<sup>17</sup> David A. Lewis, MD to Dean Ole Sten Nielsen, MD, DMSci, 22 November 2016.

<sup>18</sup> Naguib Mechawar, PhD to Dean Ole Sten Nielsen, MD, DMSci, ‘Re : Aarhus Brain Collection’, 13 December 2016.

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included *in situ* fixation less than 12 hours after death (and sometimes considerably faster). Unpleasant as it was for Irene and her peers, this procedure means that the tissue is still very well preserved today, showing no deterioration in the tissue structures. Third, the age of the material, which means it holds pre-psycho-pharma specimens, makes it useful for control studies. Fourth and lastly, the different means of preservation, both wet fixation and paraffin-embedding, means that complementary methods of investigation can be made on tissue from the same brain, e.g. stereological analysis of a wet brain slice, and immunohistochemical studies of a paraffin-block, thus obtaining two distinct types of information that supplement each other in a synthesising analysis. These four qualities alone, according to the ten letters,<sup>19</sup> makes the brain collection so valuable that its loss would constitute a grave virtual waste. The material value of the collection thus rests to a high degree on the conscientiousness of K.A. Lorentzen, and his work in standardising the practices at HPI.

Some of the letters mentioned additional considerations, which served rhetorically to heighten the collection's value further. Sabina Berretta, the Scientific Director of the Harvard Brain Tissue Resource Center, unknowingly continued Lárus Einarson's history of truth, as she urged the dean of medicine at AU to "continue the long tradition of enlightened, visionary work that created and curated it".<sup>20</sup> While her primary focus, along with the other letter-writers, was on the promises the collection held for the future, Berretta also directed attention to tradition as an important component in scientific value. Like Einarson, she attempted the double work of portraying at once a continuance and a break. The message is, it makes sense to continue enlightened visionary work, in line with

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<sup>19</sup> David A. Lewis, MD to Dean Ole Sten Nielsen, MD, DMSci, 22 November 2016; Goran Šimić, MD, PhD to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016; H. Ronald Zielke, Ph.D. to Dean Ole Sten Nielsen, MD, DMSci, n.d.; Maree J. Webster, PhD to Dean Ole Sten Nielsen, MD, DMSci, 21 November 2016; Naguib Mechawar, PhD to Dean Ole Sten Nielsen, MD, DMSci, 'Re : Aarhus Brain Collection', 13 December 2016; Sabina Berretta, M.D. to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016; Bente Finsen, Professor, dr. med. to Professor, MD, PhD, Dr.med., Gregers Wegener, 22 December 2016; Bente Pakkenberg and Susana Aznar to Læge, PhD, Dr.med., Gregers Wegener, November 2016; Mads Nielsen and Datalogisk Institut, Københavns Universitet to Læge, PhD, Dr.med., Gregers Wegener, 21 November 2016; Hof to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016.

<sup>20</sup> Sabina Berretta, M.D. to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016.

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tradition, *and* it will most likely pay off in unimagined ways with regard to future knowledge production.

Patrick R. Hof, Associate Director of Autism BrainNet and the Mount Sinai Alzheimer's Disease Research Center, introduced yet another valuation. It is worth quoting him at length, as it draws interesting threads to arguments from earlier chapters:

While one may argue that such materials are old and have had a **shelf-life** too long to make them **useful** still, such brains are in fact a nearly unique collection of **historical significance** and a relatively unexploited resource in the study of neuropsychiatric illnesses. It is unique in the sense that there are very few repositories of human brains available and such collections have an **intrinsic value** that cannot be measured in terms of **scientific value**.<sup>21</sup>

This quote, which is from the second paragraph of Hof's letter, is interesting because it foregrounds historical significance and intrinsic value in relation to scientific value. The trope of shelf life (which echoes Paul Francis from the Wellcome Witnesses Seminar in chapter 5), indicates that Hof is aware that value is usually dependent on usage. He quickly dismissed this issue in the case of the Danish brain collection, noting its uniqueness and that it was unexploited, implying its worth if exploited. Later in his letter, he reiterated this valuation, as he predicted that future "advances in technologies will make the use of such resources increasingly easier and affordable, rendering brain materials such as those deposited in the Aarhus Collection invaluable for more scientists".<sup>22</sup> However, his phrasing gives primacy to an ontological valuation that is not dependent on scientific or economic factors, but relies instead on a culture and history in which some objects are valuable simply by virtue of their object status. By referring to this value before arguing for the collections scientific value, Hof anticipated readers with the foregone conclusion that the collection's scientific value did not warrant the economic cost.

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<sup>21</sup> Hof to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016.

<sup>22</sup> Hof to Dean Ole Sten Nielsen, MD, DMSci, 2.

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Many of the letters attempt something similar, namely by referring to the ethical duty to sound medical science, and the need to pursue knowledge wherever possible, especially when it comes to understanding diseases. This is the same argument we saw Raben Rosenberg pursue in chapter 6: that ethical behaviour is scientifically sound behaviour, and that failing to conduct scientific studies amounts to unethical conduct. In his 2016 letter, Goran Šimić warned it would be “a terrible scientific and ethical mistake to discard [the collection]”,<sup>23</sup> and Webster used the same phrasing in the quote above. Hof called the notion of disposal “plainly unethical”.<sup>24</sup> By invoking ethics alongside scientific value, the letters pre-empted a purely scientific response from AU, urging the dean of medicine to reflect upon ethical principles as well.

A final persuasive device in the letters of support is the awareness of money. Many of the letter writers are experienced brain bankers themselves, and they used this to convey to dean Nielsen that their pleas were not naïve or ill-informed, but that they were familiar with the practicalities both fiscal and logistic involved in brain collection. For example, Šimić, who is the head of the department that hosts Zagreb Collection of Human Brains, informed that he was “fully aware of the large costs necessary to build and maintain a brain collection”, but was adamant that the value in this case would “justify every effort to secure it for future researchers”.<sup>25</sup> Hof also was “fully aware of the operational costs and logistic difficulties in maintaining large brain collections”, and he additionally employed the economic awareness in warning that “a major financial investment will be irremediably lost” in the event of destruction.<sup>26</sup> David Lewis, in the quote above, also used the word ‘investment’ to connote the substantial amount of money that had already gone into establishing, maintaining, and updating the collection.

A month after Wegener and Eggertsen’s letter with the attached letters of support, Kristjar Skajaa, the head of the School of Clinical Medicine, replied. His letter, dated 8 June 2017, is dismissive in tone: “Aarhus University has not found

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<sup>23</sup> Goran Šimić, MD, PhD to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016.

<sup>24</sup> Hof to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016, 2.

<sup>25</sup> Goran Šimić, MD, PhD to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016.

<sup>26</sup> Hof to Dean Ole Sten Nielsen, MD, DMSci, 23 November 2016.

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that the objections in the letter from Gregers Wegener and Dorthe Eggertsen prompts any change in the University's support of the decision to close down the brain collection". He added for one argument concerning material availability, "this matter has previously been discussed with Gregers Wegener". Likewise, for the objections from SIND and other parties who had been invited to comment on the decision, Skajaa wrote: "The arguments in the incoming letters have all already been discussed internally between the Region and Aarhus University. Thus, Aarhus University has not found that these letters prompt a change in its support for closure of the collection". He maintained the original argument, and assured that "the university recognises that the collection potentially does hold scientific value, but the university maintains its support for closure, because this value does not measure up to the costs involved in moving, storing, and maintaining the collection".<sup>27</sup> Later that month, at a meeting 21 June 2017, the Regional Council carried the motion to shut down the collection.

For all the different arguments and multiple kinds of valuation invoked by the proponents of preservation, the decision was legitimised with reference to economic necessity. Concerns beyond immediate scientific value and economic costs, such as historical or intrinsic value, ethical duty, or the unique nature of the collection, were never addressed by neither the Regional Council, nor Aarhus University. If the past and future had held any sway in previous debates, these tenses had now receded into the background to make space for the necessity of present economic value.

## 2 LOGISTICS OF CLOSURE

After the decision was final began the practical and administrative work. In the Council's decision, they had granted a hiatus period, in which other institutions could express their interest in acquiring the brain collection. The research centre BRIDGE expressed its wish to take over the collection in the fall of 2017,<sup>28</sup> and the brains moved there in November 2018, around the same time the rest of Risskov

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<sup>27</sup> Kristjar Skajaa, 'Vedr. brev om nedlæggelse af hjernesamlingen', 8 June 2017.

<sup>28</sup> Christian Boel, Region Midtjylland to BRIDGE v/ Region Syddanmark og Syddansk Universitet, 'Forespørgsel Om Overtagelse Af Hjernesamlingen Ved AUH Risskov', 17 November 2017.

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Psychiatric Hospital moved to Skejby. BRIDGE – Brain Research Inter-Disciplinary Guided Excellence is a translational brain research centre with a mission to connect “bench to bedside and back again”.<sup>29</sup> Their research fields and methods are diverse, including neuroimaging, molecular biology, computational science, and epidemiology. BRIDGE had been thinking about establishing their own brain collection for some years, but had concluded in 2014 that it would be too resource intensive. Therefore, the possibility of inheriting the Risskov collection was promising. In a review of the case from January 2018, the spent costs of the brain collection so far was estimated to DKK 150-200.000.000 (corrected for inflation), reflecting once more the large financial investment that the brain collection also constituted.<sup>30</sup> The inclusion of this figure attests to BRIDGEs valuation of the collection as not only potentiality (which predominated in the preceding section), but as actuality; it was in itself an asset because it allowed BRIDGE to forgo the great expenses, and the daunting work of establishing a collection infrastructure, donor scheme, etc. The estimated spent cost thus moves the register of scientific valuation from some unrealised potential (future knowledge) to an already actualised value, namely skipping the start-up phase of brain collecting. The document also mentions the possibility of supplementary studies on brains acquired today, but having the core collection handed over from Risskov meant such donations could be limited to a few relevant specimens, rather than a large-scale collection.<sup>31</sup>

The move itself involved extensive admin work and logistical planning: How to move the brains, where to store them, how to pay for the move, etc. The timeline was relatively compressed, as expressed in a letter from the Regional Council of Central Jutland to BRIDGE in November 2017: “If, by the end of the first quarter of 2018, it is estimated that no agreeable deal can be made, the brain collection will

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<sup>29</sup> ‘BRIDGE - University of Southern Denmark, SDU’, accessed 10 October 2019, [https://www.sdu.dk/en/om\\_sdu/institutter\\_centre/bridge](https://www.sdu.dk/en/om_sdu/institutter_centre/bridge).

<sup>30</sup> Kim Brixen and Charlotte Horsted, ‘Overtagelse: Hjernesamlingen fra Risskov’, 29 January 2018, 3; this document was attached to an email: Charlotte Horsted to Kim Brixen et al., ‘Referat Fra Procesmødet Vedr. Mulighed for Overdragelse Af Hjernesamlingen Til Odense - 20 Febr. 2018’, 22 February 2018.

<sup>31</sup> Brixen and Horsted, ‘Overtagelse: Hjernesamlingen fra Risskov’.

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be disposed of”.<sup>32</sup> Accordingly, meetings between BRIDGE and Region MIDT progressed quickly. By February, BRIDGE had received and conducted preliminary examinations on nine preparations from the collection, and from this sample concluded that the collection was acceptable for their purposes.<sup>33</sup>

According to an offer from Fortum Waste Solutions A/S, 22 June 2018, the move itself would cost DKK 340.000. The procedure would be as follows. First, the existing buckets would be placed in groups into larger buckets for the transport, and then moved into a transporter truck cooled to 19°C, and driven to Odense. AUH Risskov was willing to contribute with the estimated cost of destruction, ca DKK 2.3 million.

A total 300 square metres was needed to house the collection comfortably. The University of Southern Denmark was willing to provide the space within existing buildings, which would however need fitting with ventilation and lab equipment. After the brains had been installed in suitable rooms, a number of continual maintenance tasks would be necessary to preserve the collection correctly: A routine control every month to ensure the safe state of the buckets, and if necessary replenishing the formalin. Digitisation of the pathological reports, to be done by and internal IT department at Odense University Hospital. Retrieval or access to remotely stored medical journals, for example from the National Archives and other hospitals. Prepping of specimens for concrete research projects. Finally, the scientific custodians of the collection had to ensure ease of access for external Danish as well as International research groups, in order to maximise the scientific use of the collection. To this end, a committee was established, which included members from hospitals in Odense, Aarhus and Copenhagen, as well as a representative of SIND.<sup>34</sup> These arrangements were satisfactory to all parties, and after several meetings to work out the concrete details, the collection moved to Odense in November 2018.

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<sup>32</sup> Christian Boel, Region Midtjylland to BRIDGE v/ Region Syddanmark og Syddansk Universitet, ‘Forespørgsel Om Overtagelse Af Hjernesamlingen Ved AUH Risskov’, 17 November 2017.

<sup>33</sup> Brixen and Horsted, ‘Overtagelse: Hjernesamlingen fra Risskov’, 4.

<sup>34</sup> Brixen and Horsted, 5–6.

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With regard to logistics, it is worth describing another issue that appeared before BRIDGE had announced its interest, as the public became aware of the collection's impending closure. Days after the Region's final decision, the Council was contacted by two siblings who wished to retrieve their grandmother's brain from the collection in a state that would make it possible to bury it at their family's cemetery plot. As noted in chapter six, this possibility had previously been raised in the course of public discussions, but this was the first time someone had made a request. On 29 June 2017, a legal consultant for the Council, Charlotte Rønde Mikkelsen, forwarded this application to the Ministry of Health, along with a request for "your assessment of the Region's obligation to hand over the brain". Mikkelsen noted the Region's immediate judgment to be that there was no warrant to hand over the brain. However, she also quoted earlier statements from the Ethical Council and the National REC, both of which had expressed that next-of-kin ought to have a right to retrieval of their family member's brain.<sup>35</sup>

A week later the reply came from Emilie Norré Sørensen, a managing clerk in the Health Ministry's department for medical drugs, international relations, health law, and psychiatry. She first referred to the relevant § 34 in the Danish health law, according to which "a patient has the right to have released biological materials that this patient has surrendered in the course of treatment". Said release requires the patient can document "a special interest", which includes for example "handover with a view to burial". Sørensen noted that this § covered only the rights of the patient itself. However, "In the special case of the brain collection, the principle of patient autonomy has been neglected in accordance with the law at the time of removal. Since patient autonomy is a foundational principle today, we assess that next of kin can fill the patient's legal position". According to this reading of the law, the Regional Council was indeed obliged to surrender the brain. Sørensen concluded her letter by reiterating a statement from the National REC

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<sup>35</sup> Charlotte Rønde Mikkelsen to Sundheds- og Ældreministeriet. Afdeling for Lægemedler, Internationale Forhold, Sundhedsjura og Psykiatri. Att. afdelingsleder Dorthe Søndergaard, 'Nedlæggelse af Hjernesamlingen i Risskov', 29 May 2017.

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that urged to inform the next of kin that the brain might have been subject to surgical or post mortem operations and could therefore be not intact.<sup>36</sup>

Accordingly, the Region's legal team tried to find out how to hand over the brain. The brain could not be legally buried in its formalin-preserved state, since the preservative liquid is both damaging to the environment and hazardous for humans. It would be neither safe nor legal to surrender the brain in such a state. Cremation was therefore considered, but when a crematorium was contacted, they refused to cremate anything containing formalin. Therefore, even though the two siblings did have a right to receive their grandmother's brain, the request was denied with reference to § 34, subparagraph 2 in the health law, which states that a request for handover may be denied if the patient's interest are deemed less weighty than considerations for public or private affairs. The Region concluded, "The consequences for humans and the environment exposed to formalin are weighty indeed".<sup>37</sup>

The messy and complicated administrative work that was described in chapter 3 regarding the collection's institution has never ceased. Alongside debates about scientific and monetary value, about bioethics and waste, the quotidian work of management carries on. Management at BRIDGE showed great awareness that the acquisition of the brain collection was not simply a matter of taking over the physical specimens, but also meant assuming the responsibility of what I called in chapter 5 the infrastructure of the archive. Epistemic things are not simply things; data is not dormant; perpetual administration is necessary for a scientific archive to be useful. What exactly will be the uses of the collection in Odense, it is too early to say, but from the very first BRIDGE was prepared to dedicate the work and resources to make sure the collection would not be forgotten once more, as it had been in the 1980s and 90s. Just like Dorph-Petersen, who had desired an "organically symbiotic growth of the collection and its infrastructure",<sup>38</sup> BRIDGE knew, as had also Lorentzen, that securing the collection's value lay just as much

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<sup>36</sup> Emilie Norré Sørensen to Region Midtjylland, Att: Charlotte Rønde Mikkelsen, 'Udlevering af Hjerner fra Hjernesamlingen i Risskov', 6 July 2017.

<sup>37</sup> Charlotte Lund Pallesgaard to Sundsheds- og Ældreministeriet. Att.: Emilie Norré, 'Orientering vedrørende overdragelse af hjernesamlingen i Risskov', 20 December 2018.

<sup>38</sup> Dorph-Petersen to Erslev, 'RE: Citat', 21 February 2017.

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in its organisation and deployment in specific experimental systems, as in proper storage of its physical specimens.

### 3 CONCLUDING REMARKS

In this chapter, I have reviewed the most recent case involving the Danish brain collection: its termination by the Regional Council of Central Jutland and subsequent move to the research centre BRIDGE in Odense. In doing so, many of the themes from previous chapters have been revisited.

This chapter shows the multiplicity of the Danish brains most clearly, as actors' valuations and conceptions at times proved incommensurable to a degree where they could not understand each other's' reasoning. It became clear that different people see very different things when looking at the collection. The regional council and management at AU saw a complication, a problem, an obstacle, which tapped resources from the vital new hospital, without giving much in return. SIND saw a lingering ethical travesty, which could only be remedied by using the brains in research toward therapy. BRIDGE saw a way to save resources and funds, by inheriting an already established brain collection; what was a drain on the budget in Aarhus could be a way to save money in Odense. The international brain bankers, finally, seem to have held the multiple brain collection together in their minds. They addressed not only the scientific value of the brains, but also demonstrated awareness of monetary, ethical, cultural, and historical perspectives, and included these in their valuations of the collection.

The chapter has also continued interests from preceding chapters. The goals and ambitions of the Danish brain collections' institutors (i.e. Einarson), which I analysed in chapter 2, drove actors' narratives, as the brains were figured either as valueless remnants of science past, or as visionary presents from this past to the promising future. The quotidian work introduced in chapter 3, continues to play a big part in the Danish brain collection. The scientific engagements with it, presented in chapter 4, lingered in the background of discussions throughout, as did the intertwined ideas about waste and value presented in chapter 5. Bioethics,

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the thread followed in chapter 6, was largely absent in this most recent discussion, though exactly why this is the case is still unclear to me.

Finally, a word on the archive. Both in the internal committee meetings 2013-2016 and in BRIDGEs preparations for the take-over, the organisation and digitisation of data were central concerns. This attests to the importance of keeping the archive alive, and making it available to any possible uses. It bears repetition, even at this late stage in the thesis, that the archive is almost a living thing, an ‘organically symbiotic’ experimental system, which must be cared for and continually watched over in order to materialise questions, to create and adapt to new epistemic things, to reconstitute itself in surprising ways; in short: to be useful. A dead archive is little more than a repository of lost hopes. A living one can never be predicted. This is its strength, but it is also what makes it so work intensive.

## Conclusion

In this conclusion, I address each of the overarching concepts constructed in the theoretical framework of chapter 1, demonstrating how each has contributed to my reading of the brain collection's history. In turn, I show how the close empirical engagement with the brain collection and all its multitudinous meanings, has expanded, enriched, and elucidated the theoretical and historiographical concepts. For a summary of the individual chapters, see the introduction.

**Materiality** in chapter 1 was understood primarily with regard to the individual brains: The concept covered the histological, ontological, conceptual, and logistical work attendant on the Danish brains. It pointed backwards to the work itself – the material interventions necessary before experimental work proper could begin – which simultaneously was made to disappear into its object. According to Rheinberger, a preparation is an epistemic thing that is at once defined by its making, and hides the very process of making it. Its purpose is to stand in for the class of objects it represents. It does this first by partaking in the very materiality of its object, and seemingly remaining an unaltered specimen of this class. Second, it nevertheless allows accesses that can only be feasible through extensive material alterations. In the case of the brain collection, especially chapters 2, 3, and 4 demonstrated the importance of this concept. Lárus Einarson's work lends itself readily to Rheinberger's description of work in the life sciences: Through the elaboration of an experimental system, featuring most prominently his gallocyenin chrome alum stain, and the simultaneous construction of novel epistemic things, he developed a new vision for brain science. It relied at once on the grand system of practices, tables, models, and theories, and on the concrete

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things that he prepared within this system. Most crucially, in this regard, was Einarson's belief that his work would make histologists able to describe alterations in living brains, through the application of his classification of structural activity changes. That this classification was only possible on prepared tissues was a problem that Einarson was well aware of, and which he went to great rhetorical lengths to address.

For K.A. Lorentzen in chapter 3, material engagements with the collection held a different purpose. While he did share Einarson's ambitions at first, his work would go on to focus on standardisation of collection and preservation practices, as well as tutoring his colleagues and students in the art of *autopsia* in the brain. Lorentzen's material practices were directed less towards the individual brain as a model for the brain *in abstractum*, and more towards the creation of a well-documented collection. In making sure that procedures were the same throughout the country, and scrutinising the detail and quality of collection work continually, he worked to make sure that all results could be verified and re-examined if need be. Securing the physical specimens alongside the written reports was part of his duty to science. This points towards the second overarching concept.

**The Archive** is a concept I have received from Lorraine Daston's edited volume *Science in the Archives*, and adapted through the work of Bruno Strasser and Karin Tybjerg to suit my ends in this thesis. The most central theoretical concept in my project, it brings together all the other overarching themes and relate them in complex ways to temporality. The archive, which is also always material and multiple, ceases to be an archive when its relation to time is severed. Its *raison d'être* is, according to Daston, the need to prepare for a radically unpredictable future. From the present, archives are constructed with the future in mind, always as an attempt to predict what might be useful or valuable for future generations (whether historians, climatologists, or neuroscientists), but because the future is unknowable the archive adapts two tenets: It is opportunistic and open-ended. Archivists cannot collect everything, but they also cannot be content with collecting only one kind of thing.

These two features certainly are present in the history of the Danish brains. Strömngren and Einarson were opportunistic indeed, when they decided to amass

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brains from hospitals around the country. Open ended also was the collection, as no formal criteria determined which tissues were collection-worthy, and which were not. Lorentzen certainly doubled down on this feature when he made sure to keep everything, and every kind of thing. Besides the pathological reports and the wet specimens, he also kept the paraffin-blocks, the metadata from patients' records etc. Likewise, when he made sure that a host of different assessments were made to each brain, including morphological, histological, anatomical, and pathological examinations not only of the disease areas, but also of standardised parts across all cases. Its opportunism was documented through a review of scientific studies in chapter 4, in which it became clear that the collection could be, and has been, turned to a host of different purposes, adapted to new agendas, methods, and questions depending on the specific needs of the researchers. This review also made clear, by returning to Rheinberger's epistemology of the concrete, the co-dependency of matter and concepts in the archive, and specifically in the Danish collection.

The collection's indeterminate purpose – its opportunism – also gave rise to controversies and debates, as the last three chapters of the thesis made clear. As more and more actors made claims to or against the collection, its value was questioned. Increasingly throughout these debates, the very criteria for determining its value were debated heatedly, as actors did not agree on what kind of value was most important, or indeed how to measure specific kinds of value on which they did agree. Coupled with the concept of waste, the valuography of the collection highlights, like the perspectival concept of the archive, that history is a key contributor in the determination of the collection's nature and purpose.

Finally, awareness of the **multiplicity** of perspectives on the Danish brains, has been my way of avoiding confusions and conflation when ordering and approaching my source material. Even if the brain collection remains physically unaltered (which is not the case), the conceptual, metaphorical, and ideational engagements with it render different objects, at times radically so. This became clear especially in the last three chapters of the thesis, where I showed that disagreements over valuation practices could be largely traced back to differences in historical, epistemological, and ontological perspectives on the brain collection.

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As I wrote in chapter 1, like Mol's arteriosclerosis the Danish brain collection has undergone changes whenever it entered a new space, whether histological, bioethical, logistic, fictional, political, or historical. In meeting new actors, both its material and conceptual way of being take on new meanings. As some highlight its past connections to buried bodies, others point to its future enrolments in biomedical techno-scientific arrangements. As some draw out the collection's drain on hospital resources, others note its already invested time and money. As some insist that it is 'weird' or 'unnatural', others share a daily life with it, and see it as an integral and unquestionable part of modern medicine.

These multiplicities have proven good to think with, as it is often in misunderstandings and controversies that the historian can identify actors' implicit ideas and worldviews. However, I have aimed in this thesis not only at an analysis of difference and disagreement but also at a reintegration of the multiple perspectives. By way of a thorough historicisation, and a continual engagement with the archival features of the Danish brains, a fragmented multiplicity has gradually given way to a more cohesive account of the collection, while still allowing for perspectival difference.

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Although it has sometimes been a lonely endeavour, writing this thesis, and conducting my PhD research, has afforded me the chance to meet and become friends with many wonderful people. Moments of shared intellectual excitement, or of agreement on the hardships of writing, have made an otherwise arduous task pleasant, and left me with renewed energy and delight in my work.

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Meeting minutes and attachments from the Region of Central Jutland can be found at their website:

<https://www.rm.dk/politik/regionsradets-moder/dagsordener/>

I have accessed files from the Danish National Archives (Rigsarkivet) in both Copenhagen and Viborg. The specific files are listed below with reference to the files' creator, collection, package number, and journal number.

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## Abstract

In Denmark, from 1945-1982 the Institute of Brain Pathology collected 9.479 brains without consent from psychiatric patients who died in hospital care. The collection is still used in research. These brains and their history are the subject of this PhD thesis. In seven chapters, I follow the collection in different contexts from the histological techno-scientific conception of its creation, over the quotidian practicalities of collecting and storing, and uses in research, to heated bioethical debates, and its depictions in Danish popular culture.

Temporality and materiality is at the core of my work, connected through the theoretical concept of *the archive*. As developed in the 2017 volume edited by Lorraine Daston, 'archive' provides a vehicle for thinking about the temporalisation of matter, and the materialisation of time in ways that enrich, complicate, and elucidate both concepts. The archive consists of material things whose nature and significance is determined by their relationship to time both past and future. The archive itself is meaningless without temporality, but useless without matter. These insights contribute in profound ways to the understanding of the Danish brain collection.

My thesis presents histories of epistemology and of ethics of the Danish brains. In the first half of the thesis, I couple the concept of the archive with Hans-Jörg Rheinberger's historical epistemological work, which emphasises the life sciences as a site where matter and theory, experiment and hypothesis are always co-constitutive. Through such an 'epistemology of the concrete', and a heightened

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sensitivity for its archival components, the brain collection is shown to be a dynamic and unpredictable site of knowledge production.

The thesis' second half explores the brain collection's entry into the field of bioethics. Since the 1990s, the brain collection has been attributed a plethora of values, such as ethical, monetary, scientific, historical, and has been evaluated on equally diverse scales. Continuing the engagement with 'archive' and historical epistemology, the last three chapters investigate the construction and negotiation of value(s) in and around the brain collection. 'Value' is shown in this case to be 'archival', because valuation hinges on both historiographical narrative and material viability.

Overall, the thesis concludes that while they may seem like distinct and incommensurable concerns, the histories of ethical and epistemological engagements with the brain collection have been intertwined and practically inseparable for involved actors, even when a historians' conceptual distinction is necessary to show their co-dependence. Thus, my empirical work has been divided between sources considered either 'bioethical' or 'scientific' (broadly construed as cultural phenomena) while the theoretical and analytical employment of 'archive' allows for a reintegration of the two.

## Dansk sammenfatning

I årene 1945-1982 indsamlede Hjernepatologisk Institut i Danmark 9.479 hjerner uden samtykke fra psykiatriske patienter, der døde under indlæggelse på psykiatriske hospitaler. Hjerneerne bruges stadig i forskning. Disse hjerner, og deres historie, er emnet for denne ph.d.-afhandling. I syv kapitler følger jeg hjerneerne i forskellige kontekster, fra de tekno-videnskabelige forestillinger omkring samlingens oprettelse, over hverdagslige praksisser forbundet med indsamling og opbevaring, samt forskningsbrug, til offentlige bioetiske debatter og fiktive fremstillinger i dansk populærkultur.

Tid og stof – temporalitet og materialitet – er centrale begreber i mit forskningsarbejde. I nærværende tekst er de forbundet i et begreb om *arkivet*, som det blev udviklet i en bog fra 2017 af Lorraine Daston m.fl. Begrebet *arkiv* beforder en tænkning af stoffets temporalitet og tidens materialiseringer, som på én gang beriger, komplicerer og forklarer begge begreber. Arkivet består af stoflige genstande, hvis natur og betydning er bestemt af deres forhold til for- og fremtiden. Arkivet selv er meningsløst uden tidslighed men ubrugeligt uden stofligt indhold. Disse grundlæggende indsigter bidrager indgående til en forståelse af den danske hjernesamling.

Afhandlingen behandler på historisk vis henholdsvis epistemologi og etik i hjernesamlingen. I afhandlingens første halvdel kombinerer jeg *arkivet* med Hans-Jörg Rheinberger's historisk-epistemologiske arbejde, der fremhæver livsvidenskaberne som et område, hvorpå stof og teori, samt eksperiment og hypotese, altid er samskabende og indbyrdes afhængige. Ved en sådan "det

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konkretes epistemologi”, tillige med en øget årvågenhed for dens arkiviske komponenter, viser jeg hjernesamlingen som en dynamisk og uforudsigelig grund for vidensproduktion.

Afhandlingens anden halvdel følger hjernesamlingens indføring i bioetikken. Siden 1990'erne er hjernesamlingen blevet tilskrevet en overflod af værdier; det være sig etiske, monetære, videnskabelige, historiske. Samlingen er blevet værdisat på højest forskelligartede skalaer, og efter uforenelige principper. Idet jeg fortsat beskæftiger mig med *arkivet* og den historiske epistemologi, undersøger afhandlingens sidste tre kapitler altså konstruktionen og forhandlingen af værdi(er) i og omkring hjernesamlingen. 'Værdi' viser sig i denne forbindelse at være 'arkivalisk', fordi værdisættelse beror på både historisk narrativer og stofflige anvendeligheder.

Overordnet konkluderer afhandlingen at selvom de ved første blik synes distinkte og uforenelige, er etikkens og epistemologiens historier sammenvævede og praktisk uadskillelige i hjernesamlingens tilfælde. Dette gælder for såvel de historiske aktører, som for historikeren selv, der imidlertid må opretholde en heuristisk skelnen for at vise de to områders indbyrdes afhængighed. Idet mit empiriske arbejde derfor har været delt mellem enten 'etiske' og 'videnskabelige' kilder (begge forstået som en del af den omgivende kultur), tillader min teoretiske brug af *arkivet* således en gradvis sammensmeltning af disse to kategorier.