Regarding the brain

De Rijcke, S.

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Regarding the Brain

Practices of Objectivity in Cerebral Imaging

Seventeenth Century - Present

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RIJKSUNIVERSITEIT GRONINGEN

Regarding the Brain

Practices of Objectivity in Cerebral Imaging

17th Century - Present

Proefschrift

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Promotores:	Prof. dr. D. Draaisma
	Prof. dr. G.C.G. Dehue
Copromotor:	Dr. A. Beaulieu
Beoordelingscommissie:	Prof. dr. J.F.T.M. van Dijck
	Prof. dr. J. Elkins
	Prof. dr. F. Vidal
	Prof. dr. R. Zwijnenberg

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Contents

On Brain Imaging

On March 18, 2005, doctors in Florida removed 41-year old Terri Schiavo's feeding tube, after she had been hospitalized for fifteen years. Schiavo had suffered from cardiac arrest in 1990 - most likely the result of an eating disorder. While she survived the cardiac arrest, her brain had been deprived of oxygen for five minutes. This had resulted in neurological damage. Schiavo was able to breathe independently, but that was all she could do on her own. Her chances of recovery were unclear. Was she in a permanent vegetative state, without any hope of improvement, or were there signs of, albeit minimal, consciousness? Terri's husband Michael was assigned as her guardian.

Eight years after the incident, and much to her parents' regret, Michael offered a petition to the court. He wanted to have his wife's feeding tube removed. Michael had hired two doctors to establish his wife's prospects. On the basis of brain scans and a physical examination, they had declared that she would never recover from the brain damage. A third doctor, appointed by the court, came to the same conclusion. Michael told a journalist that he tried to honor Terri's wishes by not keeping her alive artificially (LaMendola, 2005). What followed was a long legal battle with Terri's parents, played out in the media, a battle that lasted until 2005.

Visual images dominated the discussion on the possible termination of Terri Schiavo's life. The video in which Terri is embraced by her mother, and seemingly reacts with a smile (a conscious act), quickly traveled across the web. Many believed, with Terri's parents, that removing the feeding tube equaled murder. People expressed their concerns in various ways, for instance via YouTube.[1] But the tone of the debate changed drastically when the media got hold of Terri's computed tomography (CT) scans. The number of witnesses of her severe neurological state exploded. Local newspaper *South Florida Sun–Sentinel* asked three neurologists to react to the scans.[2] Professor Leon Prockop, neurologist at the University of South-Florida, stated that he had never before seen such severe injuries. The other two neurologists estimated that Terri's chances of recovery were close to zero (LaMendola, 2005). Some researchers even used the scans in public lectures in order settle the debate. University of Pennsylvania bioethicist Paul Root Wolpe, for instance, told *Nature* that he regularly juxtaposed Terri Schiavo's CT scans with the scans of a healthy person (see figure 1). According to Wolpe, this paired presentation clearly revealed that 70 to 90 % of her brain cells had died.[3] The shocking visual evidence, Wolpe said, helped the audience realize that Schiavo was indeed really brain dead (Check, 2005).[4]

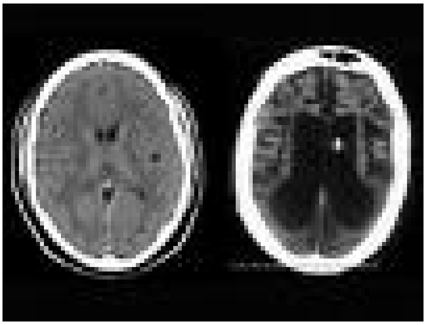

Figure 1 Paired presentation of a healthy subject's CT-scan (left) and Terri Schiavo's CT scan (right). Published on May 19, 2005 in *Nature, 435*, 254-255.

Since the late 1970s, the use in biomedicine of computed tomography scans and related technologies such as magnetic resonance imaging and positron emission tomography, has increased dramatically (Blume, 1992; Kevles, 1997). The debate around Terri Schiavo's neurological state already revealed that people are particularly drawn to brain images. But what exactly makes these images so convincing? In *Magnetic Appeal. MRI and the Myth of Transparency* (Cornell University Press, 2008), sociologist of science Kelly Joyce argues that the authority we ascribe to brain scans is a direct result of an exceedingly visually oriented culture. According to Joyce, their explanatory power can in part be attributed to a broader "sociotechnical turn towards visualization." (p. 6) She ascribes the current proliferation of biomedical images to a growing tendency to visually represent ourselves and our personal lives, and to regard the results as authoritative sources of information.

As part of a flourishing field of Science and Technology Studies (STS) on the social, cultural, and technological implications of neuroscientific visualization (Alac, 2004, 2008; Beaulieu, 2001, 2002a, 2002b, 2003; Cohn, 2004; Dumit, 2004; Prasad, 2007; Roepstorff, 2004), *Magnetic Appeal* is a compelling account of how cultural beliefs that link sight and knowledge contribute to the value we ascribe to brain images. Brain scans are surrounded with an aura of precision, of cutting-edge science. As visual icons, they proclaim new developments for an ever wider audience. Books in this trend have been appearing every few years, from the beginning of the 1990s onwards. Among the first was *Images of Mind,* sponsored by *Scientific American* in 1994 (Posner & Raichle, 1997; Raichle, 1994), followed by *Mapping the Mind* (Carter, 1998), *Phantoms in the Brain* (Ramachandran & Blakeslee, 1998), and *The Executive Brain* (Goldberg, 2001), to name but the most popular (De Rijcke & Beaulieu, 2007).

Today, the most common trope used to legitimize brain scans is to regard them as transparent, mechanically objective snapshots of the real.[5] As is the case with nearly all scientific images, most of us would not even see them as images, but as the world itself (Latour, 2002). The visual appeal of the brain scans, their quantitative underpinnings, and the prestige attached to the large, expensive machines that produce them, all play important parts in the assumption that the scans offer unmediated, objective access to our inner anatomy. However, as will become apparent in this dissertation, brain images do not *represent* reality. They *shape* the brains they are said to represent.

Scientific images are expressions of cultural and historical dynamics (Norton Wise, 2006). Intriguingly, images are markedly absent from some scientific fields, while they are very prominent in others. There are also historical variations in their frequency of use. Using images for demonstration is very much an historical phenomenon, which can be pinpointed to Renaissance developments. Most importantly, Renaissance mapping practices created highly specific ways of visually ordering space. As we will see, the use of scale, perspective, and other projection techniques had a major impact on Western brain imaging practices.

A mixture of different technologies and conventions has since then shaped neuroscientific visualization. Some of the best known examples include the camera obscura, the microscope, the photographic camera, the EEG, and of course the brain scanner. Traditional history of science often equals emerging techno-scientific developments with progress (cf. Clarke & Dewhurst, 1972). In this tradition, present-day brain images are routinely regarded as more objective than for instance early twentieth-century photographs, nineteenth century lithographs, or seventeenth century drawings and engravings of the brain. But the material, 'performative' stance I will adopt, assumes that objectivity is an outcome of certain empirical scholarly practices - not an *a priori*, transcendental category (Barad, 2007). Grounded in historical epistemology, I examine four case studies in the history of neuroscientific visualization, while taking into account relevant technical, social, intellectual and cultural factors. Historical epistemology investigates the emergence and development of fundamental concepts of scientific rationality, "by simultaneously attending to both abstract ideas (e.g., philosophical discourses about evidence) and concrete practices (e.g., how scientific images are made and used)."[6] One of my goals is to demonstrate that contemporaneous understandings of objectivity (cf. Daston, 1991, 1992; Daston & Galison, 1992, 2007) profoundly shaped certain ways of knowing, cutting, dissecting, preserving, and visualizing the brain. Following Lynch (1990, p. 153-4), one of my premises will be that scientific images are not merely pictorial images for scholarly texts, but are crucial to the way objects are disclosed and made analyzable. I will regard brain images as intersections of knowledge production and dissemination, as meeting points of representational and technological conventions, cultural customs, and individual experiences.

Although the greater part of the dissertation is devoted to historical neuroscientific visualization, I will also address contemporary brain imaging practices. The focus will be on how certain brain scanning technologies become incorporated into the material culture of research. I will concentrate on recent attempts to visualize living human white matter, the whitish connecting nerve tissue underneath the grey surface of the cortex. My way into these visualization practices is the first

MRI Human White Matter Atlas, published in 2006 by Susumu Mori and his colleagues at Johns Hopkins University. The connection between a renewed theoretical interest in white matter organization and the introduction of a new MRI parameter (diffusion-weighted imaging), results in images that do not seem to fit prevailing notions of realism or objectivity. After two decades of 'photorealistic' magnetic resonance imaging, and a much longer tradition of a mechanical objectivity (Daston & Galison, 1992, 2007), how should we understand these digital, interactive diffusion-weighted images of white matter? The analysis aims to unveil how new modes of seeing and novel mechanical technologies co-evolve with adjustments in particular epistemic approaches.

But before delving into present-day brain imaging practices, we go back to the period in which mechanical apparatus first came to occupy a privileged epistemic space. In 1873, the first photographic atlas of the nervous system was published by the French neurologist Jules-Bernard Luys. I will investigate the introduction of the camera in macroscopic neuroanatomy, with a specific focus on how the new technology got embedded in existing visualization practices. The analysis contributes to a periodization of when certain technologies held sway in neuroanatomy. But the main purpose is to find out whether and how photography was taken up by contemporary neuroanatomists, considering the commonly assumed place of photography as iconic technology of mechanical objectivity.

Subsequently, the focus moves to Spain, late 1880s. I will explore Santiago Ramón y Cajal's drawings and photographs of neurons, the development of neurohistological research and the emergence of influential staining techniques in Spain and abroad. How did the neuron gain reality in the years after Cajal became acquainted with Camillo Golgi's silver impregnation method? How, in order to make room for the neuron, did an existing network of embodied knowledge, skills, instruments, slides, staining techniques, images, people, et cetera, change? In addition, I will pay specific attention to the use of drawings by Cajal and his colleagues to organize their thoughts, structure their observations, and develop an expert eye. The analysis will also include the role of drawings and photographs of nerve cells in the communication of neuroanatomical findings.

In the days of Jules-Bernard Luys and Santiago Ramón y Cajal, studying the material structure of the nervous system was part and parcel of neuroscientific research. But there was a time when attention to structure was not yet taken for granted. The copper engravings of Thomas Willis's *Cerebri Anatome* will allow us to zoom in on visually constituted knowledge of the brain in seventeenth century England. Published in 1664, the *Cerebri Anatome* was one of the first books to address the brain's solid, material tissue, and to presume that these structural constituents were relevant. The book continued to be published in Latin and other languages for over a century after it first appeared. Nowadays, Willis is still celebrated for his extraordinary brain images. One of the engravings of the brain as seen from below still figures prominently in medical handbooks (see figure 11). I will first compare Willis's engravings to earlier, Renaissance efforts to visualize the brain. I will also review the practical, material and social skills that were required to make the images. The primary purpose, however, is to investigate the role of the images in the construction of authoritative knowledge, in order to make evident how the brain first took shape as a credible object of study.

What binds the images of these four case studies, produced in such different periods, and at such different places? What is the connection between Willis's engravings of cortical structure and Luys' first neuroanatomical photographs, Cajal's drawings of discrete nerve cells, or Mori's interactive white matter MRI's? All images discussed in this dissertation were selected because they were made and used in periods of transition. These periods were characterized either by shifts in theoretical orientation, changes in the development of new technologies for research, dissection or visualization, and/or transitions in technologies of measurement and observation. Hypothetically, established stances on what researchers consider as realistic neuroscientific visualization are brought to the surface in these transition periods. Although in practice things will turn out to be messier, the images involved all helped shape new practices of objectivity, and altered dominant ‘realistic’ modes in neuroscience. They were directly involved in the articulation of new neuroscientific facts. Therefore, these images offer a sharp view of the factors involved in changing ways of knowing the brain.

Let us now go back to the seventeenth century. The first chapter begins on a cloudy day in Oxford, in 1666[1].

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[1] A pdf of this thesis, with color images, can be found at sarahderijcke.nl. This is especially relevant for chapter 4, in which the color of the images is part of the argument.

CHAPTER 1

The Mind's Kingly Palace.[7]

Unveiling the Brain in Seventeenth Century England

The Lightning Incident

On May 10th 1666, a heavy thunderstorm caused a tragedy on the Medley River near Oxford. Two Wadham College students, who had been in a boat without a waterman, were struck by lightning on their way home. Just after they pushed their boat off the riverbank, a thunderbolt mercilessly launched them into the cold water. One student was dead on impact. The other got stuck in the mud with his feet down and his head just above the water. Apart from a severe shock he wasn't hurt.

Two days later, Dr. John Wallis described the thunder and its fatal outcome in a long letter. He had heard the first rumble around four o' clock in the afternoon, while he was at home. An hour later the roar had turned into a forceful storm: heavy rain was hitting Wallis's windows hard, and the thunderclap rapidly grew louder. While looking outside, Wallis had taken hold of his watch and had started calculating: "The thunder, for the most part, began to be heard about 8 or 10 second minutes after the flash (as I observed for a great part of the time by my minute-watch) but once or twice I observed it to follow (in a manner) immediately upon it, as it were in the same moment: and the lightening was extremely red and fiery."[8] As Wallis was registering the thunder he felt increasingly uncomfortable: "I do not use to be so much apprehensive of thunder and lightning, but I was at this time (I know not well why) very apprehensive (more than ordinary) of mischief done by it, for it seemed to me to be very low and near us." His uneasiness was worsened by a "stinking sulphurous smell in the air."

The storm raged over Oxford for nearly three hours. Just before it ended, Wallis was brought the news of the sad boat accident on the Medley River, only two miles from his home. He immediately took off to the riverbanks. Once there, he had a chance to talk to some of the people who had sat in nearby boats "about 10 or 20 yards from these." A couple of them had leapt into the water straight after the clash, to rescue the two unfortunate men. They had taken the dead student's body out of the water only minutes after he had been thrown in, and had successfully freed the other student from the mud. One of the bystanders had smelled the same strong stench Wallis had detected indoors. "[W]hen I asked him (…) what kind of stink? He said, like such a smell as is upon the striking of flints together."

Early the next morning, after an unsuccessful attempt to revive the dead student by "putting [him] into a warm bed, and rubbing, and putting strong waters into his mouth," the body was brought to town. Shortly thereafter a large crowd gathered "to view the corps." Wallis and three of his colleagues, Dr. Thomas Willis, Dr. Thomas Millington and Dr. Richard Lower, were also on the spot.

Of the four doctors present, John Wallis was not the anatomical expert - Thomas Willis was. Willis, Lower and Millington had worked together on many other dissections. Under the headship of Willis, they had since the mid-1650's been venturing "to unlock the secret places of Man's Mind,"[9] which they believed to be situated in the brain, or more precisely, in the cortex. In 1664, their cooperation had led to the publication of the *Cerebri Anatome*, a beautifully illustrated monograph on the human brain and nervous system. At the time, it was not uncommon to refer to the cortex as "a cake of suet or a bowl of curds." (Henry, 1989, p. 101) The *Cerebri Anatome* will be the main focus of this chapter. The monograph contained many extraordinary engravings of the nervous system. One of the images of the brain as seen from below (see figure 11) still figures prominently in contemporary medical handbooks. Later on in this chapter, I will explore the processes of objectification behind the book, and will describe more fully the epistemological framework the engravings were part of – a framework also coming into view in Dr. John Wallis's meticulous account of the accident on the Medley River, and the autopsy that followed.

Judging from Wallis's letter, the body must have made quite a display: "On the right side of the neck was a little blackish spot about an inch long and about a quarter of an inch broad at the broadest; and looked as if it had been seared with a hot iron. (…) Straight down the breast, but towards the left side of it, was a large place about three quarters of a foot in length, and about two inches in breadth (in some places more, in some less), which was burnt and hard, like leather burnt with fire, of a deep blackish red colour, not much unlike the scorched skin of a roasted pig."

Fascinated with the slightest detail, Wallis, after having examined the student's neck and breast, went on to his doublet (a hip-length, close-fitting jacket): "The buttons (…) were most of them off, which some thought might have been torn off with the blast getting in at the neck and then bursting its way out. But I can say nothing peremptory of this because I could not upon the view satisfy myself whether they were thus torn off or might have been wanting before; the greatest presumption was (to me) that (besides 4 or 5 buttons wanting towards the bottom of the breast) there were about half a dozen together clear off from the bottom of the Collar downwards."

That night Willis, Lower, Millington and Wallis, accompanied by "some chirurgeons" and a multitude of others, were present at "the opening of the head, to see if any thing could be there discovered." The body was still lying in the same room as in the morning, where it had been continuously watched over by curious people. At this time however, the corpse had very much swelled because of the warm weather. It also smelt hideously. The small room was still packed, although the candlelight must have seriously prohibited sight. All in all, the autopsy could not have been an easy job.[10]

While two chirurgeons held the head still, a third started sawing off the skull. After a small interruption - one of them had felt the skull jarring in his hands, so there was a brief search for possible cracks - the doctors stepped in to inspect the brain that now laid bare in the man's cranium. Because of "the body being to be buried by and by," the doctors knew they would not have enough time to perform a thorough observation. Due to the rarity of the occasion they were prepared to sacrifice some of their usual attention to detail. Wallis stressed in his letter that the doctors surely did not overlook anything that was considerably wrong. According to him there was “no sign of contusion, the brain full and in good order, the nerve whole and sound, the vessels of it were pretty full of blood.”

Obviously Wallis’s letter was written in a very detached and at some points mathematically precise manner. This, however, could not conceal the atrociousness of the accident on the Medley River. Not only was the student unfortunate enough to get killed by a stroke of lightning, but his mutilated body was also exposed for everyone to see. To top it all off, he was cut open by the very people he had only days before been greeting respectfully at the galleries of Wadham College. What a price to pay for scientific progress.

Unfolding Wallis’s letter

Dr. John Wallis’s lengthy letter on the lightning accident was addressed to “Mr. Henry Oldenburgh Esqr,” secretary of the only recently founded Royal Society.[11] Wallis, Savilian Professor of Geometry in Oxford, was one of the society’s most prominent founding Fellows. Accordingly, his letter was much more than an extensive account of a macabre accident, it was part of a scientific discourse.

Judging from his letter, Wallis was well aware of the uniqueness of his testimony. The autopsy was performed and witnessed by four prominent members of the Royal Society. As stated by Thomas Sprat in his *History of the Royal Society* (London, 1667), the Fellows favored their ‘own touch and sight’ above hearsay or second-hand experience. Their reports served to underline firsthand observations and experiences, and were typically formulated in the first-person active voice (Shapiro, 2000). Wallis, a meticulous mathematician and one of the few to possess a ‘minute-watch’, was the perfect man to put the lightning accident and the subsequent autopsy in writing: the exactness of his report corresponded seamlessly with the ‘New Experimental Philosophy’ the Royal Society adhered to.

The new experimental philosophy slowly gained momentum in England after it had been introduced on the Continent in the mid-1500s. According to Wallis, it had “from the times of Galileo at Florence, and Sir Francis Bacon (Lord Verulam) in England, (…) been much cultivated in Italy, France, Germany, and other parts abroad, as well as with us in England.” (Colby 1913, p. 196-199) Baconian natural philosophy was labeled ‘new,’ to distinguish it from the alleged generalizations and explicitly excluded anomalies of conventional scholastic natural philosophy. Bacon’s efforts to supplant Scholasticism with a natural philosophy based on firsthand observation and experiment had a major impact on subsequent generations of English natural philosophers.

Most ‘new’ natural philosophers agreed, after Bacon, that their natural philosophy was to be grounded in natural history, a collection of well-established ‘matters of fact’ in topography, geology, climatology, botany, zoology, anatomy, chemistry, medicine, and political economy, a catalogue as it were of the commonalities, but also the deviations in nature (see figure 3) (Jardine, 1974; Shapiro, 2000). Baconian natural history was to offer a clear break with its Renaissance predecessor, characterized by literary, mythical, and symbolic elements (Daston, 1994). Bacon considered all natural objects, incidents, and experimental results eligible as ‘matters of fact’. These matters of fact could subsequently serve as a foundation for the new natural philosophy.

Wallis probably estimated correctly that the lightning accident would be of interest to the other Fellows. Some of them he had known for over twenty years now: in 1645 they had founded several experimental clubs to discuss New Philosophy on a weekly basis. This Invisible College, as it was referred to at the time, was one of the precursors of the Royal Society. Although physics had initially dominated their agenda, chemistry, medicine and anatomy increasingly won their attention. This increase was largely due to the work of William Harvey, who had made a clear case in the 1640s for the ‘anatomic method’.

Harvey owed his discovery of the circulation of the blood and of the role of the heart to a combination of pathological findings and clinical correlations. Though solidly Aristotelian in his worldview, Harvey's physiological approach was based on visual inspection and dissection. This was perceived as highly innovative by many young natural philosophers. Harvey's theories and approach stimulated further investigation in the 1650s, '60s and '70s, when his followers started making the anatomical method central to their claims.[12]

Figure 2 Frontispiece *Cerebri Anatome* (Willis, 1665).

Eight Accurate Brains

The Remaining Medical Works of that Famous and Renowned Physician Dr. Thomas Willis, published in 1681, contained the first English translation of the *Cerebri Anatome*, subsequently referred to as *The Anatomy of the Brain and Nerves*. The word "anatome" or "anatomy" in the title does not refer to anatomy as in structure, but to anatomy in the second meaning of the word: examination, analysis, investigation.

Together with *The Pathology of the Nervous Stock* (1667), *The Animal Soul* (1670) *Rational Therapeutics* (1675) and *Two Discourses concerning The Soul of Brutes* (1683), *The Anatomy of the Brain and Nerves* was one of the first thorough studies on the brain and nervous system in Europe. The book was an instant success:

in the first year alone, four reprints were published (Zimmer, 2004). It continued to be published in Latin and other languages throughout the eighteenth century. Its title page proudly announced that the book contained "Eighteen Copper Plates." Eight of them depicted the brain and skull.

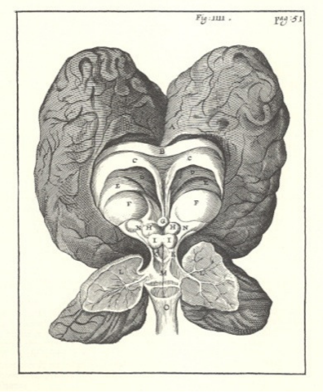

Figure 3 A 'deviation' in nature in *The Anatomy of the Brain and Nerves* (1681, p. 51): "The Effigies of a human brain of a certain Youth that was foolish from his birth, and of that sort which are commonly termed Changelings."[13]

In the preface Willis graciously acknowledges the help of two of the "most famous Men" we already encountered above: Thomas Millington, who served as a partner in discussions on the purpose of various brain parts, and Richard Lower, who added his skills as an anatomist. All engravings in *The Anatomy of the Brain and Nerves* were made by Dr. Christopher Wren, the third colleague Willis mentions in the preface. Wren, a natural philosopher with a keen eye for architecture, would not much later be asked to devise St. Paul's cathedral in London, after the Great Fire of September 1666. According to Willis, Wren was "pleased out of his singular humanity, wherewith he abounds, to delineate with his own most skilful hands many Figures of the Brain and Skull, whereby the work might be more exact."[14]

Although Willis's tribute to Christopher Wren might strike a twenty-first century audience as slightly excessive, it is very telling of the culture of gentlemen Willis and Wren were part of (cf. Shapin, 1994). But more importantly, the quote reveals the value ascribed to being exact. John Wallis's report to Henry Oldenburg already brought to light, both through writing style and through the description of the dissection procedure, that accuracy was a crucial item on the agenda of a seventeenth century natural philosopher. In the well-documented *Lecture on the Anatomy of the Brain*, delivered in 1665 and published four years later, the Danish natural philosopher Niels Stensen spent a complete section on “Good diagrams and bad.” Stensen acknowledged that “[t]he best diagrams of the brain that we have to date are those given to us by M. Willis,” but he continued by saying that “errors creep in here and there, and many items would have to be added to make them perfect.”(Stensen, 1669) Willis however continuously stressed the accuracy of *The Anatomy of the Brain and Nerves*, and of the engravings by “Dr. Chr. Wren Doctor of Laws.” He put much effort in presenting the engravings as realistic accounts of what can be seen while opening a head at a necropsy. In his own words, the figures “truly represented” what can be “found in a Man.”[15]

What did Willis mean when he referred to ‘truthful representation’? In the case of scientific images, more specifically atlases, historians of science Lorraine Daston and Peter Galison argued that a new type of scientific objectivity came into being in the nineteenth and early twentieth century (Daston & Galison 1992; 2007). In this period, scientific authority came to stand for a detached, mechanical utilitarianism that replaced other ideals, characterized by the authors as the principle of ‘truth to nature’. Truth to nature, the authors argue, should not be equated with contemporary notions of objectivity, not in the least because they contend that contemporary notions are rather confused. Daston in particular pointed out that today, objectivity can at once refer to methodological, moral and metaphysical claims, as in being scientific, factual, impartial, rational or “really real.” (Daston 1992, p. 597)

Natural philosophers sowed the seeds of the precept of truth to nature in the Renaissance. From then on, references to metaphysical explanations and echoes of ancient authorities were increasingly omitted from natural philosophical treatises

(Martensen, 2004). However, as indicated by Daston and Galison, the concept of truth to nature allowed for multiple readings.

To explore Willis's epistemological standpoints more carefully, I will investigate his engravings, along with the images of some of his predecessors and contemporaries. I will do so by drawing on Svetlana Alpers' distinction between *northern* and *southern* images, and on Barbara Shapiro's *culture of fact* and *legal witnessing* (Alpers, 1983; Shapiro, 2000). But let us first take a closer look at the scholarly landscape Willis operated in.

The Cell Doctrine

In 1660 - the year that Charles II was invited to the throne after two violent decades of Civil Wars - Thomas Willis was appointed Sedlian Professor of Natural Philosophy in Oxford. Willis owed his appointment to Gilbert Sheldon, the Archbishop of Canterbury, who from the Restoration onwards grew to have a sizeable influence on university affairs.

According to the All Souls College university statutes, Willis's task as a Professor was to propagate Aristotelian Scholasticism in two weekly lectures, to an audience of bachelors of arts and auditors in astronomy (Dewhurst, 1982). Until well into the seventeenth century, European university-based natural philosophy had identified the Greek philosopher and biologist Aristotle as their key authority. Scholasticism was a mixture between Aristotle's work and the Christian doctrine. It became known as the official philosophy of the Roman Catholic Church.

Willis, a pious catholic, did not unequivocally adhere to Scholasticism. Aristotle had held the view that the heart was the central organ of the body. According to him, the brain was an important structure, but merely as a cooling edifice for the heart's heat. By serving as a 'refrigerator' for the heat originating from the blood, the brain provided the necessary counterbalance.

Besides the work of Aristotle, the study of anatomy in medical natural philosophy relied on other Greek texts, above all Galen's. For over a thousand years, the Greco-Roman physician and philosopher Galen (130-200) had been a prime figure in medicine as a whole, and the study of anatomy in particular (Martensen, 2004). Galen's original Greek texts had disappeared from view after they were published. They were rediscovered and translated by a group of dedicated humanists as late as the sixteenth century.[16] From then on, early modern anatomists revived classical sources and continued this line of empirical research (Cunningham, 1997). But the revival also led to a gradual recognition of inconsistencies between their own observations and the key ancient models created by Galen and others. Nevertheless, the discrepancies were not discovered over night.

In Galenism, animals and humans alike were thought to have 'humors' that travelled through their bodies and assembled in a few vital organs. According to Burton (1651, p. 97), the four humors were blood, "a hot, sweet, temperate humor whose office is to nourish the whole body, to give it strength and colour;" phlegm or "pituita, a cold and moist humor, his office is to nourish and moisten the members of the body;" yellow bile or "choler, hot, dry, bitter, helps the natural heat and senses, and serves to the expelling of excrements;" and black bile, or "melancholy, cold, dry, thick, black, and sour."

The four humors and their corresponding temperaments – sanguine, phlegmatic, choleric and melancholic – were part of a four-part cosmology of the elements, seasons, winds and the 'four ages of man' (Kemp, 1998); they were thought to mirror the larger unities in God's universe. In order to stay healthy physically and mentally, the humors needed to be properly balanced.[17] If they became unbalanced, they could for example lead to epilepsy, or even to madness. An important part of a physician's job was to cure mental disturbances by bringing the humors back in balance, usually by bleeding or purging (Zimmer, 2004b).

Galen's humoral model emphasized the body's hollow spaces over solid tissues, because they were said to serve as 'containers' for the humors. The heart, liver, spleen and ventricles of the brain were the most important containers. According to Galen, the brain's ventricles had a second function: he claimed they served as the site of storage of the *spiritus animalis*.[18]

The animal spirits were considered to be an ethereal life force. They were the refined outcome of a conversion from the vital spirits that were produced in the left ventricle of the heart, after which they were distributed via the blood throughout the body. According to Galen, the animal spirits moved freely through the ventricles and the nerves, which he thought were hollow. The animal spirits were responsible for perception, thought and motor skills. Galen believed that the conversion to animal spirits took place in the *rete mirabile*. Having never performed dissections on humans, he assumed that this network of blood vessels he had discovered at the base of the brain of large ungulates and carnivores, existed in man as well (Clarke & Dewhurst, 1972).

Galen laid the groundwork for the localization of mental processes in the ventricles of the brain. The theory was further adjusted from the fifth century onwards by Galen's Christian and Persian successors, and transformed into what is now known as the Cell Doctrine (ibid). The following small excerpt from Robert Burton's *The Anatomy of Melancholy*, published in London in 1651, furnishes proof of the prominence of the doctrine in seventeenth century England:

"The brain itself is divided into two parts, the anterior and posterior part; the anterior part is much bigger than the other, which is called the little brain in respect of it. This anterior part has many concavities distinguished by certain ventricles, which are the receptacles of the spirits, flowing from the arteries of the heart, and are there refined to a more heavenly nature, to perform the actions of the soul. Of these ventricles there are three - right, left, and middle. The right and left answer to their site and beget animal spirits; if they are in any way damaged, sense and motion ceases. These ventricles, moreover, are held to be the seat of the common sense. The middle ventricle is a common concourse and cavity of them both, and has two passages - the one to receive pituita, and the other extends itself to the fourth creek; in this they place imagination and cogitation, and so the three ventricles of the fore part of the brain are used. The fourth creek behind the head is common to the cerebral or little brain, and marrow of the back bone, the last and most solid of all the rest, which receives the animal spirits from the other ventricles, and conveys them to the marrow in the back, and is the place where they say the memory is seated."[19]

Conquest of Naturalism?

Thomas Willis and other educated people in the 1600's who were interested to learn about the brain, would have encountered two types of images in books and other treatises on the natural philosophy of the human body. In his articulate *The Brain Takes Shape* (Oxford, 2004) Robert L. Martensen separates the images that emphasize the solid cortical tissues from the images originating of a more spiritual approach to the body. An example of the latter category are the images of the Cell Doctrine, discussed above (see figures 4 and 5). To proponents of the Cell Doctrine, spirit and matter were not separated; consequentially, they were not necessarily concerned with providing mimetic representations of anatomical structure. Andreas Vesalius serves well as representative of the former category. It is usually argued that he wrote history as the instigator of the "dawn of accurate and representational scientific illustration;"[20] Thomas Willis allegedly followed in his wake.

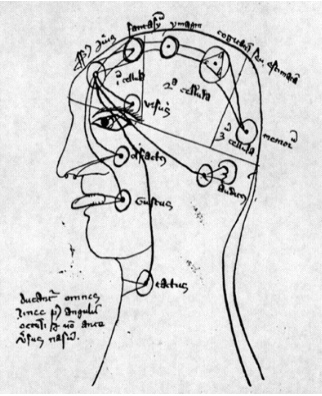

Figure 4 Avicenna's version of the Cell Doctrine resulted in five cells: the first two represent the anterior ventricle, the second two the middle ventricle and the last the posterior ventricle. Around the head is written "sensus communis, fantasia, ymaginativa, cogitiva seu estimativa, memorativa." (Clarke & Dewhurst 1972, p. 30)

Martensen distinguishes an early modern shift in natural philosophical books and treatises on the human body from a spiritual or holistic approach, to a desire for similitude. But, assuming that this shift indeed took place, it did not entail a substitution of schematization for naturalism, nor did it simultaneously cause a move from inaccuracy to exactness. The recurrent suggestion that from the Renaissance on imperfect schematization was substituted for a more objective naturalism[21] is not feasible. It springs from the conceptual armamentarium that judges visual images by a uni-dimensional definition of functionality.

In 1543 Vesalius published a spectacular folio of the human anatomy, *De Humani Corporis Fabrica*, and its *Epitome* or ‘appendix’. The *Fabrica* is habitually recognized as the first exhaustive atlas of the human anatomy. Its elegant wood engravings have achieved the status of icons of the renaissance in science. Their entrance in the world of medicine instantly pulled old-established images into the realm of the schematic, the inaccurate, the non-functional, the caricature.

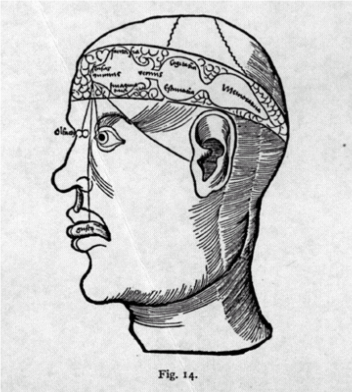

Figure 5 Cell doctrine (Reisch 1504)[22]

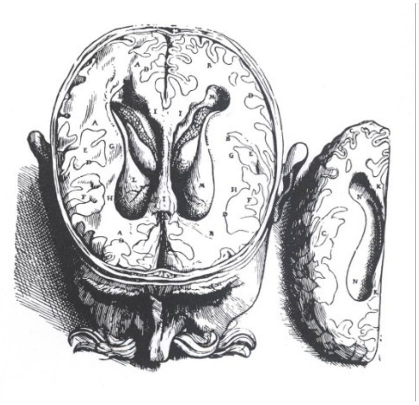

Figure 6 Human brain in Vesalius (1543)

Vesalius himself nurtured the impression that his thirteen beautiful woodcuts of a brain in successive stages of dissection were a fundamental conquest over those of the proponents of the Cell Doctrine. Writing on the subject of Reisch’s image, for instance (see figure 5), he claimed that “[s]uch are the inventions of those who never look into our maker’s ingenuity in the building of the human body.” (Gross, 1992, p. 38; Singer, 1952, p. 32-40) He believed that medicine was ruined when its various constituents, such as surgery, were separated from it in antiquity and consigned “to laymen and people with no knowledge of the disciplines that go to serve the healing art." (Long, 2002, p. 76) Vesalius, only twenty-eight at the time, was one of the first Renaissance natural philosophers to prefer the anatomizing of the human body over the reading of classical texts.

Evidently, Vesalius wanted to draw attention to the surplus value of his own work. The *Fabrica* was a novelty, yet I will argue that it would be a mistake to see Vesalius's engravings as more functional or accurate than their predecessors.

The Cell Doctrine, as we have seen, was part of Galenism, a systematic combination of ancient and medieval medicine, humoral theory and Galen's own observations. From around the twelfth century onwards, theories about the four humors, their corresponding four temperaments, the umbrella four-part cosmology, and the four ages of man were brought back to life after the rediscovery of Galen's publications. It was a highly conceptual medicine that relied greatly on spiritual faculties. Owing to the fascination for the non-material nature of the soul, not for the potential structural equivalent, proponents of the Cell Doctrine would not have benefited from a more detailed visualization of the structure of the ventricles, for instance.

Furthermore, while at a certain point novel depiction and dissection techniques were readily available, diagrammatic images of the Cell Doctrine kept appearing in medical treatises until well into the nineteenth century. Reisch's drawing (figure 5) was one of the most famous and also most plagiarized depictions of the Cell Doctrine. On many occasions diagrammatic representations were a deliberate choice, not a certificate of insufficient means.

"Nosce te ipsum – know thyself"

An analysis of images of the brain and mind in terms of increasing functionality is problematic, because what is functional is directly proportional to contemporary technological, scientific and artistic developments. For the same reason, an analysis solely in terms of increasing truthfulness to nature would not be satisfactory either. Naturalistic rendering was especially valued by Renaissance natural philosophers and artists alike, because they attached increasing importance to the primacy of sensory knowledge. But what was considered naturalistic was subsequently interpreted differently.

Vesalius's pictorial epistemology in the *De Humani Corporis Fabrica* hinges strongly on the metaphor of the eyewitness. He claimed that "images greatly assist the understanding, for they place more clearly before the eyes what the text, no matter how explicitly, describes." He insisted that his images were particularly of interest to those who were themselves not in a position to dissect bodies, or were too sensitive to do so (Long, 2002).

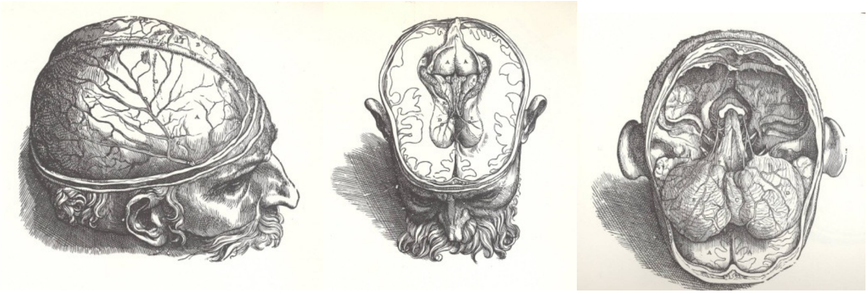

Figure 7 Brain in successive stages of dissection (Vesalius, 1543)

Vesalius aspired to make the reading of the *Fabrica* as much as possible like the experience of a dissection: looking at his wood engravings mimics the experience of standing alongside the anatomist to marvel at the view of a progressively more anatomized skull. The suggestion of an unmediated access to the experience of a dissection is reinforced in the text, in which he gave a detailed account of his dissection procedure. Hence, text and images joined forces as evidence of what can be seen when dissecting a human brain. Nevertheless, given that Vesalius published several horizontal cross-sections of the brain in which the ventricles were drawn with markedly more detail than the brain's convolutions, he implicitly adhered to certain elements of the Cell Doctrine. Even in Vesalius's very naturalistic engravings of the brain, one can clearly see that the ventricles are in the lead, while the cortex was apparently not yet a relevant structure.[23]

In addition, Vesalius's images contain many details that could strike a twenty-first century audience as stylized, or even redundant: the beautiful curly hair of the moustache, the stern nose, and the bone structure of the lifeless man are all unfamiliar features in contemporary brain images. Vesalius's main aim in the *Fabrica*, as revealed in his remark on Reisch's depiction, was to unveil in nature evidence of the magnificence of God's creation. This was a typically humanist philosophical stance towards nature that saw God's manufacture as premeditated to be understood by humans. The ancient Greek dictum 'Nosce te iptum' or 'know thyself' was much cited in the humanist era - man was the measure of all things. Subsequently, throughout the humanist era the "art of persuasion" ruled in communicating great truths (Kemp, 1999, p. 14).

In Vesalius's day, the beauty of the images was seen as an essential part of achieving accuracy (Daston & Galison, 1992). Many figures in the *Fabrica*, most obviously the osteological and myological figures, were more or less explicitly illustrative of the popularity of classicism in Italian art academies (see figure 8).

Figure 8 (Vesalius, 1543)

The stylishness and the rhetoric of presentation were absolutely integral parts of the images in this particular context of communication. Therefore, the audience did not get to see the bloody, anatomized head of the hanged criminal that Vesalius resorted to for his dissections. Instead, they could admire a head that was idealized by design, elegantly cleaned up, and displayed in an ornamental style.

Thomas Willis, like Andreas Vesalius, relied heavily on the assertion that the images in his *The Anatomy of the Brain and Nerves* were accurate. However, their images are poles apart. I will get back to this point shortly. Naturally, the images were different as a result of a century's worth of developments in engraving techniques. Although the copper engravings in Willis's book were slower to print from than Vesalius's wood engravings, and could not be made to produce very large editions, they were more detailed and therefore - technically - of a higher quality (Ivins Jr., 1969). Furthermore, the knowledge of the anatomical structure and function of the brain had advanced as well. But this does not mean that seventeenth century natural philosophers simply offered better, more reliable and accurate images of the brain. This will become apparent when we compare Willis's engravings (see figures 3, 11 and 12) to the images in the treatise of a significant contemporary, René Descartes.

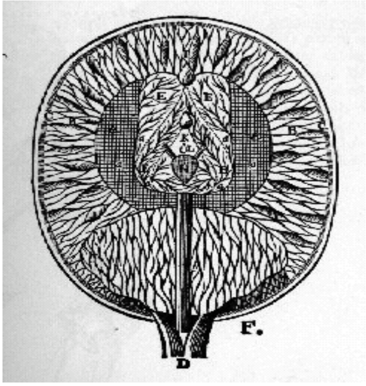

Figure 9 (Descartes, 1664)

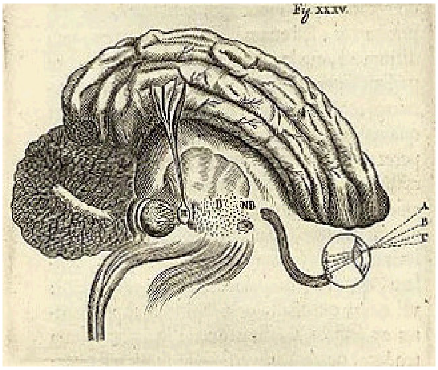

Figure 10 (Descartes, 1664)

Descartes' *De Homine* first appeared in Latin in 1642, followed by a French edition in 1664. It was his contribution to the study of human physiology and anatomy. For clarification purposes, Descartes included several images, varying from highly diagrammatic to naturalistic ones (see figures 9 and 10). In doing so, he capitalized on reader's expectations of the emerging post-Vesalian ethos of the naturalistic rendering of solid structures. He simultaneously aligned his model of the human body to theories that hinged on bodily cavities, on vital and animal spirits. The latter called for more diagrammatic images, which had the upper hand in the *Traité de l'homme*. By seemingly adhering to both approaches, Descartes made room for his own conceptual framework, which had a mechanical disposition.

Partly, Descartes' natural philosophy was very much in keeping with contemporary English 'new' natural philosophy. He did not simply adhere to the wisdom of the ancients. He founded his own natural philosophy on experiments and empirical findings. He regularly paid a visit to the local slaughterhouse in Amsterdam in the late 1620s and early 1630s in search of dissection material. In a letter to his friend Mersenne he aligned himself with Vesalius as one of the 'moderns', and was well-informed in the field of comparative anatomy (Martensen, 2004).

But Descartes was not a proponent of knowledge of anatomical detail. In texts that were intended for publication, like the *Traité de l'homme*, he rejected the importance of anatomic knowledge. Instead of moving from anatomical fact to theory, like the empiricist Willis would do a while later, Descartes seemed more committed to deductive reasoning (ibid). For instance, to bridge the space between the indivisible soul (*res cogitans*) on the one hand and the expansiveness of the physical world (*res extensa*) on the other, he searched for an anatomical structure that could match his theory. Descartes thought the pineal gland, a grayish-red, cone-shaped appendix in the middle of the brain, could serve as a passageway. It appeared on nearly all of his drawings of the brain, marked by the letter H. Simultaneously, he paved the way for a description of the brain in terms of matter that performed actions according to mechanical rules. This was revolutionary: As we have seen, in Descartes' day the brain was the principal embodiment of the body's hollow spaces.

Descartes' framework is beautifully mirrored in his images. See for instance figure 9. The lush, wavy branches around the pineal gland (H) in the centre of the brain (originating in the ventricles EE, "les concavités") and the fiery sea of flames at the outer layer elegantly echo Descartes' metaphors of matter: to explain the course of the animal spirits he used as metaphors very fine breezes and lively, pure flames. With a little imagination one can even see the theory-ladenness in the frame surrounding the ventricles. The frame according to Descartes was a dense net of small hollow tubes through which tiny particles flowed towards the pineal gland, where they were converted to animal spirits for release into the ventricles. Its geometric shape is remotely reminiscent of his devotion to mathematics.[24]

The Dawn of the Cortex

While René Descartes had worked from a general theory of matter to the matter itself, Thomas Willis preferred to start unraveling what he thought were unsolved mysteries regarding the brain's anatomy (Martensen, 2004).

The Anatomy of the Brain and Nerves was Willis's first feat as a professor. Without actually mentioning who he was opposing, the book was meant as a repudiation of what was hitherto known on the brain's structure and function, and on the centre of thought. The book's preface breathes novelty, the treading of unknown territory. In line with the bold rhetoric we also came across in Vesalius's *Fabrica*, Willis provided an account of the endeavors that prompted the book:

> "[A]s I did chiefly inquire into the offices and uses of the brain and its nervous Appendix, I addicted my self [sic] to the opening of Heads especially, and of every kind, and to inspect as much as I was able frequently and seriously the Contents (…).and so a firm and stable Basis might be laid, on which not only a more certain Physiology than I had gained in the Schools, but what I had long thought upon, the Pathologie [sic] of the Brain and nervous Stock, might be built."[25]

Willis and Descartes both believed that the mind was independent of cerebral structures. For a large part this division between immaterial mind and material brain sprang from the fact that theology and natural philosophy were still profoundly intertwined (Martensen 2004; see also Knoeff, 2004).

Willis maintained a concept of twin souls: man possessed both a rational, immortal, divinely created soul and a sensitive, corporeal soul, a “Soul of Brutes.” Humans shared with some animals this corporeal soul, but the rational soul was exclusively human. According to Willis, its role was to transform the sense impressions distributed by the corporeal soul into abstract thoughts.[26]

Amongst other focal points, Willis wanted to establish the primacy of the rational soul, which he located in the cortex, over the corporeal soul. Echoing Harvey’s research of the heart, he emphasized the solid portions of the brain rather than its cavities. Willis considered the brain’s ventricles, vital in Greek and Galenic physiology as well as in the Cartesian model, to be of minor importance. According to him, their existence was merely a result of the complexity of the brain’s enfoldings. In chapter one of *The Anatomy of the Brain and Nerves* he noted almost offhandedly that upon lifting the cerebrum “the three Ventricles, commonly so called, go into one empty space or mere vacuity,” but in fact a major part of the book was reserved for the development of this observation.

Willis was one of the first, if not the first, to see the brain’s ventricles as mere vacuities. He also wanted to do away with Descartes’ notion of the pineal gland *and* prove that the soft grayish cortex was much more than a ‘bowl of curds’. Moreover, he had found that humans, as opposed to large ungulates, lacked a so-called *rete mirabile*, a dense net of arteries and veins at the base of their brains. Instead, he argued that humans had an arterial circle, furnishing the brain with an evenly balanced and steady blood supply. Willis had come to these viewpoints because he had, in the company of anatomists like Robert Boyle and Richard Lower, familiarized himself with three material technologies: the intravenous injection of colored liquids, the possibility of preserving organs in what Boyle had called ‘spirit of wine’ (pure alcohol), and the up to that point relatively untried method of dissecting heads from the base of the brain upwards. The latter two technologies made it possible to perceive the brain as an independent organ.[27]

Willis’s anatomical viewpoints are beautifully mirrored in Christopher Wren’s copper engravings. Given that Wren integrated these points in single images, they served an indispensable role in the setting forth of Willis's views. In Latourian parlance, Wren had assisted Willis in increasing the cost of disagreeing with *The Anatomy of the Brain and Nerves*, because in order to "doubt back" (Latour, 1990, p. 59), potential adversaries would have to write a new book, have it published, and mobilize counter-examples in equally convincing engravings.

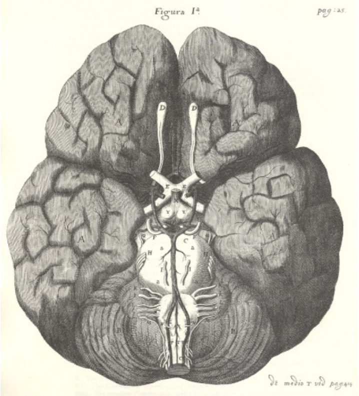

Figure 11 Base of the brain. Also visible in the middle is the circle of veins that was subsequently named after Willis (Willis, 1664)

Northern detachment

There is a second reason why Wren's images were repeatedly reproduced in textbooks long after they were first drawn: they connected to an emergent, typically northern system of pictorial conventions. In *The Art of Describing. Dutch Art in the Seventeenth Century* (London, 1983) Svetlana Alpers distinguishes between the constituents of typically northern, as opposed to more southern seventeenth century paintings, pencil drawings, book images and engravings. She argues that seventeenth century northern image makers added 'actual viewing experience' to the alleged artificiality of the southern perspective system. In northern images, observable facts were captured apparently without human meddling. Strengthened by a growing experience with state-of-the-art lenses (tele- and microscopes for instance), Alpers argues, many northern artists and natural philosophers alike were more or less forced to accept the absence of any fixed proportion or human measure. Often a parallel was made with images created by a camera obscura, a devise that purportedly provided “optical” as opposed to “perspectival” images (ibid., p. 27-30). Images created by the camera obscura, so it was said, registered visual phenomena without human intervention, ‘truly natural’, whilst images that embraced linear perspective were geometrically calculated and were therefore deemed artificial.

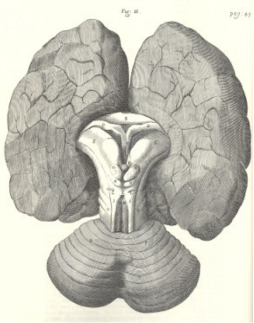

Figure 12 (Willis, 1664)

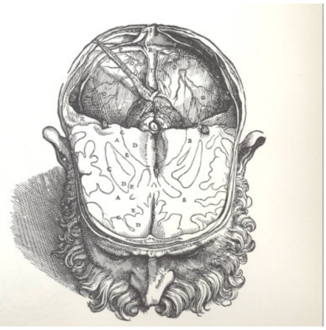

Figure 13 (Vesalius, 1543)

Generally, Svetlana Alpers asserts, northern seventeenth century natural philosophers chose to sacrifice the humanistic privileged, perspectival view that was empowered to summarize knowledge as beautifully as possible. Instead, they called in the “attentive eye,” an eye that claimed to observe nature instrumentally, empirically (p. 73). This, I claim, is precisely what sets the images in *The Anatomy of the Brain and Nerves* apart from Vesalius’s very ‘southern’ wood engravings.[28] Vis-à-vis Vesalius, who selected a single, privileged view on his brain in succeeding stages of dissection, Willis indeed appears to have captured the brains without any prior human intervention, almost mechanically. Apart from the crosshatchings Wren spartanly clotted on the copper plates to connote shadow and depth, hardly any conventions of linear perspective (chiaroscuro, vanishing points, and so on) are deployed. Rather, his brains seem to float on the white space of the paper. This floating effect was partly due to the fact that the copper engravings and the text could not be printed on the same page. But clearly an effort has been made to rid the brains of their human contexts: there is no background scenery, no flesh, no blood, there are no human figures. All images in *The Anatomy of the Brain and Nerves* have a relatively flat visual field, and variables of depth, lighting, pose and background are as a rule held constant. And, in congruence with Willis’s new dissection technique, the brains are not sectioned, and are all depicted from below. The focus lies entirely on the solid grey structure of the cortex, on the equally solid brain stem and on the blood vessels. This focal point was accentuated even more because on occasion Willis clearly folded his specimen backwards (see for example figures 2 and 12).

In the preface of *The Anatomy of the Brain and Nerves* Willis self-consciously asserted:

> "I determined with my self [sic] seriously to enter presently upon a new course, and to rely on this one thing, not to pin my faith on the received Opinions of others, nor on the suspicions and guesses of my own mind, but for the future to believe Nature and ocular demonstrations."

Wholly in line with the new natural philosophical methodology Willis believed "ocular demonstrations" would support his "more certain Physiology" and his "Pathologie of the Brain." It is important to bear in mind that Willis is not stating the obvious here. Vesalius, too, had appealed to visual demonstrability and exactness. Seeing equaled believing to Vesalius as well; the eyewitness testimony was given crucial weight. But it was only after Francis Bacon that it was increasingly deemed important to be suspicious of artfulness, partiality, and an ornamented style. Although English natural philosophers' words were not always as good as their bonds, statements against scholasticism and the ornamental Renaissance humanistic natural philosophy were ubiquitous.[29]

John Wallis's letter to Henry Oldenburg, unfolding the accident on the Medley River and the subsequent autopsy of the scholar's body, was a key derivative of Baconian new natural philosophy. Thomas Willis's rhetoric in *The Anatomy of the Brain and Nerves* was slightly more ornamented. For instance, when discussing his discovery of the absence in humans (as opposed to animals) of a "wonderful net," the *rete mirabile*, he formulated his findings as follows:

> "[I]n a human Head, where the generous Affections, and the great forces and ardors of the Souls are stirred up, the approach of the blood to the confines of the Brain,

ought to be free and expeditious; and it is behoveful for its River not to run in narrow and manifoldly divided Rivulets, which would scarce drive a Mill, but always with a broad and open channel, such as might bear a Ship under Sail."

But Wren's mechanical, detached images in *The Anatomy of the Brain and Nerves* fitted the new natural philosophy's methodology like a glove. They were very successful communication devices because they deliberately joined forces with a larger, typically northern system of pictorial conventions.

'I was not ashamed to require the help of others'

The problem most anatomists striving for truth to nature had to deal with eventually, was to find an artist skilled in naturalistic representation. Andreas Vesalius could not have wished for a better artist: Jan Stephan Van Calcar, trained in the workshops of Titian, was highly skilled in the convincing and meticulous depicting of the anatomy of the human body. But Vesalius still took pains to place the authorization of the anatomical correctness of the images solely in his own hands, not in the hands of Van Calcar. According to art historian Martin Kemp Vesalius "exercised a direct and meticulous control over his illustrator."(Kemp, 1993, p. 97)

In contrast, Thomas Willis used every opportunity to underscore that it was Christopher Wren who made the engravings in *The Anatomy of the Brain and Nerves*. Wren, according to Willis, made the work significantly "more exact." In doing so, Willis aligned himself with the observational and experimental strand promoted amongst others by "Lord Verulam" Francis Bacon, followed by William Harvey in the 1630s and '40s and taken up by many of the future members of the Royal Society in the '50s and 60's.

One of the main goals in Baconian natural philosophy was to cast out misleading opinions or "Idols," by focusing strenuously on observation and experimentation. Sense-based information, particularly what was visually acquired, was decidedly elevated in epistemological status (Shapiro, 2000). 'Ocular demonstrability', it was thought, would lead away from the misleading world of human abstraction and imagination to the concrete world of things (cf. Hooke, 1665). However, as Svetlana Alpers pointed out, this intention to investigate as closely as possible nature leaned heavily on a trust in intermediaries to represent nature (Alpers, 1983). The facts were to be trusted because they were established empirically and were therefore truthful to nature, yet for their corroboration a thoroughly social technology was increasingly deemed crucial.[30] Without reliable eyewitnesses, facts could not be established.

Historians of science have by now established in detail the emphasis on the ‘fact’ as a theoretical construct in early modern English natural philosophy.[31] In *A Culture of Fact. England 1550-1720* (Ithaca, 2000), Barbara Shapiro adds to the canon an innovative line of reasoning concerning the rise of the scientific fact in early modern England. She argues that the development of English empiricism was highly influenced by a discourse formerly restricted to the legal arena, a discourse that gradually became influential in other areas of public life too.

According to Shapiro, the new Baconian natural philosophy in England slowly but surely forsook the humanistic distinction between *verum* and *factum*, between the works of God and those of man. Natural phenomena were increasingly handled under the decree of *factum*. Consequently, methodologies that were previously limited to judging human actions became valid in the realm of natural philosophy as well.

Shapiro relates the construction of the Baconian natural philosophical ‘fact’ to the legal concept of ‘fact’ or ‘matter of fact’. The common law in early modern England distinguished between ‘matters of fact’ on the one hand, and ‘matters of law’ on the other. The former fell in the realm of jurors, while the latter was the province of judges. Jurors were charged with determining the credibility of evidence, and, by the end of the sixteenth century, of witnesses as well. From that time on jurors, as maintained by Shapiro (2000), judged the authenticity of testimonies according to a set of criteria they themselves helped formulate: ability, probity, skill, opportunity, fidelity, status, experience, and reputation. By listening to witness testimony produced by trustworthy persons under certain conditions and by following strict procedures, so-called ‘judges of the facts’ were able to produce authentic knowledge of matters of fact.

After the fact had made headway in the legal arena and had expanded to a wide range of intellectual endeavors as well, the construct arrived in the community of naturalists, where it soon played a fundamental role in the development of English empiricism.

Contrary to our contemporary usage of the word - in which anything labeled a fact is already established as valid - a matter of fact in English seventeenth century natural philosophy was an issue still subject to empirical validation. Accordingly, matters of fact in the new Baconianism were preferably corroborated by careful observation and experimentation. They required a human presence, the rejection of hearsay, and a multiple witness testimony. Witnesses needed to possess firsthand sense knowledge and were to be of ‘appropriate’ credibility, established after a careful evaluation according to the strict, legally derived criteria mentioned above.[32] Bacon’s successors did not all agree on how to move from facts to the explanations of those facts, to the principles of natural philosophy. Most members of the Royal Society, Willis included, were inclined to move from well-proved facts to conjectures and hypotheses. According to Shapiro absolute, permanent and even ‘moral’ (the highest humanly possible degree of probabilistic assurance) certainty shouldn’t be expected of hypotheses, because causal analysis and inference involved applying reason, and sometimes even imagination, to established facts. This was actually one of the fundamental differences between Willis’s *The Anatomy of the Brain and Nerves* and Descartes’ *Traité de l’Homme*: the latter went far beyond the observable. And where Vesalius had wanted to provide an all-encompassing atlas of the human anatomy, Willis intended to consider nothing that was already “well enough known”[33] in *The Anatomy of the Brain and Nerves*. In the text he generally indicated only briefly what he considered to be accepted knowledge. By the same token he chose his images carefully. They were part of the much longer sections in which Willis discussed his hitherto untried matters of fact.

This is not to say there was no controversy over the embodiment of observations in images amongst new English natural philosophers. Their epistemological status was problematic, because they were typically prepared by persons other than eyewitnesses (Shapiro, 2000). Andreas Vesalius, and many of his successors, relied on the skill of gifted craftsmen, but felt that they needed to have complete power over the dissections and subsequent anatomical claims. By taking on Wren as an engraver, Willis accomplished something else. To begin with, he diminished the amount of mediators engaged in the materialization of his book.[34] Wren was a highly competent, trustworthy expert witness, with firsthand sense knowledge and very appropriate medical credibility. But even more importantly, he was a witness who was capable "with his own most skilful hands" of attesting to Willis's anatomical innovations. Therefore Wren's images were the most advantageous evidence Willis could ask for.

Conclusion

Although most present-day historical treatises of the neurosciences acknowledge the elegance of Wren's drawings, they simultaneously maintain that elements in the depictions of the brain are inaccurate, not of a high standard, partially obscured or stylized (e.g. Clarke & Dewhurst 1972; Gross 1998). Arguments like these suggest that it is hard for twenty-first century audiences to account for historical variations in what we consider to be rational explanations of nature. The idea is widespread that in order for objects under scrutiny to be categorized as 'scientific', 'objective', 'or 'truthful', they need to be captured with as less human agency as possible. As a result, what gets obscured is the idea that both objectivity and realism are contingent and historically situated notions. It is not so much a matter of the *actual* realism of the images, it is a matter of how they were *constructed* to *look* realistic. In the apt words of Robert Martensen (2004, p. 202), the true-to-life engravings in *The Anatomy of the Brain and Nerves* are a "cultural hologram," radiating the anatomical, medical and artistic milieu of seventeenth century England. By embracing certain key elements of the New Philosophy, Willis declared himself in favor of an unadorned, undogmatic style. His strategy to achieve truth to nature was in line with Baconianism, for he set out not to capture accepted ancient wisdom, or to ground his anatomy on hypotheses and imagination, but to provide a description of new knowledge, acquired by means of immediate, factual observation.

This is not to say that Willis did not, willingly or coincidentally, obscure anything in his engravings. They were highly mediated communication devices with their own agenda. This mediation is not a sign of their weakness. Rather, to a large degree it defined what made the images powerful.

CHAPTER 2

Light Tries the Expert Eye.

The introduction of Photography in Macroscopic Neuroanatomy.[2]

It is hard to imagine a time when taking a photograph was not evidently interwoven with people's lives. Although the basic mechanics and chemistry had been known for long, it was not until around 1800 that attempts at making a mechanical, permanent record directly from nature started to occupy the minds of a small number of experimental natural philosophers. The light-sensitivity of silver-salts had been discovered well over a hundred years before. The camera obscura - an optical instrument that projects fleeting, upside-down images of distant scenes via a lens onto a surface - dates back to antiquity, and was used widely from the sixteenth century onwards.[35] Still, it took until August 19, 1839 for photography to be publicly announced at the Institut de France by the astronomer and natural scientist François Arago, who spoke on behalf of Louis Daguerre. Protocol prevented Daguerre to present the invention himself, as he was not an official member of the Institut.

At the invention of photography its images were embraced by scientists for their acclaimed precision, as well as their mechanical production process. One of the main reasons why photographs appealed to the sciences was that they were deemed more accurate than drawings or engravings. They were thought of as authentic records, imprinted by light itself. In *The Pencil of Nature* (Fox Talbot, 1844) William Henry Fox Talbot, pioneer of what he referred to as photogenic drawing, spoke for many when he proudly described his invention as "the process whereby natural objects can trace themselves, without the help of the artist's pencil." In the words of Talbot "[t]he scenes represented (…) contain nothing but the genuine touches of Nature's pencil." (Fox Talbot, 1844, preface)

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[2] A slightly adapted version of this chapter was published in the *Journal of the History of the Neurosciences* (De Rijcke, 2008a).

It is often argued that photography's scientific inauguration meaningfully coincided with a shift towards the ideal of mechanical or non-interventionist objectivity. The point is made forcefully in a classic article on scientific image making by historians of science Lorraine Daston and Peter Galison (1992), already referred to in the previous chapter. Daston and Galison have recently developed their argument in *Objectivity* (Zone Books, 2007). According to the authors, mechanical objectivity entailed a heavy reliance on methodological imperatives, on automatic recording devices and on impersonal instruments. Positive assessments were easily made with regard to machines like the camera, because they outdid humans on all fronts: machines had no thoughts, no feelings, and were incapable of reasoning. Machines "offered freedom from will – from the willful interventions that had come to be seen as the most dangerous aspects of subjectivity." (Daston & Galison, 1992, p. 83) Ideally, the scientific images produced in the latter half of the nineteenth century were highly matter-of-fact, the authors contend. Intentional selection and distillation were banned as much as possible, as was the adding of lines or marks to aid the reader in understanding the images. The reproduction of a whole series of photographs from the object under scrutiny was preferred over representations of single images standing for a whole class. While selection and refinement had been one of the chief actions of Renaissance and Enlightenment natural philosophers, photography allegedly enabled scientists to delegate the "burden of representation" to their audience, in order to safeguard the transparency of their (series of) individual representations (ibid, p. 107).

Anthropologists were among the first scientists to experiment widely with the camera. They attached much value to a strictly regulated observation, and because of its acclaimed precision and speed they saw a bright future for photography (see Edwards, 1992). In medicine the benefits of using photography were recognized at an early stage as well. Around mid-nineteenth century, clear perspectives were developed on many diseases, their inceptions, anatomy and cellular pathology. Doctors increasingly sought numerical, graphic and pictorial evidence of particular diseases (Reiser, 1993). In view of the latter goal, the use of photography was rapidly considered indispensable. A third example of a scientific field that experimented extensively with the new technology was astronomy. Astronomers saw the photographic camera as a weapon in the struggle for an all-encompassing map of the heavens (Kapteijn, 1891). The camera, in the words of astronomer Jules Jansen, was “la rétine du savant” (Snyder & Allen, 1975).[36] This was obviously a metaphor: one of the advantages of the new technology for astronomy was the very fact that it facilitated the registration of information *invisible* to the human eye.

The very values of precision, determination and acuteness were readily attributed to photography and, like any other self-respecting scientific discipline, shared by the emerging field of neurology. Still, it took until 1873 for the first photographic atlas on the brain and nervous system to appear: Jules Bernard Luys’ *Iconographie Photographique des Centres Nerveux*. The atlas contained seventy albumen prints of frontal, saggital, and horizontal sections of the brain. Some of them were enlarged with a microscope, but the majority represented gross neuroanatomy. Surprisingly, the publication of the *Iconographie* did not lead to a proliferation of neuroanatomical photographic atlases in the subsequent decades. In addition, the use of photography or of photomechanical technologies in contemporary journals and textbooks remained rare. This raises a number of questions. What exactly was the role cut out for photography in Luys’ atlas? Why wasn’t his initiative followed-up? Was there something particular about neuroanatomy that made the images ill-suited to photography, whereas other ‘sciences of the eye’ did find the new technology useful? What were the conditions in neuroanatomy that privileged other modes of representation besides photography?

Much valuable historical research has focused on the use of the camera in late-nineteenth century practices of classifying neurological disorders (see for example Burns, 1983; Didi-Huberman, 1982; Fox & Lawrence, 1988; Gilman, 1976). To a degree the same holds for the introduction of photomicrography in histological research (cf. Schickore, 2002; Stahnisch, 2005, Taureck, 1980). So far historians have paid little attention to the investiture of photography in gross neuroanatomy.

Macroscopic images of the brain figured prominently in topographic studies, in investigations of lesions and in localization debates, but also in textbooks on morphology, physiology and pathology of the brain. This chapter constitutes a first inquiry into contemporary practices of visualizing gross neuroanatomy. It contributes to a more detailed analysis of the use of photography in relation to neuroscientific values underlying practices of objectivity. Because the *Iconographie* has reached the status of an icon (Canguilhem, 2004), it will serve as a point of reference in the current analysis. In what follows, I will tie Luys' work up with that of some of his immediate predecessors, and will also scrutinize the atlas Luys published prior to the *Iconographie*. In conclusion, I will examine the adoption of photography in the visualization practices of Luys' successors.

Below I will argue that in macroscopic neuroanatomy, photography did not offer a satisfactory alternative to drawing or engraving. Intriguingly, it was in the first photographic atlas of the brain and nervous system - containing the very images that were praised by their author for their 'précision photographique' – that the reasons for the demise of photography were already manifest. But let us first return to the early 1860s, when Jules Bernard Luys took his first steps on the path of what turned out to be a very successful career in neurology.

The First Atlas

Jules Bernard Luys (1828-1897) was born in Paris. After completing his classical and medical studies he went on to spend the greater part of his career in his birthplace. Paris was the cradle of modern medicine for the mentally ill: between 1785 and 1826 French alienists (Pinel, Pussin, Esquirol and their successors) had founded what is now often referred to as the Paris Clinical School, exemplified amongst other things by the use of medical statistics, by a correlation between external physical examinations and lesions found at the autopsy, and by the hospital being the site of medical activity and research (Magner, 1992). Hospitals and psychiatric institutions, where patients were institutionalized and cared for, but also sometimes passed away, provided the doctors with excellent opportunities to relate clinical findings to their investigations at the dissection table.

Luys made important contributions to neurology, including the identification of two forebrain structures to which his name is still eponymically attached: the subthalamic nucleus and the major thalamic nucleus he termed “centre médian” (Krieg, 1970; Parent, Parent & Leroux-Hugon, 2002). He became a doctor of medicine in 1857, and was elected hospital physician five years later, at the age of thirty-four. That same year he became ‘chef de service’ at the Salpêtrière, where Jean-Martin Charcot and Alfred Vulpian had just become directors of the clinics and had initiated a systematic anatomical-medical study of neurological pathology (Poirier, 2003). In addition, Luys worked at the Charité, to which he remained affiliated until he gave up work in 1893. From 1864 onwards he was also the director of the psychiatric institution in Ivry (Parent, Parent & Leroux-Hugon, 2002). Moreover, he was co-founder and editor of *L’Encéphale*, the first “impartial, practical and skeptical” journal on nervous diseases in France (Ball & Luys, 1881, p. 2).

Three years after his inauguration as a hospital physician Luys published *Recherches sur le système cérébro-spinal, sa structure, ses fonctions et ses maladies* (1865). The *Recherches* was the first book that displayed his views on the brain and central nervous system, its anatomy, physiology and pathology, by expounding on several meticulous sections of the brain and spinal cord. The atlas contained forty ‘planches’, comprising up to fifteen beautiful images per plate.

Interestingly, while Luys provided his readers with a lengthy description of the procedures followed to rule out mistakes in preparing sections of the brain, he remained completely silent on the images. But there was a little more credit to be gained by the lithographs. First of all, they were all drawn ‘d’après nature’ (after nature) by Luys himself; the lithographer Jean Baptiste Léveillé subsequently rendered them into lithographs (see figure 14). Léveillé was an excellent draftsman; before associating with Luys he had been involved in numerous medical iconographies (see for example the works of Anger, 1865; Hirschfeld, 1853; Malgaigne, 1862). Secondly, the atlas was printed by Lemercier & Cie, the best printing house in Paris. Last but not least, the distinguished (medical) academic publisher J.-B. Baillière & fils handled the distribution.

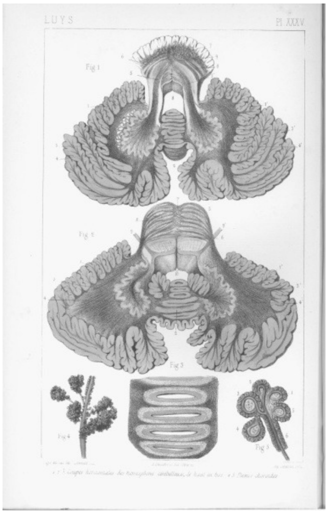


Figure 14 Lithograph by J.-B. Léveillé (based on drawings by J. B. Luys) (Luys, 1865). Courtesy of University Library Groningen, The Netherlands.

As can be seen in figure 14, the lithographs in the *Recherches* stand midway between a naturalistic and a diagrammatic representation of structure. This is revealed by the way segments of the cerebellum are folded-out, but also by the very reductive and thick lining. The lithographs contrast sharply with the images in the publications of some of Luys' foremost predecessors, where ideal-typical images still figured frequently. Lorraine Daston and Peter Galison asserted that seventeenth to mid-nineteenth century atlases were dominated by the typical image, both in its ideal and in its characteristic form (1992; 2007). An ideal-typical image presented a perfected representation of the subject. It was a universal image, based on the judgment acquired through years of experience. A large role was awarded to the artist's expertise, as the stylishness and rhetoric of presentation were integral parts of the images (Daston & Galison, 1992; Hildebrand, 2004; Kemp, 1999). The characteristic type was a 'naturalistic' alternative to the ideal-type: displayed was an individual object that stood for a type, but with the ambition to display a direct - that is, as unmediated as possible – image of form as it appears in reality.

It is perhaps not surprising that the characteristic image emerged for the first time in England. As we have seen in the previous chapter, from the mid-seventeenth century onwards, English natural philosophers were increasingly suspicious of artfulness, partiality, and an ornamented style in anatomical images. Two additional examples, brought forth by Daston and Galison, can be found in William Cheselden's *Osteographia or the Anatomy of the Bones* (London, 1733) and in William Hunter's *Anatomia uteri humani gravidi* (Birmingham, 1774) (see figures 15 and 16).[37] Where Cheselden turned to the camera obscura as a drawing aid, Hunter's monograph is a good example of the 'flesh-and-blood' school of illustration (Daston & Galison, 1992). The idea behind the latter school was to display objects exactly as given by perception, and to take that as literally as possible: each blot, stain, blood cloth or hair was to appear on the image, "warts-'n-all" (Kemp, 1993, p. 105). Interestingly, the mediation entailed in this process of visualization, for instance the injection of anatomical specimen with wax or dyes, was not perceived as problematic at the time (Daston and Galison, 1992).

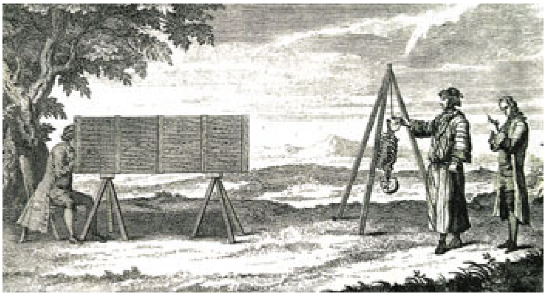


Figure 15 (Cheselden, 1733)

Figure 16 (Hunter, 1774)

At the turn of the nineteenth century, the question of the faithful depiction of the human body increasingly became an undertaking in need of support by empirical methods. Ideally, and contrary to the matters of fact of Thomas Willis's day (sustained by - and essentially non-existent without - human witnesses), nineteenth century facts “hardened,” and had nothing to do with a human presence at all (Daston, 1998). Only a small section of the art world remained affiliated with science, and became an instrument of anatomy. Artists were basically expected to have knowledge of essential morphology and an eye for detail, as well as particular technical skills relating to the act of visually representing (human) form. The extent to which they could find room for expression shrank to a bare minimum (Hildebrand, 2004). This is consistent with a transformation from 1800 onward into a new, science-directed art with a focus on diagrams, models, schematic images, and photographs (Daston & Galison, 1992, 2007).

In France, the ideal-typical image still figured frequently in the anatomical treatises published during the first half of the nineteenth century. The ties between art and science were not entirely severed. Hildebrand (2004) described how the newly articulated demands for factuality and empiricism, clearly visible in the Paris Medical School, bore a significant relation to a dominant Neoclassical style. Characteristic was a desire to ‘return to nature’. Today, Neoclassicism is habitually labeled as the antonym of Romanticism (where the imagination was allowed to flourish). In contrast, novelty and self-expression were not Neoclassical assets; Neoclassicism was the summit of a long tradition of abstraction. It was the art of the ideal, and implied the exhibition of complete control over a technique. Due to its ideal-typical realism, art historians often allude to the work of painter Jacques-Louis David to epitomize the Neoclassical style. ‘David’s school’ was on occasion referred to as an exact science, because it was thought to have mastered the faithful rendition of nature so well (Hildebrand, 2004; Janson & Janson, 2004).

Not everyone was pleased with the omnipresence of Neoclassicism. “‘David’s School’,” as the novelist and art critic Stendhal (1783-1842) commented in the Salon of 1824, “is only able to paint bodies; it is unquestionably unable to paint souls.” (Stendhal in Hildebrand, 2004, p. 303) This was not a problem for those working in the sciences: slowly but surely a smaller amount of ‘soul’ was exactly what was called for.

Interestingly, there is a professional lineage between David's school and Jules Bernard Luys' lithographer Léveillé: the anatomist-draughtsman N. H. Jacob, a member of David's school, was Léveillé's mentor. Together with Jean-Marc Bourgery, Ludovic Hirschfeld's teacher, Jacob published the *Traité complet de l'anatomie de l'homme* in 1831 (see figure 17).

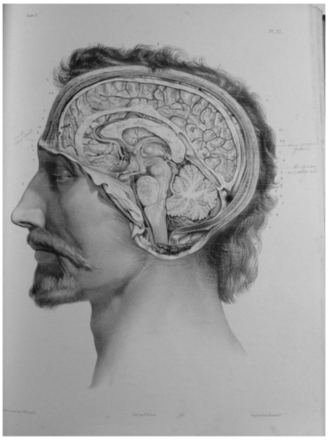
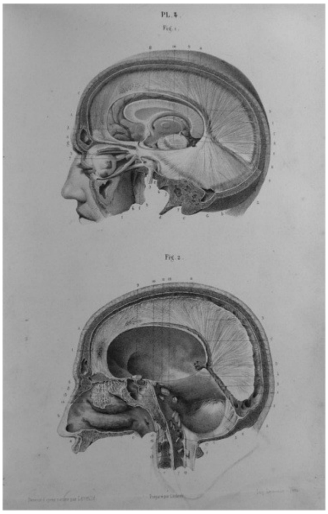


Figure 17 Lithograph by N.H. Jacob (Bourgery & Jacob, 1831). Courtesy of University Library Groningen, The Netherlands.

The lithographs in Bourgery and Jacob's atlas readily met the demands of exactness and effectiveness of the Paris Clinical School, exemplified by an iconography of physical barriers, hollow spaces, and hidden organs (Hildebrand, 2004). There is a clear relationship between Bourgery and Jacob's lithograph and Neoclassicism's defining features. Although the brain is semi-exposed, the man in the lithograph appears to be alive, with a head full of hair placed firmly on a muscular neck, his eyes slightly opened. The lithograph reproduced above is not a representation of an actual dissected head, but an idealized image. It stands in a long tradition of anatomical visualization practices, initiated in 1543 by Andreas Vesalius in his *De humani corporis fabrica*.

Bourgery's student Ludovic Hirschfeld published his *Névrologie ou Description et Iconographie du Système Nerveux et des Organes des Sens de l'Homme* in 1853, twenty-two years after Bourgery and Jacob's *Traité*. The book was printed by

Lemercier et fils, and published by the Baillière family business. Léveillé, Jacob's student, both drew and lithographed the images in Hirschfeld's atlas. Let us compare one of those lithographs to one of the images in Luys' *Recherches*, drawn by Luys and lithographed by Léveillé (see figures 18 and 19).



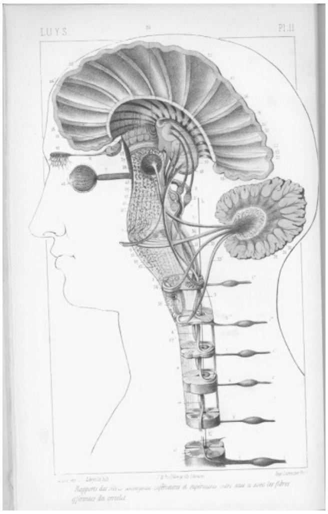


Figure 18 Lithograph by J.-B. Léveillé (Hirschfeld & Léveillé, 1853). Courtesy of University Library Groningen, The Netherlands.
Figure 19 Lithograph by J.-B. Léveillé (based on a drawing by J. B. Luys) (Luys, 1865). Courtesy of University Library Groningen, The Netherlands.

A striking similarity between the lithographs is that they both only broadly sketch what region of the body is on display. In Hirschfeld's atlas, Léveillé still drew skin, while in Luys' atlas an effort is made to abridge and simplify the subject matter. Luys left much less room for Léveillé to do what he was trained for. His disembodied image represented the growing dominance of a mode of representation that consciously avoided stylishness (Kemp, 1998). Luys devoted a lot of space to these synthesizing, diagrammatic lithographs in the *Recherches*. For present purposes it is important that at no point did he denounce his own interventions. This was about to change rapidly.

The *Iconographie*

In 1873, nine years after the *Recherches* was issued, Luys published his two-volume *Iconographie Photographique des Centres Nerveux*. Book I was a 114 page text, book II was the accompanying atlas (size 27 x 35,5 cm). The atlas contained seventy beautiful albumen prints made from collodion (glass plate) negatives of frontal, saggital, and horizontal sections of the brain. The photographs were made by Luys' son Georges, himself a physician, and were printed by the photographer Valette (see figure 20).[38]

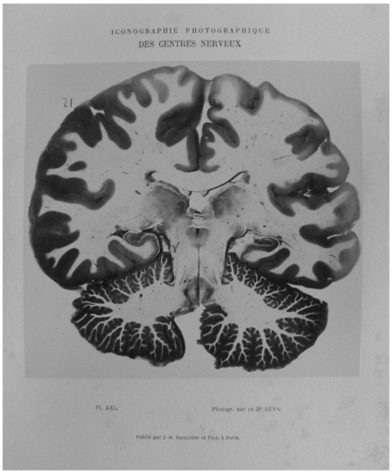


Figure 20 Photograph by J.B. Luys' son Georges Luys (Luys, 1873). Courtesy of BIUM (Bibliothèque Interuniversitaire de Médicine et d'Odontologie), Paris, France.

The *Iconographie* was most likely intended both as a 'précis' and a strategic document. Firstly, compared to Luys' earlier work the number of novel findings was negligible. Secondly, to Luys employing photographs was not self-evident. As we have seen, he had previously made use of lithographs, which were based on drawings of anatomical specimen Luys himself had made. Thirdly, after the *Recherches* was published, Luys was praised for his pioneering work on the brain and central nervous system, but the decision to draw the slices himself was heavily questioned (Ribot, 1876). In the 'avant-propos' Luys referred as follows to the criticism:

“The favorable reception in France and abroad of my previous investigations of the nervous system did not make me indifferent to legitimate inclinations of a considerable portion of the scientific world that does not accept without serious guarantees new ideas that arise in a field. Indeed, after this publication I have seen enlightened men, men to whose utmost impartiality I would gladly pay tribute, cast doubt on my initial drawings of the nervous centers from which I elaborated my overall understanding, and heard them wonder whether these drawings indeed truly reflect that which I had seen, or even whether I had seen this solely with the help of my own eyes, or whether the overly dedicated author’s overactive imagination had not also contributed to what I had seen.” (p. III)

As can be inferred from the above quote, Luys associated drawing with the risk of falling prey to an overly active imagination, while what was needed instead was distance and impartiality. This is exactly where photography entered the equation.

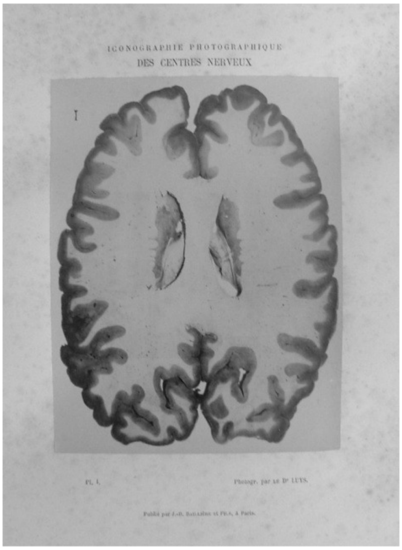
“I thus resolved, by erasing myself completely, and by substituting the action of light for my own personality, to obtain a reproduction, equally impersonal and authentic, of the main anatomical details that had served as types for my previous descriptions, and thereby to answer triumphantly the criticisms leveled at my first publications.” (p. IV)

A careful reading of Luys’ presentation reveals that he decided to photograph the same neuroanatomical cross-sections he used for the *Recherches*. The meticulously prepared slices of his ‘model’-brain (a collection of stained sections from the brains of several men in their late-twenties) served as types for previous descriptions of the nervous system, and Luys apparently saw no reason to change that for the *Iconographie*. This implies that, although he obviously grappled with relying too much on his own interpretations, in practice his photographs were intended to stand for a whole class of brains. In his case abstinence and mechanization - core features of the nineteenth century ideal of mechanical objectivity - went hand in hand with obvious selection. Each particular slice, carefully selected, prepared and stained by Luys, stood for numerous alternatives of that slice in nature.

Moreover, it seems that not only the artist's intrusiveness was to be avoided (according to Luys the photographic prints contained no 'charactères artistiques', artistic elements), but also that of the scientist himself. The way Luys mobilized photography, diverged vastly from the way for instance Vesalius and Willis drew attention to their images. Luys testified to having been prepared to efface himself completely, in order to let nature speak for itself. His mea culpa reveals a very strong ethos of modesty, geared at deflecting distortions of facts by judgment or intervention.

George Levine (2002) argued that nineteenth century scientists began to struggle with the inescapable presence of the interpreting self, and wanted to rigorously repress their own biases. In his words, they were 'dying to know'. The scholar's personality was increasingly deemed inconvenient, a factor that literally stood in the way of authenticity.[39] By mobilizing the camera, Luys made it clear that not he, but the action of the light would give evidence of proper scientific conduct. But I this is only half of the story: in practice, Luys and his neuroanatomical successors were prepared to go very far in taking precautions to eliminate bias, but this did not mean that they prohibited the use of synthesizing images altogether. This is overtly visible in the first photographic neuroanatomical atlas: all photographs in Luys' *Iconographie* are accompanied by explanatory lithographs on the next page (see figures 21 and 22).

Let us briefly call to mind Daston and Galison's analysis of the scientific images produced in the latter half of the nineteenth century, highlighted in the introduction to this chapter. Daston and Galison stated that in the production of these images - ideally - selection, refinement and distillation were banned as much as possible. In order to safeguard the transparency of individual representations, scientists attempted to delegate the 'burden of representation' to their audience. Because photography allowed for the reproduction of a whole series of photographs from the object under scrutiny, the new technology seemed the perfect tool to accomplish this detachment (Daston and Galison, 1992, p. 107). I would like to question the appropriateness of this analysis in the case of Luys' *Iconographie*. In an essay on selection and mathematization in the visual documentation of microbiological scientific objects, sociologist of science Michael Lynch discussed two examples of paired representations of a photograph and a diagram. Initially, Lynch wrote, he presumed that the relationship between the paired representations of the same object was regulated by the fact that the diagram selects relevant information from the photograph. Upon closer scrutiny however, he argued, the photograph and diagram "have a *directional* relationship to one another (…). Relative to the diagram, the photograph appears to be more 'original' material, whereas the diagram is more evidently analyzed, labeled, and 'idealized'." (1991, p. 160. See also Tucker, 2005) According to Lynch, the result of pairing a photograph and a diagram of the same object is that the photograph will be deemed "unique, situationally specific, perspectival, [and] instantaneous", while the diagram "brings into relief the essential, synthetic, constant, veridical, and universally present aspects of the thing 'itself'." (p. 163)



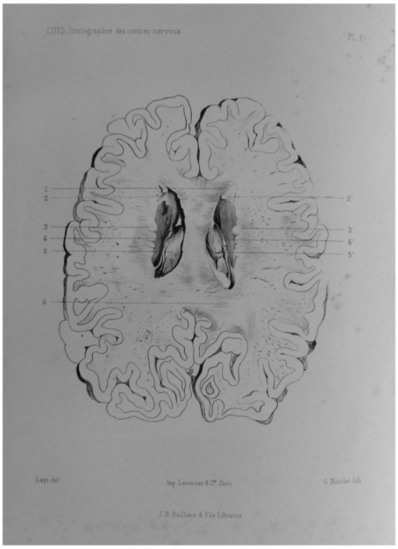


Figure 21 Photograph by J.B. Luys' son Georges Luys (Luys, 1873). Courtesy of BIUM (Bibliothèque Interuniversitaire de Médicine et d'Odontologie), Paris, France.

Figure 22 Lithograph by G. Nicolet (based on a drawing by J. B. Luys) (Luys, 1873). Courtesy of BIUM (Bibliothèque Interuniversitaire de Médicine et d'Odontologie), Paris, France.

It goes without saying that photographs are not unmediated representations of reality. As is the case with drawings, photographs are mediated by decisions made during the production process – in this case decisions pertaining to angle, frame, exposure time, et cetera. But if we transfer Michael Lynch's line of reasoning to Luys' *Iconographie*, one might argue that by coupling photo to litho Luys revealed what photographs were *not* capable of, namely to emphasize the basic characteristics of the brain's gross anatomy. It still took the expertise of the neurologist and the craftsmanship of the lithographer to endow Luys' readers with that emphasis. Moreover, by including photographs in his atlas, and pairing them to explanatory lithographs, Luys actually reinforced the assumed mimetic character of the photographs. But one needed skill to think them through, and to understand reality, as the images were not legible in and of themselves.[40]

The Image as a Map

In the following decades, both technological advancements in photography and disciplinary expansion in neuroanatomy were expected to lead to a growing use of the new technology as a visualization tool. Due to the rapid carving up of the sciences into specialized domains, each requiring arduous academic training (Lankford, 1981), there was a systematization of neurological practice. A great pioneering spirit prevailed: in Europe alone several neurological societies and journals were instigated. In London, the founding of the *National Hospital for the Paralyzed and Epileptic* in 1860 officially institutionalized neurological studies and treatments. In Berlin, Griesinger set up the *Archiv für Psychiatrie und Nervenkrankheiten* in 1868, while the *Neurological Society of London* was established in 1885. The *Société de Neurologie de Paris* followed in 1899. A number of journals saw the light that either focused entirely on neurology or did so under the colors of psychiatry or physiology. For instance, Wilhelm Erb was the initiator of the journal *Deutschen Zeitschrift für Nervenheilkunde* in 1891. Jules-Bernard Luys co-founded the journal *L'Encéphale* in 1881 with Benjamin Ball. The journal *Brain*, the official periodical of the *Neurological Society of London*, was instituted in 1887, and Pierre Marie set up the *Revue Neurologique* in 1893, together with Eduard Brissaud. The range and the number of inquiries into the workings of the brain and central nervous system flourished. Disciplined declarations on proper methodology not only augmented the scientist's credibility, but also facilitated the replication of results. Although scholars kept repeating that nothing could replace the actual experience of dealing with a fresh brain (cf. Jakob, 1895, Vorwort), there was a growing need for their systematic visual categorization - not in the least because teaching novices became a fundamental part of the neurologists' workload.

When Luys published his atlas in the early 1870's, the growing pains of photography were largely over: photomechanical printing processes like the woodbury type (1864) and the collotype (1869) (where a photographic recording was transferred to a printing form, which was subsequently inked up and printed) solved the problem of the fading of paper photographs. Both the woodbury type and the collotype were relief printings. Therefore, text and photographs could not yet be printed together. It took until 1882 for the autotype to be invented. This was not a print from a photographic negative, but a reproduction of a positive print (either photographic or engraved), that could easily be printed along with accompanying text (Boom & Rooseboom, 1996).

Contrary to what one might expect, neither neuroscientific professionalization nor technical progress led to a boost in neuroanatomical photographic atlases. This is all the more interesting, taking into account that several neuroanatomists experimented with photography in their microscopic research. One of the leading experts in the field, Santiago Ramón y Cajal, chose to publish drawings instead of photomicrographs of his ground-breaking microscopic observations - even though he was a passionate amateur photographer.[41] Not only was Cajal endowed with excellent photographic skills, he also made numerous improvements to the technique. In the following chapter, I argue that there was something particular about representing nerve cells that impeded the use of photography. In a monograph on photographing nervous tissue, Cajal described that photographs were only comparable to good drawings when the slices of nervous tissue were very flat and thin (between 1 and 10 µm) (Cajal, 1907). But problems arose, he continued, when the histological slices were thicker, as is the case with nervous tissue (where the cells spread out in many directions and over long distances). Observing tissue through a microscope entailed that neuroanatomists constantly had to shift focus, and had to compose a synthesis of a large number of different optical planes, mostly under considerable enlargements. A photograph could not reproduce the important details perceived in multiple focal planes, and leave out the unimportant ones. This was something only an expert observer was able to accomplish in a drawing.

Let us now return to the question why photography was not effortlessly taken up in macroscopic neuroanatomy. I think this can be explained in part by a move towards abstraction. In the section on Luys' first atlas, the *Recherches*, I have shown the specific dynamics of such a move in relation to different types of neuroanatomical representation. Although photographic atlases proliferated in the late-nineteenth century, in (neuro)anatomy the professionalization of scientific and specialist practices brought about a rising dominance of the plain technical textbook. According to art historian Martin Kemp, this rise was epitomized by Gray's *Anatomy of the Human Body* (see figure 23). The first edition was published in 1858, but new editions are still appearing regularly. The images in Gray's *Anatomy*, Kemp argued, displayed an "awesome degree of abstinence" and were "drained of visual effects that did not serve rigorously didactic aims." (Kemp, 1998, p. 333)

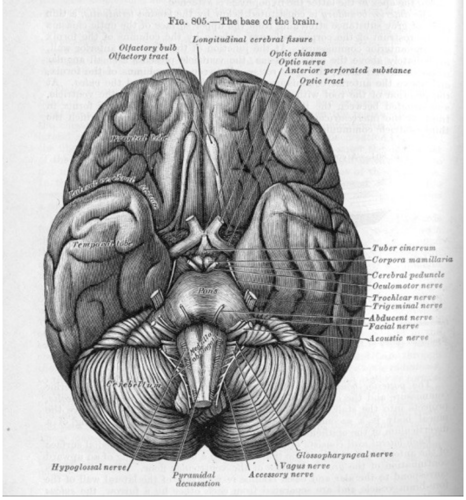


Figure 23 Engraving of the base of the brain (Gray, 1920). Courtesy of University Library Groningen, The Netherlands.

This is the type of image we already came across in Luys' 1863 *Recherches du Système Nerveux*. These images were deliberately not photographs, because they were designed more like a terrestrial map. They were meant to point the way through unfamiliar territory, and were aimed at a reader who was most likely a student or a practicing physician. The following is but one of the many possible examples of this strategy. It is particularly illuminating for its visual and linguistic manifestation, and can be found in the French edition of Hermann Nothnagel's *Handbuch der Krankheiten des Nervensystems* (a joint venture with Wilhelm Erb and Albert Eulenberg, published between 1876 and 1878):

"[J]ust as one needs a geographical map in a military campaign to know where the action takes place, we deemed it necessary to add some figures of normal anatomy. (…) We believe (…) that the arguments can only gain in transparency when the reader himself can mentally trace the scope and topography of the lesions according to a well-chosen plan, especially in an age in which the comparative descriptions of the central nervous system requires much more considerations than before."(Nothnagel, 1885, p. xxi)

In drawings and engravings, specific aspects of the brain could be stressed by an interplay of colors or by crosshatchings, while others disappeared to the background by shading or omission. As was clearly demonstrated by Luys in his *Iconographie*, this was very hard to do with photographs.

Although the camera was thought to provide images that were perfect mechanical equivalents of reality, erasing the act of representation in the process, (photo)mechanical technologies were only partially adopted in the decades after Luys' photographic atlas was published. Even in the neurological textbooks, journals and atlases appearing after the 1870s that concentrated on demonstrating anatomical likeness (as opposed to drawing maps), the reproduction of actual photographs was rare. Often it was deemed more suitable to rein in artistic freedom, as was the case with the astounding chromolithographies in Christfried Jakob's 1895 *Atlas des Gesunden und Kranken Nervensystems* (see figure 24), of which the author stated in the preface:

> "The images present matters as they truly are in reality, without schematizing. (…) The reproduction in lithographs and woodcuts is of a superb quality, and was executed under my strict control."

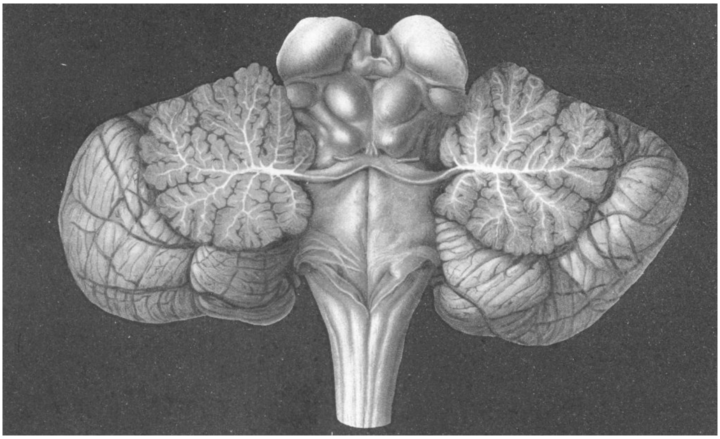


Figure 24 Chromolithography of the cerebellum (Jakob,1895). Courtesy of University Library Groningen, The Netherlands.

Twenty-six years after the *Iconographie* was published one of Luys' renowned successors, Joseph Jules Dejerine, issued the voluminous *Anatomie des Centres Nerveux* (Paris, 1898). In the preface he wrote the following on the use of photographs in contemporary neurological volumes:

> "Until today, despite some very fortunate attempts, the photograph has not replaced the drawing for the reproduction of slices of the central nervous system. As far as macroscopic slices are concerned the remarkable representations obtained by Luys are as of yet unsurpassed and, we find, are no match for the results accomplished by a skillful draftsman (…)." (Dejerine & Dejerine-Klumpke, 1898, p. 55)

One of the reasons why neurologists kept depending on the artist's expertise was that many believed photographs were too detailed: they provided a record of the preparation that encompassed even the most superfluous idiosyncratic element and the smallest sign of decay (Wolf-Czapek, 1911). Similar to drawing anatomical material, photographing the objects necessitated elaborate mediation. As a minimum, preparations had to be copied by hand before a photograph could be taken. This is the reason why Carl Toldt continued to employ an engraver to produce the woodcuts for his *Anatomischer Atlas* (1907):

> "The widely held opinion that the autotype guarantees a higher degree of truth to nature than a woodcut, is in actual fact not correct. Using photographic recordings for immediate reproduction of anatomical preparations is as a rule unsuitable, without having distorted the preparations through tracing them. As is the case with the woodcut, the degree of truthfulness to nature of the autotype depends on the composition of the drawing."[42]

Like Luys, these neuroanatomists were out to identify in their specimen more general characteristics of the brain, a desire that becomes all the more apparent in the rare cases in which actual neuroanatomical photographs were published: the images without exception overtly displayed interventions in the actual photograph, or in the preparation itself (see figure 25).

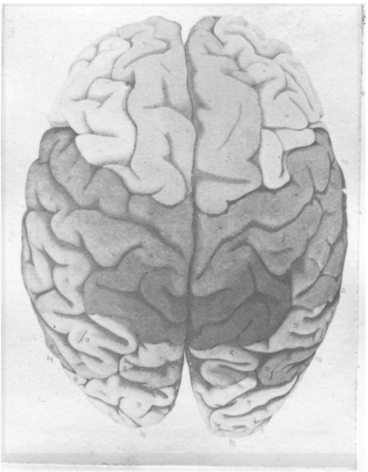


Figure 25 Photograph of the brain, seen from above (Bevan Lewis, 1882). Courtesy of University Library Groningen, The Netherlands.

In the only other neuroanatomical atlas I came across that made use of actual photographic prints, Gerbrandus Jelgersma wrote that drawings in those days were deemed contaminated because they were constructed. According to Jelgersma this was not the case with photography. He stated that the photograph “does not care for convictions the photographer might have.” However, he acknowledged that photographs were not ideal either. Contrary to some of his colleagues he thought they were not detailed enough: “(…) [A] photograph is more indistinct than a drawing, even if it is very good. It will never succeed in representing details (…) of which one objectively knows they are no tricks of the eye.” In order to accommodate for this lack of detail, Jelgersma decided to ask the collotypist to tamper with the original negative: “He knows of many small tricks, carefully kept secret by every collotype printer, that can sometimes surprisingly enhance the negative.” (Jelgersma, 1931, p. 6.) But even after the negatives were reworked and printed, Jelgersma felt that his photographs needed additional interpretation. He therefore copied his collotypes on tracing paper in order to point to relevant areas on the preparation, while discarding other information.

The partial use of photomechanical technologies and the utilization of projection devices, tracing paper or ground glass provided for the necessary mechanization, without compromising the markedly robust need for abstraction. This pertained to the Dejerines as well:

> "The reproaches addressed to the drawing as an agent in the reproduction of slices, particularly in relation to not having the mathematical precision a photograph does possess, can be avoided by copying the preparation onto tracing paper or by means of a projection device." (Dejerine & Dejerine-Klumpke, 1898, p. 55)

Projection drawings were produced by the aid of magic lanterns, consisting of a light source and a single lens. The Dejerines used a very strong illumination, equivalent to 'soixante bougies' (sixty lux). The lens focused a picture on a slide, or a sample between slides, onto a distant screen, after which the projected image was copied on paper. They described the process of tracing as follows:

> "The [macroscopic] slices are first traced by one of us in all its detail on a sheet of ground glass. The first drawing, which is entirely straightforward, is subsequently traced again on paper by the draftsman [M. Gillet] who has the preparation in front of him and provides the drawing with its finishing touches and shades." (Dejerine & Dejerine-Klumpke, 1898, p. 55)

It is a telling detail that the Dejerines openly affirmed the indispensability of a 'dessinateur habile'. Although the use of drawing was kept to a bare minimum and was standardized as much as possible, they could not – and more importantly, *would* not - do without human meddling to complete the representation. Because they obviously endorsed the ideal of non-intervention, the Dejerine's had Gillet's drawings reproduced by means of the photogravure. The drawings were photographed and a glass transparency was made of the negative. Subsequently the transparency was copied to a carbon print, and transferred to a copper plate. After printing, the image looked like a photograph, but then with series of connected lines, not dots. It had the subtlety of a photo, with the artistic quality of a lithograph (see figure 26).

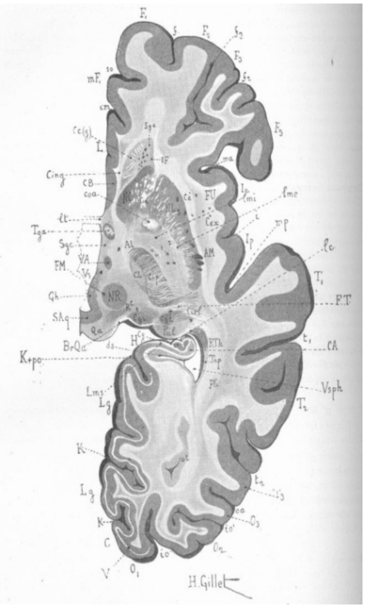


Figure 26 Photogravure of brain slice(Dejerine & Dejerine-Klumpke, 1895-1901). Courtesy of University Library Groningen, The Netherlands.

Conclusion

One of the purposes of this chapter has been to draw attention to an overlooked area in the history of imaging in neurology. Much like today's brain scanning techniques, the photographic 'faithful depictions of reality' held the promise of considerable contributions to neurology. However, most neuroanatomists discussed above had an ambiguous stance on using photographs in their works - despite their longing for a mechanical visualization process. Photography promised pictures uncorrupted by judgment, but this lack of judgment was precisely the problem. Photographs were deemed problematic because they were unlabeled, not selective, did not schematize, and were either too detailed or not detailed enough. Furthermore, since they were idiosyncratic rather than universal, their educational worth was small. Arguably, by switching over to photography, neuroanatomists would obtain mechanical representations, but the choice implied abolishing the power of discernment that is so characteristic of drawings. Not many scholars were prepared to pay the price.

Moreover - and this is very a-typical in light of the contemporary scientific ideal of non-intervention - neuroanatomists were not prepared to fully abolish their *own* expert appraisal. After all, they sanctioned mechanization but simultaneously openly defended elaborate intervention. In the previous sections I have analyzed the precise alignment of scientific values and disciplinary aspirations, of processes of publishing and production (including relations with artists and publishers), and of the representational techniques and conventions favored by authors. This analysis reveals that the label of mechanical objectivity only covers to a degree what actually went on in macroscopic neuroanatomical practice. The neuroanatomists considered above did indeed partially mechanize their visualization processes by the use of photomechanical procedures, tracing paper, magic lanterns and projection drawings. They also regulated their own behavior and that of the artists and technicians with whom they worked. But they never opted for the reproduction of whole series of photographs in order to delegate the 'burden of representation' to the audience. Photography never fully eliminated the need for abstraction that is so characteristic of drawing. Even in the rare instances where photographs were used, nearly all were either accompanied by an explanatory image, or were furnished with numbers, lines, letters or other symbols to assist the reader in understanding the image. The neuroanatomists discussed above made no attempt to disguise this. Evidently, authoritative visual representations of the brain never fully materialized to the complete exclusion of the personal, or by the sole use of mechanical instruments.

CHAPTER 3

The Ways of the Neuron. Observation and Visualization in the Work of Santiago Ramón y Cajal.[3]

Penetrating the seemingly unfathomable web of nervous tissue was one of the major challenges in the nineteenth century. From the 1830s onwards, greater microscopic magnification and resolution, and advanced preparation and staining methods, provided increasingly clear images of the nervous tissue. But it was not until the end of the 1880s before the discrete nerve cell was isolated as a theoretical and empirical object of study. This was done most forcefully by the Spanish microscopist Santiago Ramón y Cajal, using a relatively new double staining technique, the chrome silver method. The method was the first to stain an entire nerve cell, and enable the study of its thin and transparent filamentary extensions. It was introduced by the Italian histologist Camillo Golgi in 1873 as *la reazione nera*, or the 'black reaction'.

Golgi's invention resulted in the Nobel Prize for Physiology or Medicine in 1906. But Cajal is still lauded today for contributing the greatest number of high-quality, lavishly illustrated, studies on the morphology and relationship between nerve cells and nerve fibres - over 300. From 1888 onwards, he consistently argued that nerve cells are discrete entities, ending freely and communicating by way of contiguity ('neuronism'). In hindsight, his visualizations of neuronal morphology marked a turning point in an ongoing discussion with 'reticularists' like Golgi, who believed that the nervous system consisted of a continuous web of cells and fibres (cf. Clarke and Jacyna, 1987; Hagner, 2000; Jones, 1999; Otis, 2001). Eighteen years after his first article on the contiguity of neurons, Cajal shared the Nobel Prize stage

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[3] A slightly adapted version of this chapter appeared in *Interdisciplinary Science Reviews* (De Rijcke, 2008b).

with his Italian opponent. There was of course an irony in this fact. Although they were united by the *reazione nera*, on a microscopic level, they were worlds apart.

Cajal and Golgi's very similar methodology resulted in opposite theoretical perspectives. This raises a number of questions. For instance, what exactly were neurohistologists looking at when they peered down their microscopes? How did their observations take shape, and under which conditions were they taken seriously? How were Cajal's observations translated into a communal way of seeing? And what was the role of his much-admired drawings of nerve cells in this translation process?

Cajal's daily observational and experimental practices were intimately tied up with techniques of visualization.[43] This chapter analyzes how the three were related (cf. Lynch, 1998), by exploring the defining material mediations that shaped Cajal's observations. In the following, I will take visualization to include crafting actual drawings or photographs of the microscopic scene, but also other important visual practices that constitute the work of a microscopist (preparing, staining and fixing tissue, adjusting the focal distance, viewing the tissue, etcetera). I will first discuss Cajal's drawing techniques and his own epistemic stances on observation, in order to better understand his commitment to the image. In the second part of the chapter, I will examine a dispute over the existence of minute spines on dendrites. Cajal contended that he had provided conclusive visualizations of the spines, while other histologists believed they were artifacts of the chrome silver stain. Proving that the dendrites had marked endings was crucial for the establishment of the anatomical discreteness of nerve cells. Throughout, I will demonstrate that the 'reality' of contiguity was the result of negotiations on highly particular ways of seeing, drawing, and manipulating tissue, and investing tissue with meaning.

The Chrome Silver Method

Santiago Ramón y Cajal was born in 1852 in Petilla de Aragón, a small town in northern Spain. After completing his medical studies, he obtained the degree of

Doctor of Medicine at Madrid in 1877. He was appointed Professor of Descriptive and General Anatomy at Valencia in 1883.

Cajal saw his first preparation stained with the black reaction four years later. At the time, there was a certain *status quo* regarding knowledge on the morphology of brain cells. Already in 1839, Theodor Schwann had formulated the cell theory of animal life. The theory stated that all organisms are composed of individual organizational cells. Schwann’s work had encouraged others to agree that this also held for the nervous system.[44] But due to their more complicated ramifications, nerve cells were believed to differ from other cells. From 1865 onwards, and mainly due to the work of German neuroanatomist Otto Deiters, nerve cells were seen to have a single, extended ‘axis cylinder’ (today: axon) on one side of the cell body, and finer, branched ‘protoplasmic processes’ (dendrites) on the other. Seven years later, a staining method developed by Joseph von Gerlach visualized a part of the axis cylinder, merging into a coarsely woven network, while the protoplasmic processes were thought to fuse into a fine-meshed net (Gerlach, 1872). In the following years, Gerlach's view won many adherents. Although a few histologists continued to see separate cells, reticularism remained the dominant way to regard nervous tissue until the end of the 1880s.

Not long before Cajal was appointed Professor of Histology and Pathological Anatomy in Barcelona in 1887, he had decided to travel around Spain to inspect the country’s best-equipped laboratories. On a trip to Madrid, Cajal visited the private laboratory of Dr. Luis Simarro Lacabra. Simarro had just returned from Paris, where he had studied with Louis-Antoine Ranvier and Jean-Martin Charcot. He brought back to Spain an intimate knowledge of the latest histological techniques - including the *reazione nera*.

The procedure roughly worked as follows. Blocks of nervous tissue were impregnated with solutions of potassium dichromate (for 1 to 45 days or longer), were subsequently treated with dilute solutions of silver nitrate, then dehydrated, sectioned into minute slices, cleared in turpentine, and mounted in gum dammar. A precipitate of silver chromate randomly rendered 1-5 % of complete cells brownish-black against a transparent yellow background. The sight rendered Cajal speechless.

Cajal's visit to Simarro took place fourteen years after Golgi had officially announced the double staining technique in the *Gazetta Medica Italiana, Lombardia* (Golgi, 1873). Despite several attempts to promote his work outside of Italy, the *reazione nera*'s potential to reveal neuronal structure was not immediately recognized.[45] Despite useful descriptive papers by Ehrlich, Bohmer, Frey and Weigert, most histologists kept experimenting with several different staining techniques (Bracegirdle, 1986). In the histological handbook that Cajal relied on most at the start of his career, Ranvier's *Histologie* (1875), the chrome silver method was merely mentioned in passing - and only to discourage scholars from working with it. The prevailing opinion was that it was an unreliable stain, because it produced irregular deposits of silver granules.

At the time, a number of histological techniques were in use for the study of the nervous system. Typically, brain tissue was compressed between cover glasses in water or alkali after being fixated (hardened) in osmium tetroxide or potassium dichromate and impregnated with either carmine or gold chloride. Usually only the nerve cell's soma, and the beginnings of the axon and dendrites were revealed. Between the 1870s and '80s virtually every colored substance was tried out for its relevance. The process of transforming fresh brain tissue into a workable preparation allowed for many local adaptations. Variations were for instance possible in the combination of the age of the animal and the part of the brain under study, the temperature in which the tissue was hardened, the rules for embedding, and the freshness of the tissue (varying from a couple of days after death to living tissue, depending in part on local weather conditions).

In his autobiography, Cajal described the conditions under which he ran his first experiments with the black reaction. On a typical day, he wrote, he got behind his microscope at around nine. He spent the whole day dissecting, staining, fixing, observing, and drawing nervous tissue, and usually did not leave his (in-house) lab until midnight. The lab had access to a garden, where he kept rabbits, mice, rats, and other animals for dissection (Cajal, 1937). His first experiments confirmed the method's capriciousness. An analysis of the articles Cajal published between 1888 and '89, reveals that he tried hard to improve the technique. For instance, he experimented with multiple re-impregnations of tissue in the dichromate and silver solutions. Moreover, Cajal varied the duration of the impregnation, depending on for instance the age of the animals. The tissue that most easily absorbed the emulsion originated from brain areas in which the stratification of cell types and cellular ramifications were relatively simple, as was the case in the cerebellum, for instance. To increase the chances of staining complete cells, Cajal also tried working with thicker-than-usual brain sections (Cajal, 1888a-d).

On the basis of these early experiments, Cajal initially agreed with his colleagues about the unreliability of the stain. Although it appeared that he was seeing marked nerve endings, valid conclusions were impossible when they could also have been produced by irregular silver precipitation. But the consistency of his findings finally led him to argue against anastomoses (structural connections) between the nerve cell's endings. Among other things, he declared that the ramifications of axons ended in the gray matter, not in a diffuse network. According to Cajal, these ramifications approach the bodies and dendrites of nerve cells, but do not touch them. Therefore – and like all other cells in the body - nerve cells should be regarded as independent units, and not as belonging to a reticulum (Cajal, 1888a-d; 1889a-c).

Drawing into Abstraction

Cajal's adaptations to the chrome silver method assured him of better visualization of cellular tissue. The stain - rather coarse compared to other histological techniques - isolated only a small number of apparently discrete nerve cells, leaving its neighboring cells untouched. As we will see, Cajal took this selection process even further in his drawings.

Cajal's biographers agree that drawing had always been second nature for the Spaniard. They describe him as a visual scholar, who looked at the world with an artist's eye. Already as a young boy, he obsessively scribbled on every piece of paper that came to hand (Cajal, 1937; Esteban Leal, 2003; Otis, 2001; Rapport, 2005). It will not come as a surprise that in his neurohistological undertakings, too, Cajal took to drawing. Marius Kwint and Richard Wingate (2006, p. 745-51) pointed to the intriguing reflexivity between the "structural components of the nervous system" Cajal was studying, and the very same "dendritic mechanisms" that were at work while he was drawing tissue (his touch receptors, fingers, motor neurons, and retinal ganglia, for instance). Drawing the very delicate neuronal structures pressed his technical skills, and also nourished his aesthetic inclination. In 1900, Cajal showed some of his drawings to a Spanish journalist. The newspaper article quoted him as follows:

> "To a profane observer they would appear strange drawings whose detail is measured in thousands of a millimetre but they unveil mysterious worlds of the architecture of the brain, retina… Look, (…) here I am seeking a why and wherefore much beloved of painters: the appreciation of line and colour." (S. Junquera, 2003, p. 349).

In general, Cajal's drawing technique was modeled on the particular staining method he used (Garcia et al., 2003). When he drew nerve cells stained with the chrome silver method, he used a pencil and Chinese ink to draw an outline of the cells, and graphite pencil when he needed relief effects (see figure 30). After the invention of his reduced silver nitrate method in 1903, he added watercolor or aquarelle to the mixed technique of pencil and ink (see figure 34). Preparations stained with the sublimated-gold method, invented in 1913, were drawn with a similar technique, but in these cases Cajal also used colored aquarelle (see figure 27).

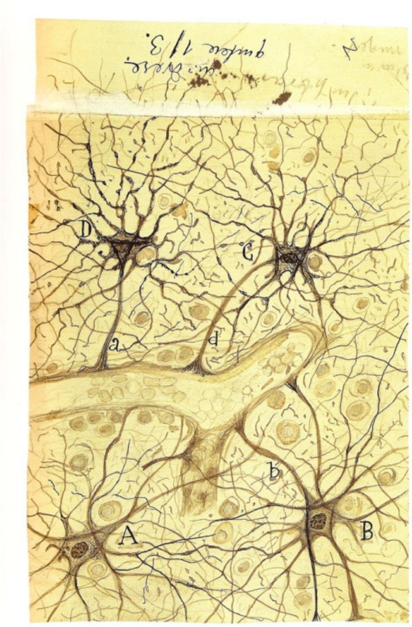


Figure 27 Neuroglial cells in the cortical white matter, stained with the sublimated-gold method (1913). 16, 6 x 12, 7 cm. Reproduced from *Santiago Ramón y Cajal (1852-2003) Ciencia y Arte*. Madrid: V.A. Impresores, S.A., p. 81.

Most of Cajal's drawings were done on used paper (for example, on the back or in the margins of letters). At first glance, this might seem odd. But most drawings were meant for publication, and needed to be translated into lithographs or wood engravings anyway (the latter was the most common method of reproduction for small-format reproduction, before photomechanical methods were introduced). Figure 28 is an original drawing. It is stored at the Ramón y Cajal museum in Madrid, together with approximately 2000 other drawings. The drawing reveals that Cajal gave specific instructions for publication in the margins. Many relate to the required reduction factor (in this case: "quitar 1/3" – remove 1/3).

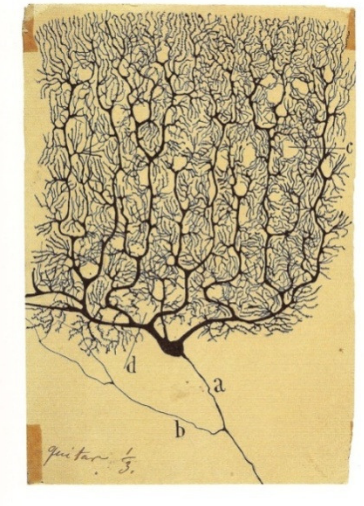


Figure 28 Purkinje cell cerebellum adult male, 1899. Original: 15,5 x 10,7 cm. Reproduced from *Santiago Ramón y Cajal (1852-2003) Ciencia y Arte*. Madrid: V.A. Impresores, S.A., 143. The drawing was published in Cajal's 1904 *Textura del sistema nervioso del hombre y de los vertebrados* II, p. 316.

Brain sections stained with the Golgi method were relatively thick, as nerve cells spread out in many directions and over long distances. Even after the tissue was stained, it took a skilled observer, using very strong magnifying lenses, to abstract discrete forms out of the complex interrelated structures. Although Cajal's objectives equaled today's best lenses, they were not flat-field, and his microscope had a relatively small viewing angle.[46] In addition, high magnification also eliminates depth of field. Observing the tissue thus necessarily entailed continual movements of the microscope's fine focus and of the slide itself. Even the smallest nerve cell ramifications constantly undulated in and out of focus (DeFelipe and Jones, 1992).

Figure 29 gives us an idea of what this may have looked like (although it does not come close to the experience of microscopic observation). The figure displays two of Cajal's photomicrographs, taken at two different focal planes. Combined, they form a stereographic representation of the cells.[47] Cajal's experiments with photomicrography will be discussed later on in the chapter. For now, it suffices to briefly compare the two images. This will give us a feel for the observation of nervous tissue prepared with the black reaction, taking place over multiple focal planes.

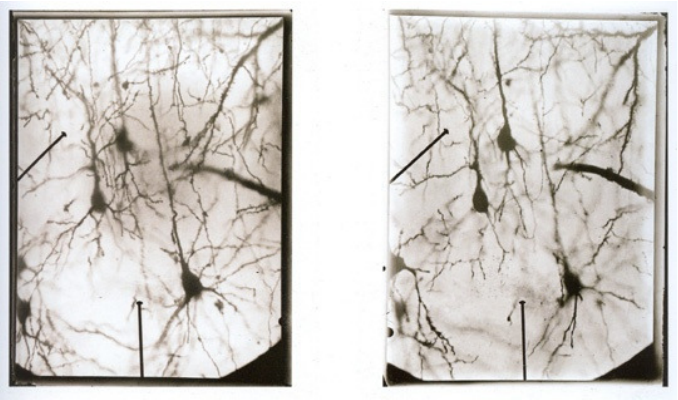


Figure 29 Digital copy of one of Cajal's biplane stereophotomicrographs of pyramidal cells, photographed with a blue and a green screen, gelatine bromide plate. Exposure time was very long in this procedure, taking from 30 minutes to an hour, while other methods took 2½ to 4 minutes. A sense of a depth is obtained by looking at the photographs using glasses with the colours green and blue reversed (anaglyphic method). Reproduced from *Santiago Ramón y Cajal (1852-2003) Ciencia y Arte*. Madrid: V.A. Impresores, S.A., p. 293.

The photographs display three nerve cells and (part of) their ramifications. The cells are surrounded by the dendritic and axonal processes of neighboring nerve cells. Together, they make out 1-5 % of that total amount of cells in the preparation. Of the top-left neuron in the photograph on the right, only the cell body, the axon, and some of the dendrites are clear and distinct. To see the other dendritic branches, we need to resort to the image on the left. The same holds for the cell in the right-hand corner below: If we follow the axon upwards in the image on the left, it loses focus immediately after the small curved shape near the middle of the image. This part of the axon is in focus in the image on the right. In addition, most of the cell's dendrites travel away from the viewer, deeper into the preparation. Resolving complete cells evidently meant that neuroanatomists constantly had to shift focus and move the slide.

Cajal was trying to arrive at an understanding of the structure and the position of nerve cells in relation to other cells, and of the communication between their minute structural components. He did so by staining them, by observing them closely, and by transferring their shape onto paper. According to some of his contemporaries, Cajal was such an experienced draughtsman, that he was able to draw by looking through the microscope with his left eye, and focusing the other eye on a piece of paper (De la Villa, 1952). However, Cajal's descriptive papers confirm that he was also a proficient user of the camera lucida. In one of his seminal, internationally oriented, papers on the contiguity of neurons, published in the journal *La Cellule*, he stated that the majority of his figures were made with a Zeiss camera lucida, and objective C of the same company (Cajal, 1891a). Cajal considered the optical device to be troublesome, but regularly resorted to it when he was dealing with large cells or large regions of the brain (DeFelipe and Jones, 1992).

The Wollaston four-sided prism,[48] better-known as the camera lucida, consists of a half-silvered mirror, tilted at 45 degrees. The device is either mounted on a fixture, and attached to the drawing surface, or mounted on for instance a telescope or microscope. The upper half of the eye looks through the mirror, the lower half at the paper. Due to the double reflection on the inside of the prism, the object under study is reflected on the retina with the right side up. An optical illusion creates a virtual image of that same object on paper, which is subsequently traced (Fiorentini, 2005).

Let us take a closer look at one of the drawings Cajal made for the *La Cellule* article (see figure 30). It does not display depth in the way we would intuitively expect it to. This would have resulted in an image in which the ramifications of the nerve cells changed from dark to light, from in focus to out of focus, while slowly disappearing into the depths of the preparation. Instead, the image is quite flat. All lines are equally in focus, and depth is only displayed by a differentiation between thick and thin lineation. This can partly be explained by the demands of the printing process. At this point in his career, he mainly resorted to wood engravings. They benefited from drawings with very clear outlines. But there was more to it than that.

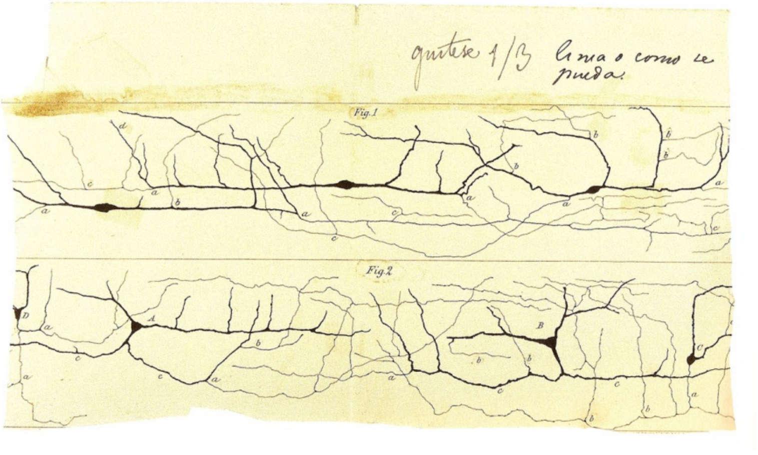


Figure 30 Longitudinal section, molecular layer of the cerebral cortex of an 8 days old rabbit. *La Cellule* (1891). Reproduced from *Santiago Ramón y Cajal (1852-2003) Ciencia y Arte*. Madrid: V.A. Impresores, S.A., p. 90.

Observing relatively thick slices of nervous tissue entailed repeatedly shifting the focal distance. It was a process that stretched out over a certain amount of time. A complete image of a cell was arrived at by putting together the pieces of the puzzle in the observer's mind, as it were. I will discuss this more in-depth in the section on photomicrography. The drawing process, on the other hand, was discontinuous. Cajal brought all undulating parts to the same focal plane, one after the other. A detailed registration of one focal plane, in which he plotted the shape and size of the nerve cells, in this case with the camera lucida, was followed by changing the microscope's fine focus, and the observation of the subsequent focal plane, etcetera. In the case of figure 34, he probably also regularly moved the preparation under his microscope, in order to get other parts of the tissue in focus. The camera lucida enabled him to draw all important structural elements with the same amount of detail, on one plane surface. The device also aided the study of the cell's morphology. It made it easier to estimate for instance the relative sizes and distances of cells, leaving Cajal with more time for analysis. In the drawings, Cajal brought out the fundamental aspects, and played down superfluous detail. These decisions were based on what he saw as the complete 'mental image' of the nervous tissue at hand. He quite literally reduced highly complex, dynamic viewing experiences into flattened, static, comprehensive images of neurons.

Paying Attention

Cajal’s drawings were material articulations of an expert selection process. But from his own perspective, there was also another side to drawing. To Cajal, drawing was much more than a technique for reproduction; it was an essential tool in disciplining the eye. This standpoint figured prominently in most of his handbooks, as we will see shortly, but also in Cajal's creative writing. One of the stories he wrote in his spare time, *Natural Man and Artificial Man*, is especially relevant for the present purpose.[49] Of course, we should take care not to equate these leisure writing activities with Cajal's own autobiography. But one of the story's main characters, Don Jaime Miralte, shares important stances on drawing and observation with his creator. In a characteristically ironic undertone, Cajal admitted as much in the preface to the stories:

> “The characters in our stories sometimes expound and proclaim the most exaggerated, contradictory systems of thought, and as one may presume, they are liable to considerable inconsistency, ignorance, and naïveté. This results from our desire that the protagonists be more like real men than symbols, and that they offer the passions, defects, and limitations of real flesh-and-blood people. Of course, the author does not accept responsibility for any of the preposterous ideas defended by these characters, not even when he fails to disguise his sympathy for the moral figure of Jaime (...).”

In the story, the reader follows a conversation between two former Spanish classmates, now in their mid-thirties, taking place on the streets of Paris. Don Jaime Miralte is a Spanish expatriate and a celebrated engineer. Don Esperaindeo Carcabuey, baron of Vellocino, is a lawyer. The baron, born to considerable fortune, is suffering from a severe identity crisis. Lately, he tells Miralte, he has come to realize that his whole protected upbringing and expensive education had amounted to nothing. Admittedly, all those years of studying the classics, rhetoric, history, religion, and scholastic psychology, catapulted him into his current career. But he now feels his life is completely built on empty rhetoric. To make matters worse, his wife recently left him, and fled the country with a long-haired dandy. Carcabuey is in Paris to find his wife, and file for a divorce. He is desperate for change.

After hearing Carcabuey out, Miralte declares that the baron is obviously a victim of "the artificial nature of education." (p. 195) But at least he realizes that he needs help. Miralte - in his turn - decides to unfold the story of his childhood, hoping it will put Carcabuey on the right track.

Contrary to Carcabuey (but much like Cajal), Miralte has always been a 'natural man'. He has spent most of his childhood outdoors, drawing whatever came before his eyes. His first and most important teacher stimulated the young boy's inquisitive nature and artistic skills. The teacher taught him early on to observe things closely, and record these observations in a notebook. He told Miralte that "to draw is to analyze, to discipline your wandering attention, to observe by correcting and meditating." (p. 201) The only way to prevent book wisdom from implanting suggestions in the mind, he told his eager student, was to observe things with your own eyes first, before turning to the books. Miralte: "When I thought inductively, it never occurred to me to ponder causal laws in themselves, converting efficient causes into final ones." (p. 199) The key thing was to look and listen, put in long hours, pay attention, and record all impressions.

Cajal could not have agreed more. Drawing was an exercise in visual acuity. He believed that "great observers are usually skillful drawers. Morphological studies, histology, anatomy, and embryology would be incomprehensible without the art of design."[50] In his *Manual de Histología Normal y Técnica Micrográfica* (the first edition was published in 1889 and the last in 1928), Cajal explained that drawing was fundamental to scientific observation and understanding. Ideally, he reasoned, the artist and the microscopist were united in one person. Although the microscopist has learned how to interpret what is seen, it was the artist that has learned how to see in the first place. He had the experience of intense, repeated, observation. He was trained to pay close attention to the world around him, and translate his observations into visual images.

The view that drawing was an exercise in attentive observation clearly also surfaced in his handbooks - by definition condensations of what he considered to be proper methodology. Cajal considered seeing clearly to be the most important quality of a scientist (Otis, 2001). In the preface to his magnum opus, the *Textura del sistema nervioso del hombre y de los vertebrados*, Cajal wrote that a scientist's discernment always risked distortion and simplification. On the other hand, he believed his drawings were scientific documents that indefinitely retained their objective value (Cajal, 1899).[51] In the same interview with a Spanish journalist mentioned in the above, Cajal explained that "to win myself any fame with a brush" he literally had to turn his hand into a "precision instrument." (S. Junquera, 2003, p. 349) He studied his preparations intensely by drawing them - over and over again. By 1900, he estimated to have made over 12,000 drawings of nervous tissue. This means a minimum of two highly detailed drawings a day.

The Illusion of Continuity

Cajal was a gifted draughtsman, but he was an inventive photographer as well. He had discovered photography as a high-school student in 1868, and developed a life-long passion for the technique. Soon after he was married (at the end of the 1870s), he had found a way to produce rapid gelatine-bromide plates - hard to come by in Spain, and very costly. At the time, Cajal regularly spent his nights pouring emulsions in his barn, "in the red glow of a lantern, in the face of the wonder of curious neighbors, who took me for a goblin or a necromancer." He soon found himself in the odd position of manufacturer of the plates. Were it not for his anatomical work, Cajal stated, he would have had little trouble setting up a 'perfectly viable industry' in Spain (Cajal, 1937|1917). He made numerous improvements to the technique, resulting in an impressive list of publications (Cajal, 1901, 1903, 1904, 1906a, 1906b, 1907a, 1910, 1912). His contributions to the field were recognized by professional photographers, who made him honorary president of the Spanish National Royal Photographic Society in 1900.

Intriguingly, photography - with its connotations of mimesis and accuracy – would appear the perfect medium to fulfill Cajal's desire to 'objectively' register observations. Not in the least because photographs are often considered as powerful immutable mobiles: all copies are identical and can be reproduced in large quantities, which allows for easy distribution and circulation (Latour and Woolgar, 1986). In light of his experience with photography, it is remarkable that Cajal rarely resorted to the technique in his neurohistological work. Javier DeFelipe and Edward Jones (1992) counted the scientific articles in which he did, and found only three. They were published well after his sixtieth birthday. My focus in this section will be on two of Cajal's technical papers, entirely devoted to photographing nervous tissue (Cajal, 1907b, 1918). These contributions to the field have thus far largely gone unnoticed. The papers - one in Spanish, the other in French, twenty to thirty pages each - were never translated into English. Neither were his general papers on the photographic process, nor his book on color photography.

Both papers contain detailed clarifications as to why photography proved incapable of effectively representing nerve cells. In the previous chapter, I have argued why the same applied to macroscopic representations of the brain (see also De Rijcke, 2008a). Cajal explained in the 1907 paper that photographs were only comparable to good drawings when the slices of nervous tissue were very flat and thin (between 1 and 10 μm). But problems arose, he continued, when the histological slices were thicker, as is the case with nervous tissue. "Under these conditions," he wrote, "a photograph is so incomplete, that one has to give up using it in lieu of a drawing." (Cajal 1907b, 24) A photograph can reproduce only one focal plane at a time. In order to observe complete cells, collapsing multiple planes was crucial.

At the time, binocular microscopes were not well-adapted for critical work with high powers. As Frederic Ives stated in the *Transactions of the American Microscopical Society* (1903), expert microscopists were more or less able to see in depth and determine structure with a monocular microscope by focusing successively upon different planes and thus deriving from a series of observations what he called a concrete ‘mental image’ of the object. According to Ives, the capacity to do this depended upon an inherent ability which some people possess more than others. It was partly an unconscious process, relating to a peculiarity of human perception. While the human eye automatically adjusts for changes, a photograph slices this mental phenomenon up (cf. Canales, 2002). Cajal was familiar with Ives’ work. His own views on the process of 'direct' observation also exploited certain features of vision, to demonstrate the microscopist’s expert eye.

> “In order to understand the defects of this method of photomicrography it suffices to reflect briefly on the difference between the examination of a slice, and the photographic synthesis of different planes. When we examine an histological slice, we see multiple planes of the object in a perfectly detailed manner, because we can make rapid adjustments and have learned to disregard the parts that are out of focus (…). A certain mental phenomenon reinforces the focal region, due to the effort of attention, and leaves room to ignore all the objects that present itself to the retina beneath circles of diffusion.”

Instantaneous photography recorded what could be seen in one focal plane, while direct drawing by an expert observer had added value: he added his experience (the result of sustained attention) in averaging over more planes. As both Otis (1999) and Daston and Galison (2007) have argued, Cajal had a particular reading of the term ‘attention’. His reading was grounded in late-nineteenth century psychological writings, in which an effort of attention was tantamount to the exercise of will, or arduous duty. Making visible required both intelligence and self-restraint. Only an expert observer could do it.

In both the 1907 and the 1918 technical papers on photographing tissue, Cajal described three potential photographic solutions to the problem of multiplane representation: composite photography, stereophotography, and cinematography. In composite photography, multiple images are combined to form one final image (notice the similarity with the drawing method discussed above). Cajal used a technique similar to that invented by Francis Galton in his anthropometric studies

(Galton, 1883). Galton believed composite photography could help identify ‘types’ of people by their appearance, by measuring the individual’s deviation from an average type. Cajal exposed multiple photographs taken at different focal planes to a photographic plate, with the respective exposure time for each image made in relation to the number of planes, in his case three. But this method, he claimed, only resolved the problem up to a certain point. He demonstrated the result in a figure (see figure 31), followed by the comment that the result – though not completely negligible - was unsatisfactory, also taking into account the amount of work needed to complete the process of obtaining three photographs (Cajal, 1907b).

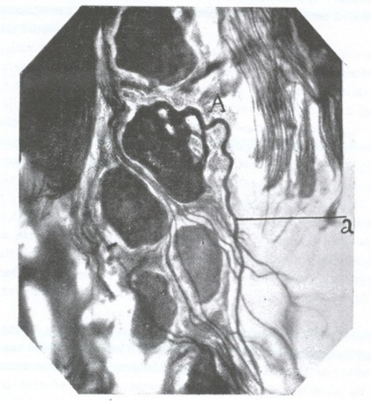


Figure 31 Ganglion, plexiform layer nervus vagus adult dog, composite photomicrography (Cajal, 1907b).

In the 1918 paper on photography, Cajal added two other photographic solutions to the problems of limited depth of field: stereophotomicrography and cinematography. The first rested on the principles of binocular vision. Stereoscopic images create depth by presenting each eye with a slightly different representation of the same object, similar to the deviation in natural binocular vision. Cajal summed up eleven ways to obtain stereophotomicrographic images of cells on the same focal plane, and also described experiments with biplane and multiplane stereophotography, where the images were taken at different focal planes (see figures 32 and 33). Biplane images were juxtaposed as in regular stereoscopy. The eye synthesized the two images, filling in and correcting the imperfections of the separate images. But multiplane photography had an obvious drawback: the eyes cannot process more than two images simultaneously. When the number of planes multiplied, a cinematographic device was indispensable, according to Cajal. Only then could all focal planes in a section be reproduced, by projecting successive photomicrographs on a white screen.

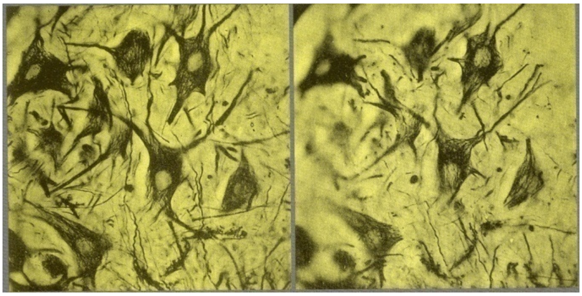


Figure 32 Neuron spinal medulla 8 day old cat (Cajal 191, reproduced with permission of the Cajal Institute, Madrid).

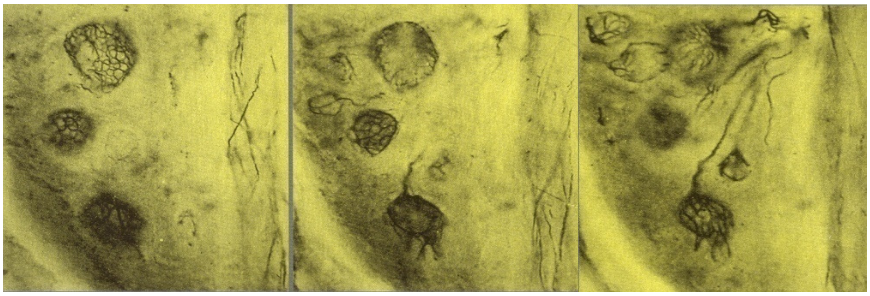


Figure 33 Leech ganglion, triptych, multiplane method (Cajal 1918, reproduced with permission of the Cajal Institute, Madrid).

Cinematic devices were introduced in the labs of scientists as one in a range of techniques of inscription and visualization already in laboratory use (kymography, microscopy, photography) (Cartwright, 1992). They re-create an illusion of movement, but Cajal voiced the advantages of cinematography slightly differently. Cinematographic methods provided the viewer with an illusion of continuity, Cajal wrote. This second kind of illusion invoked much more of the complicated process of observation, stretching out over space and time. If you time the sequential cinematographic process right, Cajal believed the same profound sensation could be obtained of the relative depth that he saw when examining a microscopic slice over a certain period of time.[52] He thought there were two means to fulfill his cinematographic ambition. He first referred to a simplified device similar to “Marey's” old photographic revolver - although it was actually Janssen who invented the photographic revolver in 1874 (Coissac, 1927). With this device he could project multiple photomicrographs that were recorded on a turning glass disc. The project was never actually pursued, perhaps because adding a microscope to the technical set-up highly complicated matters. But Cajal had a second option: the Gaumont cinematograph, at once camera and projector. The results from experiments with this device were reasonably favorable, according to Cajal. He did suggest adapting the device to his particular requirements.[53] In the conclusion, Cajal mentioned writing a follow-up article, pending the special cinematograph “now under construction at the Laboratorio de automática of the distinguished engineer senor Torres Quevedo.”[54] Cajal was convinced that the cinematographic method was “the only solution to the problem of photographing thick slices, projected through powerful lenses.”

Cajal’s cinematographic experiments remained at an experimental stage. Nevertheless, they clearly demonstrate that he was after a photographic re-creation of the neuron not in three, but in four dimensions. This was the only way to render complete cells and their boundaries.

The Power of Presence

Cajal's illusion of continuity and the difficulties of transferring to film what was visible through the microscope, brought to the fore the unique spatiality (depth) and temporality (continuity) of ‘direct’ observation. The drawbacks of photography, and his preference for drawing as a representational tool, reveal what Cajal believed to be crucial ‘ways of knowing’ (Pickstone, 2000). In addition, his epistemic stances on drawing and photography bespeak of the strains and exigencies of scientific visualization. Cajal intervened with the nervous tissue by staining and drawing, foregrounding different aspects of the material at hand, while putting in long hours of attentive observation. A question that has thus far been left unanswered, is how such materially and locally inscribed observations would travel to other labs.

Interestingly, it took a while for Cajal's observations to be noticed by his foreign peers. Bear in mind that Cajal saw his first preparation stained with the chrome silver method in 1887. His subsequent comparative studies on the cerebellum, spinal cord, brain stem, retina, olfactory bulb, and peripheral ganglia, led him to argue against the nerve net hypothesis. At the beginning of 1888, he launched his own journal, *Revista trimestral de Histología normal y patológica*. The journal enabled him to publish his stream of new findings, both in writing and visually, at a quick and steady rate. Most copies were sent to his colleagues in England, France and Germany. Unfortunately, the impact was negligible: nobody knew Cajal, and most neuroanatomists were not experts in reading Spanish.

Because of this language barrier, it is perhaps understandable that not all of Cajal's peers read his articles. But does this also hold for the accompanying drawings? On the face of it, they seem the perfect means to communicate new findings. Evidently, one need not speak Spanish to understand them. Philosopher of science Bruno Latour and others have maintained that images – as immutable but mobile embodiments of knowledge claims – have the ability to create assent in scientific communities (Burri and Dumit, 2007; Elkins, 1999; Latour and Woolgar, 1986; Lynch and Edgerton, 1988). Arguably, they could serve as 'vehicles of mediation' between different local laboratories (Roberts, 2007). But *de facto* neither Cajal's publications, nor the drawings accompanying his articles, would make all the difference. In order for Cajal to enforce an international breakthrough, he had to use up all his savings, pack his Zeiss microscope, gather his best preparations, and set out for Germany.

In October of 1889, the third annual conference of the *German Anatomical Society* was held in Berlin. Here, in front of an esteemed group of colleagues, Cajal did manage to direct attention to his work. In his autobiography, he described how he had to physically force his colleagues to take a look at his preparations. In a witty paragraph, he portrayed himself as a lonely outsider. He described mustering all his courage to walk over to Albert Von Kölliker (the doyen of microscopic neuro-anatomy), grab him by the sleeve, and seat him behind his microscope (Cajal 1917|1937, 356). Kölliker, surprised by the clarity, quality, and quantity of Cajal's preparations, asked how many men had worked on them. Cajal remembered uttering that it was his work, and his work alone. Other colleagues followed Kölliker's example, and observed Cajal's preparations. Every single one of them was baffled by the sight. Gustav Retzius, also present, remembered the occasion as follows:

> "I shall never forget the overwhelming impression that the demonstration by Cajal (…) of a large series of his preparations produced upon those of us who were especially interested in the subject. Albert von Kölliker and I were enchanted by the sight of the preparations which Cajal placed before us. Both he and I were converted and we started home to begin working afresh with Golgi's method, which was not in great repute among other anatomists of the day." (Retzius, 1908)

The conference in Berlin was crucial for the dissemination of Cajal's work. It also redirected the focus to the chrome silver method. Most of the neuroanatomists who attended the conference had already tried the black reaction in their own laboratories. They had first-hand knowledge of the work Cajal had put in his preparations. And they knew exactly how many failures preceded every successful section. Cajal had installed his own Zeiss microscope in a special demonstration area, next to some other microscopes he had seized for the event. He had taken care to select the right lenses, and had perfectly adjusted the devices beforehand. This allowed him to show a whole series of excellent preparations, and explain – in broken French – which adaptations to the chrome silver stain had given the best results. The physical transportation of a box full of Cajal's own preparations, and his quite heavy microscope, increased the chances that his colleagues would have exactly the same view he himself had on neuronal structure. This was the big difference with sending out a journal article. In a sense, Cajal never really left his lab, but moved it to Germany.

At this point in his career, Cajal's drawings played a modest role in the communication of his observations. This was about to change.

Building Bridges

In the years following his persuasive performance in Berlin, Cajal spent most of his time in his laboratory. While continuing to publish on the spinal cord and cerebellum, he also began to experiment with cerebral cortical tissue. Investigating the cerebral cortex was difficult. The cellular arborizations in the cerebellum and the other nervous structures that Cajal and his colleagues had researched thus far, were located in specific layers. But the network of terminal branches and ramifications in the cortex is extremely dense and expansive (see figure 34).

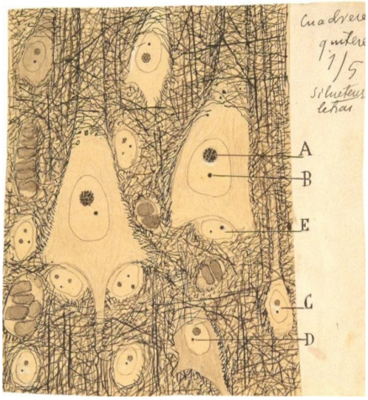


Figure 34 The network of terminal branches and ramifications in the cerebral cortex. Reproduced with permission from *Santiago Ramón y Cajal (1852-2003) Ciencia y Arte*. Madrid: V.A. Impresores, S.A., p. 349.

Like most microscopists, Cajal had developed - and continued working on – his own specific ways of handling nervous tissue. In the case of cerebral cortical tissue, for instance, he regularly painted the surface with the animal's blood or with gelatin. He did this in order to promote staining, and reduce the chance of artifacts (Cajal 1891a; 1894b). Cajal’s experience with manipulating tissue, learning when it resisted observation and when it gave in, taught him how to see. But this mutual shaping of the material and the observer can also create a distance between researchers.

After the conference in Berlin, Cajal's work found its way to the international podium relatively quickly. His colleagues were now paying attention to his work, and had started replicating his results in their own laboratories. In the years 1890 and '91 alone, Cajal published 28 articles. The articles were published in Spain (20), in Germany (5, all in French), in France (2), and in Belgium (1).[55]

Among all the structural details he put forth,[56] one in particular caused quite a stir. Cajal's observations with the chrome silver method had convinced him of the existence of minute spines on dendrites, which purportedly ended in small knobs. The dendritic spines and their knobs were literally on the edge of what could be made visible at the time (the knobs ranged from 0.01 to 0.8 micrometer). Cajal claimed to have observed the contiguity between the spines, and young, unmyelinated axons of other nerve cells. Proving that dendritic spines ended freely was essential for his theory of communication by contact (instead of continuity).

Cajal first described the dendritic spines in the cerebellum in 1888 (Cajal 1888a). Two years later, he presented similar observations on cerebral cortical tissue in the *Gaceta Médica Catalana* (Cajal, 1890a). After this paper, a number of others followed in which he devoted particular attention to the spines (Cajal, 1891a,b, 1892, 1894a, 1896a,b, 1933). For instance, he published a larger monograph on the cerebral cortex in the Belgian journal *La Cellule* in 1891, containing detailed drawings of the spines (see figure 35). Some of these drawings were also published in the shorter communication of 1890.

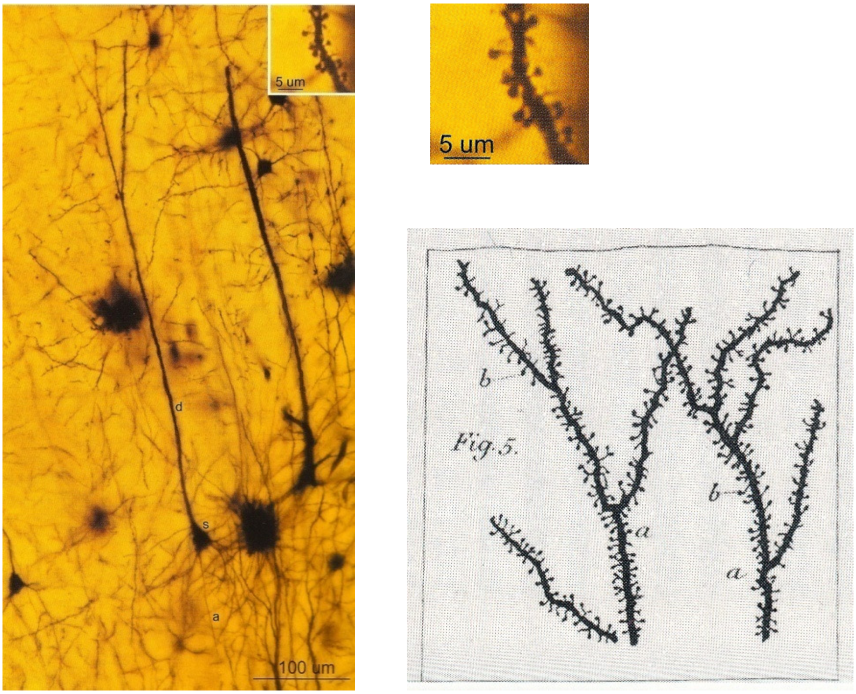


Figure 35 Left: digital photograph of one of Cajal's preparations, housed at the Museo Cajal. Reproduced from *Santiago Ramón y Cajal (1852-2003) Ciencia y Arte*. 2003: 296 Right: one of the camera lucida drawings in Cajal (1891). "Portion of the terminal arborization of an [apical dendritic] shaft of a pyramidal cell of the adult mouse. 1.30 Zeiss apochromatic objective. *a*, shaft and dendritic branches; *b*, collateral spines." Reproduced from DeFelipe & Jones (1988, p. 28-29).

A year later, the transcriptions of a lecture series at the *Academy of Medical Sciences of Cataluña* were issued. They were translated into German, French, and English. Additionally, in 1896, he devoted a special study to the dendritic spines, in which he compared results obtained with the chrome silver method, to preparations stained with the *intra vitam* methylene blue stain. This paper was also translated into French. And finally, thirty-three years after the first paper, at the age of eighty-two, he wrote down and drew his final view on the neuron doctrine. It contained a large section on the spines, in reaction to a belated attack on the doctrine by Held.

Cajal's 1890 paper described and visualized observations with the Golgi method on the cortex of rabbits, cats, rats and mice in various stages of development (newborn and 15-to-30-day-old tissue). His experiments had led him to focus particularly on the morphology of the mammalian pyramidal cells. This was attractive, because it created opportunities for far-reaching conclusions. The pyramids, named after their triangular shape, were located in the outer layer of the cortex.[57] They had the most numerous and differentiated dendrites, and the greatest and most extended axons and branches. Arguably, they could therefore influence, and be influenced by, a great number of other cells.[58] Cajal believed that their dendrites stood in intimate contact with numerous small nerve fibres of the first or molecular layer, without fusing together.

The selectiveness of the chrome silver method, in combination with the underdeveloped tissue, provided Cajal with brownish-black, diagrammatic images of cells. As I have described above, his drawings were further abstractions of his microscopic observations. Cajal's particular methodology, together with his classifications of the resulting diagrammatic shapes, were developed in tandem with the formulation of thought-provoking hypotheses. By 1891, Cajal felt confident enough about the observed differential properties of the protoplasmatic and neural processes to communicate a theory of the distribution of nerve impulses. This sequence of events fitted with his firm belief in inductive methods: hypotheses of underlying causes or physiology could only arise slowly, after attentive observation and description (Cajal, 1999).

“It is impossible to contemplate with indifference, as do certain authorities, this admirable relationship established between the nerve fibres of the first layer and the terminal parts of virtually all the pyramidal cells of the cortex. This connection, which constantly demonstrates the same appearance in all mammals in all regions of the cortex (…), must have great importance in the functioning of the brain. (…) In our earlier works, we have considered the dendrites of nerve cells, not so much as the providers of nutrition or absorption, following the opinion of Golgi, but rather as arrangements permitting the establishment, by multiple contacts, of the communications of nervous action either between neighboring cells or between cells at a distance. Whenever nervous transmission occurs over a great distance, we have to admit, in which we rely upon examples, that the propagation of the nervous excitation occurs between the dendritic arborizations on the one hand and the unmyelinated nerve fibres on the other." (Cajal, 1891, translated in DeFelipe & Jones, 1988)

Cajal's theory was based on the assumption that nerve cells were not only morphologically but also physiologically distinct units. His *law of dynamic polarization* stated that cells are polarized: the nervous impulse travels through the dendrites, via the cell body, and leaves through the axon, which in its turn excites a new dendritic apparatus.[59] The compelling abstraction of neuronal form through staining and drawing, described in previous sections, brought about similarly reductionist interpretations as to the brain's physiology (Kwint and Wingate 2006). This also fed back into even more diagrammatic images. Drawings such as figure 36 had a three-fold purpose. Not only were they testimonies of his adept use of the chrome silver method, and his artistic skills, but they were also used to expound his neuronal theory. The delicate brownish-black cells and their ramifications are visibly communicating by way of contiguity, not continuity (see for instance E and H). Cajal used arrows to indicate the directional flow of nervous conduction.

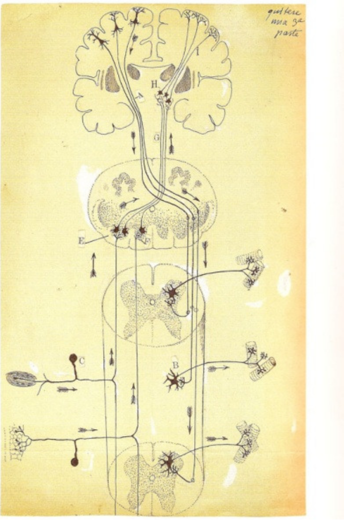


Figure 36 Schematic representation of the law of dynamic polarization, 1899. "The protoplasmic expansions and cellular body possess a form of axipetic conduction (in other words, towards the axon); while the axon possesses a form of dendrifugal and somatifugal conduction (in other words, that comes from either the dendrite or the cellular body)." (Cajal 1899, 88). Reproduced with permission from *Santiago Ramón y Cajal (1852-2003) Ciencia y Arte*. Madrid: V.A. Impresores, S.A., p. 194, quote on page 362.)

Some of the colleagues who had seen Cajal's work in Berlin, were now keeping a close eye on his new work. For instance, his first paper on the cortical spines was sent to Stockholm, home of Gustav Retzius. Not long after receiving the paper, the Swedish histologist corroborated Cajal's observations (Retzius, 1891). Retzius expressed his skepticism about the description of the protoplasmic processes by Golgi, and his pupil Martinotti. According to Retzius, there was no connection between the fine branched prolongations and either glial cells or blood vessels –as Golgi and Martinotti had maintained.

> "On the contrary: in a well-stained preparation – I have been able to study it beautifully in young rabbits and dogs - one can witness the processes striving upwards, and split repeatedly into rich ramifications that continue up to the surface. All these dendritic spines are furnished with knobs (…)." (Retzius, 1891, p. 97-98)

Like many of his colleagues, Retzius had not been very successful in applying the chrome silver method in the past. He witnessed his 'well-stained preparation' only after Cajal had passed on the appropriate methodological formula for relevant adaptations to the chrome silver stain (Cajal, 1901-1917|1937, p. 398-9). Von Lenhossék, too, had been encouraged by Cajal's performance in Berlin to re-try the Golgi method. He confessed to the Spaniard that he had stopped using it, because of recurring technical mishaps. The same held for Van Gehuchten, professor of anatomy in Louvain.[60]

Incidentally, one searches Retzius' paper in vain for an official reference to Cajal's article. It was enough to refer to one of Cajal's drawings:

> "As can be seen in Cajal's newly communicated little figure (from the mouse) [figure 34 in the current chapter], this scientist interprets the extensions in a similar vein. (…) [The extensions] do not anastomose, but nestle themselves through each other and often follow their course at a tangent." (Retzius, 1891, p. 98)

Retzius' rather off-handed style suggests that by this time, the drawings certainly traveled. At the start of his career, apparently crystal-clear images of neurons, standing out from the Spanish text, did not attract the attention of his colleagues. But this had changed: he was now an acknowledged member of the neuroanatomical community. In Cajal's own lab, his drawings steered the process of observation and the formulation of hypotheses. They also focused his attention. In communication with other researchers, they were used to deduce how Cajal interpreted his observations. Cajal, Retzius and others used each other's images as maps to guide their own visualizations, to calibrate the other’s perception, attention, and action.

Aligning the Collective

By 1896 many of Cajal’s colleagues, including Retzius, Schaffer, Edinger, Azoulay, Berkley, and Monti, had corroborated and illustrated the dendritic spines. But there were also reservations toward Cajal’s evidence and the use of the black reaction, ranging from subtle to impudent (Hill, 1896). Kölliker, for instance, who was a supporter of Cajal and the Golgi technique, was not convinced of the existence of dendritic spines:

> “In my experience, adult humans and certain mammals (horse, dog, cat, and rabbit) do not have little knobs on the spines of the pyramidal cells, but I did see them in young mammals (…) [see figure 37], and therefore I think the spines are stages of development, and the knobs are artifacts.”[61]

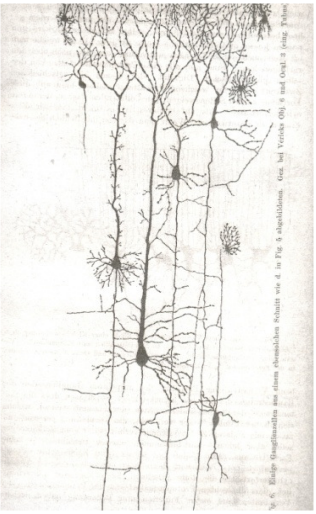


Figure 37 Ganglia cells, drawn with Verick’s objective 6 and ocular 3. Kölliker, 1896. Handbuch der Gewebelehre des Menschen, 6th ed., vol 2, part 2, figure 6.

Other histologists also persisted in rejecting their existence, because they were unable to stain the collateral spines with a different staining method: the methylene blue stain (c.f. Dogiel, 1896; Meyer, 1896a-b, 1897). In 1896, Alexander Hill, M.D. at Downing College, Cambridge, gave his Presidential Address to the Neurological Society. The address was entirely devoted to Golgi’s method. Hill was not a big advocate, to say the least:

> “The great conclusion deduced from observations made with the chrome-silver method is the discontinuity of the end-brushes of fibres and the ramified processes of nerve-cells. This anatomical conclusion is of profound interest to all who endeavor to explain the action of the nervous mechanism, whether physiologists or psychologists, since (…) it is completely subversive of all existing notions of nerve-conduction. (…) It is impossible for a microscopist to distinguish, in a preparation stained black, between contact and continuity of substance.” (Hill, 1896, p. 25-27)

These refutations attested to the fragility of the network of embodied knowledge and skills that was needed to see what Cajal saw. The fact that it was quite common for neurohistologists to draw different conclusions from very similar preparations (same staining technique, same technological equipment, same area in the brain, etcetera) was reason for them to worry about the influence of theory on observation. Corroboration of the other's observations included scrutinizing the (visual) representational accuracy of the microscopic scene, of the instruments he used, and of the quality of the stain in relation to the object of inquiry (Cartwright 1995). In addition, trust needed to be afforded to his vision and expert eye. If one of these factors was left out of the equation, the shaping of the discrete nerve cell as an object of scientific interest risked failure.

Cajal was aware of Kölliker’s reservations toward the existence of dendritic spines, and Hill’s complete rejection of the claims based on the black reaction. In the same year that Hill and Kölliker wrote down their critical evaluations of the chrome-silver method and the structural components that it was said to corroborate, Cajal published two other papers on the dendritic spines. He compared results obtained with the black reaction, to those obtained with a control-stain, based on methylene blue (Cajal 1896a-b).

It was not the first time that Cajal had used a different staining method to obtain similar results (cf. Cajal, 1890b), but in hindsight the experiments with methylene blue were considered as the definitive demonstration of the existence of the spines, and - consequently - of the neuron theory (García-López, García-Marín and Freire, 2007).

This did not mean that Cajal had convinced everyone. Ten years later, Camillo Golgi once again tried to settle the dispute in favor of reticularism. In his Nobel Prize acceptance speech, Golgi seized the opportunity to reintroduce his theories on the diffuse nervous network – much to Cajal’s aggravation. In his autobiography, published eleven years after the Nobel Prize ceremony, he accused his Italian colleague of suffering from a “strange mental constitution.” (Cajal, 1937, p. 553) Obviously, Golgi’s observation skills were seriously hampered, Cajal argued. How else could one account for the fact that Golgi believed dendritic spines and their small tips were artifacts of his own staining method?

Cajal applied a similar line of reasoning in 1933, a year before his death, when he published his last book, *¿Neurismo o Reticularismo?* Among many other things, he used this platform to disprove the latest attack on his neuron doctrine, and publish several new drawings of the spines (see figure 38).

Figure 38 Types of collateral spines in the rabbit, a two-month-old child, and a cat (fully developed spines and dendrite before the knobs are formed).

In a 1929 paper, Held had maintained that the axons had *Endfüssen* or terminal boutons, ending on the cell body and dendrites, linking pyramids with one another and with the nerve plexus. Cajal reacted by stating that Held obviously had not been keeping up with his field; a continuity between axons and the spines was never confirmed. Cajal claimed that Held's preparations (judged by Held's illustrations) did not display *characteristic* spines, but "accidental unions at times produced by silver chromate, deposited between the spines of proximate cells and the passing fibres."[62] Obviously, Cajal argued, Held had not been able to discriminate between artifacts and actual morphology.

More generally speaking, Cajal countered attacks against his neuron doctrine in three strokes, all wrapped in polite sentences. Firstly, he pointed out that his opponents had never succeeded in providing a solid theoretical foundation for reticularism. Secondly, he stated that his peers lacked the ability to observe properly, because they were clouded by their theoretical preconceptions. And thirdly, he continuously staged his morally authoritative scientific persona. He was the only one who combined strenuous observation with dedication, perseverance, and unrelenting attention.

> “Without vainglory I can consider myself in this case a witness of the highest exception because I have dedicated more than 30 years to applying the method of Golgi to the cerebrum and other nerve centers with a perseverance which could be equaled by others, but difficult to surpass. Now, I have never seen anastomoses between the spines and the nerve fibres despite the fact that I have devoted particular attention to them since 1888. (…) I know well that in the realm of science that which is obstinately looked for is usually found; but when that which is not looked for establishes a frequent distribution and appears in all clearness it finally arouses the attention which was most distracted and most preoccupied with other problems.” (Cajal, 1954, p. 98)

As I have demonstrated above, Cajal believed that there was an important role cut out for drawing in this channeling of attention. It took more than plain registration for a microscopist to capture the cell's essence. He needed to really attend to his subject, in a controlled effort of hand, eye and mind.

Conclusion

In 1981, philosopher of science Ian Hacking argued that microscopists learn to move around in the microscopic world by seeing and doing. He compared developing expertise in microscopic observation with learning how to see in three dimensions (as a baby, for instance). Stereoscopic vision is not an inherent quality of our 2-D retina. It is an acquired skill, obtained through active intervention with the world around us. Similarly, Hacking maintained, microscopists become convinced about their observations because they interfere with their material “in quite physical ways.” (Hacking, 1981, p. 152). My analysis of Santiago Ramón y Cajal’s practices of observation and visualization illustrates the continued relevance of Hacking’s argument. Cajal’s materially mediated interventions largely defined how the neuron gained reality in the years after he familiarized himself with the chrome silver method. His experiments with nervous tissue resulted in highly particular ways of seeing and visualizing brain cells. In this chapter, I have analyzed the construction of the Spaniard’s eye-catching, diagrammatic visualizations of brain tissue. I have described why Cajal considered drawing to be by far the best rendition method. Not only did drawing facilitate the analysis, it also taught scientists how to see. Drawing disciplined the scientist’s eye. Furthermore, Cajal’s experiments with photography and film revealed the importance of collapsing multiple planes, in order to witness complete cells and distinct nerve endings. According to Cajal, photography was unsuitable, because it sliced up the mental phenomenon that abstracted over multiple focal planes.

Cajal's results were established in his own lab, with his own specific ways of handling nervous tissue, and his own instruments. In the remainder of the chapter, I have explored how Cajal's observations were translated into a collective viewing experience, and the role of his visual representations of nerve cells in this translation process. In order for him to enforce an international breakthrough, sending out illustrated articles was not enough. He had to pack his own microscope and preparations, and set out for a conference in Berlin. In a sense, Cajal needed to create an environment that was quite literally like his own lab. This stresses the fact that what needed to circulate was a way of seeing, that had spatial, temporal, theoretical, and material dimensions.

In the years after the conference in Germany, his drawings did play a crucial role in a controversy over the existence of dendritic spines. Cajal and his colleagues used each other's images to guide their own visualizations. They calibrated the appropriate perception and attention.

The question of connectivity of neurons was strongly tied to particular practices of observation and intervention, with its specific representational techniques, epistemological approaches, and modes of communication. This came to the surface forcefully in the 1920s, in the final stages of Cajal's career. The neurohistological field had shifted its focus toward cytoarchitectonics. This was the study of the arrangement of neuronal cell bodies in the cerebral cortex, and its functional subdivisions. Cajal felt that this was not a direction worth pursuing, because it sacrificed what he believed to be the "purest ideal of neurohistology [determining the morphology of the cerebral neurons] for the mere task (...) of physiological localization from the applied point of view." (DeFelipe and Jones, 1988, p. 521). The quote is from an article on the visual cortex of the cat. Cajal managed to get it published in the *Journal für Psychologie und Neurologie*, edited by one of the foremost representatives of cytoarchitectonics: Oskar Vogt. But this had not been easy. The Swedish neurologist Salomon Henschen sat on the board of Vogt's journal, and probably acted as one of the reviewers. He reproached Cajal for providing overly sketchy drawings of the human calcarine fissure. Challenged to defend his representational techniques, Cajal referred to his images as faithful and accurate representations of first-rate preparations. Unsurprisingly, this visual methodological discussion coincided with a theoretical conflict of interests. Cajal's career spanned more than five decades. He had persisted in paying attention to the neuron's emerging properties. He had foregrounded particular elements in the tissue through staining and drawing. He had continuously argued why it was worth doing so. But at the end of his career, his colleagues had shifted their attention to other topics. They had different ways of interfering with the microscopic world. They were asking different questions, and were building different ontologies of the nervous system. Unfortunately for Cajal, they were no longer interested in the ways of the neuron.

CHAPTER 4

Connecting the Brain.
Visualizing White Matter in Digital Visual Culture

A thought experiment. Suppose Santiago Ramón y Cajal was not born on the first of May, 1852, but a century later, in 1952. Suppose he would once again devote his entire career to neuroanatomy, and suppose that his career came into full stride when he was in his 30s. What would Cajal's practices of observing and visualizing the brain look like? In what way would his daily routines differ from those of his nineteenth century counterpart?

Obviously, the times have changed considerably. This time around, Cajal would most likely be working in a laboratory equipped with computer-assisted anatomical imaging technologies. Techniques such as magnetic resonance imaging (MRI) would allow him to examine the depths of the living animal or human brain (one of contemporary neuroimaging's 'unique selling points'). They would probably demote his regular visits to orphanages, or the habitual slaughter sessions in his backyard. There would probably *be* no lab with a backyard.

Beyond any doubt, the 'new' Cajal would be absolutely captivated by the extraordinary visual appeal of today's brain images. A we have seen, his nineteenth century ancestor was a virtuoso at applying complicated histological staining methods to his anatomical preparations. By perfecting Golgi's chrome silver method, he managed to visualize only a small percentage of complete nerve cells, follow the extended pathways of neuronal axons, and translate these observations into beautiful drawings. Assuming that his twentieth century descendant inherited Cajal's visual acuity and neuroanatomical curiosity, he would most likely be very interested in a technology that makes virtual reconstructions of every fibre in the living brain (see figure 39). The technique is called diffusion-weighted magnetic resonance imaging (DWI), and will be the focus of this last empirical chapter. DWI provides information on the connective tissue in the brain.[63] The possibility of linking this structural data to functional imaging studies significantly contributes to its status as a promising new imaging technology. I will use the advancement of DWI in the neurosciences to argue that, at present, established stances on what is considered 'objective' brain imaging are gradually starting to change.

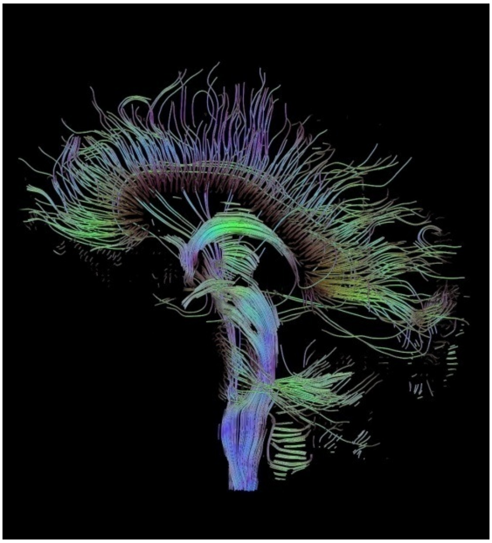


Figure 39 White matter tracts in the human brain. Reproduced from www.piaggio.ccii.unipi.it

One thing Cajal would certainly have to get used to, is that computer-generated imagery appears to relocate vision "to a plane severed from the human observer."

> "Most of the historically important functions of the human eye are being supplanted by practices in which visual images no longer have any reference to the position of an observer in a 'real', optically perceived world." (Crary, 1990, p. 1-2).

Diffusion-weighted images do not originate from the reflection or absorption of light from an object as projected onto a sensitive surface, such as the retina, a microscopic lens, or photographic paper (Prasad, 2005). As such, the images of white matter at issue here, do not seem to have an 'optical' relationship to the anatomical structure they represent, nor to the position of an observer. One of the purposes of this chapter is to describe the implications of this ‘abstraction of the visual’ (Crary, 1990), by analyzing the first atlas to contain diffusion-weighted images of human white matter, plus the manual of the software used to create the images.

I have described in previous chapters that there is a lot at stake in the creation of atlases. Among other things, they make public the aspirations of the scientist who produces the atlas, and reveal preferred epistemic and ontological stances of a scientific field (Daston & Galison, 2007). Atlases are “repositories and enforcers of objectivity – a concept that dances (like its scheming twin normativity) on the border between what we know to exist and how we ought to know it.” (Beaulieu, 2001, p. 638)

The *MRI Human White Matter Atlas* (Mori et al., 2005) was co-produced by researchers at the Johns Hopkins University School of Medicine, Department of Radiology, and the F.M. Kirby Research Center for Functional Brain Imaging (Kennedy Krieger Research Institute).[64] The atlas consisted of high resolution two- and three-dimensional visualizations of the major human white matter tracts. Many prominent white matter bundles were characterized long ago (cf. Dejerine, 1895; Flechsig, 1920), but only at its core regions. Key players in the field of neuroanatomy attach importance to Mori’s atlas, because they believe it succeeds in visualizing three-dimensional deep white matter structure (Oishi et al., 2008).

In what follows, I will draw on the *MRI Human White Matter Atlas* to investigate the implementation of new digitally mediated visual knowledge of white matter structure. In addition, I will analyze the software manual used to create the atlas images. This serves to demonstrate how the atlas was explicitly placed in the pictorial tradition of neuroradiology - with an emphasis on the transparent immediacy of a photographic realism.[65] By placing the images in this tradition, the authors opened the door to the ‘hypermediated’[66] diffusion-weighted images at the heart of the atlas. The term ‘hypermediacy’ was coined by Jay Bolter and Richard Grusin in their book *Remediation. Understanding New Media* (MIT Press, 2000). It refers to a "style of visual representation whose goal is to remind the viewer of the medium" (p. 272). In the intriguing images of white matter structure, I will argue, a sense of immediacy is achieved not by *erasing* but by *accentuating* their mediatedness. But I will first provide a brief overview of the basics behind diffusion-weighted imaging, and its development as one of the parameters of magnetic resonance imaging. This is the topic of the next section.

Measuring White Matter

Magnetic resonance imaging has been around for over four decades now. Rooted in the measurement technology called nuclear magnetic resonance (NMR), the technique is based on the fact that the nuclei of atoms spin on an axis. In the 1950s, it was found that when certain atomic nuclei are placed in a magnetic field, they absorb energy from radiofrequency pulses, and re-emit a signal when returning to equilibrium. This process of releasing their excess stored energy came to be known as 'relaxation'. The purpose of NMR was to measure relaxation parallel to the magnetic field and/or in the transverse plane. Both occur exponentially with time constants T1 (longitudinal relaxation) and T2 (transverse relaxation) (Mansfield & Morris, 1982).

MRI was made available in the 1960s and early 1970s as a method to differentiate where, spatially, NMR signals came from in bodily tissues (Blume, 1992; Dussauge, 2008; Joyce, 2008; Kevles, 1997). Basically, a very powerful computer processes the nuclear magnetic resonance signals, represented as mathematical data, and translates them into visual representations of the body (part) in the scanner. The specific frequency of resonance varies with the strength and direction of the magnetic field, and with the particular tissue being imaged.[67] These varying signals can be visualized, and give a different contrast in the resulting image. For instance, 'normal' and 'abnormal' tissue are said to resonate differently, as are gray matter and white matter.

Early on in the history of MRI it was found that, unlike other imaging technologies, MRI can use additional parameters for image production, besides relaxation times. For instance, MRI enabled the visualization of the diffusional properties of water molecules in the body. It was found that these properties vary in areas of the body where the movement of molecules is restricted. In the early 1980s, this prompted attempts to define the boundaries of these spaces with MRI image data.[68]

The basic idea behind (water) diffusion is as follows. Thermal agitation makes molecules move and bounce against each other continuously. This random motion is known as Brownian motion. Over time, Brownian motion produces a net displacement per molecule. This displacement is randomly distributed in large molecular populations ('free diffusion'), but restricted in areas of the body where molecules are confined to bounded spaces, such as cells. When molecules reach the boundaries of these spaces, they reflect back into the medium. In restricted diffusion, researchers found that the diffusion distance does not increase *ad infinitum* (as is the case with free diffusion), but saturates by the time all molecules have reached the boundaries of the medium (see figure 40). The 'diffusion coefficient' characterizes the mobility of molecules inside and in relation to the diffusing medium. The value of this saturation diffusion can be compared to the free diffusion coefficient, in order to characterize the features of the medium at hand (ibid).

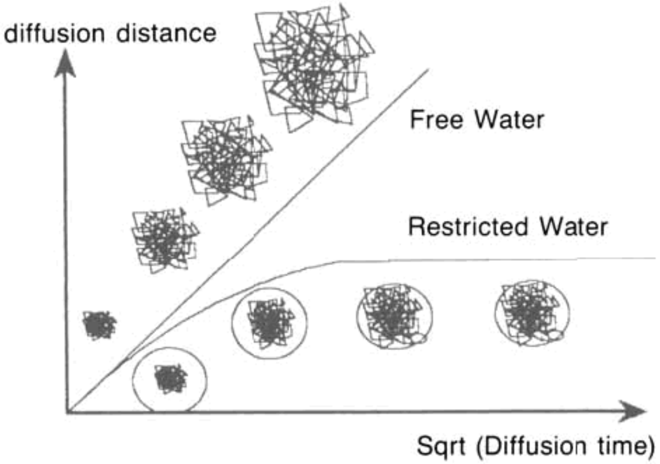


Figure 40. Reproduced from Le Bihan (1991). The figure legend reads: "Free versus restricted diffusion. With free diffusion, such as for water molecules in a bottle, the diffusion distance increases linearly from with the square root **(Sqrt)** of the diffusion time (straight line) according to the Einstein equation. The slope of this straight line defines the diffusion coefficient *D.*"

The first molecular diffusion imaging experiments were carried out under the auspices of the San Francisco Radiology Research and Education Foundation by medical students George Wesbey, Michael Moseley, and Richard Ehman. With success: Not only were their results published in *Investigative Radiology*, a peer-reviewed journal for clinical and laboratory investigations in diagnostic imaging, but the three students also won the annual memorial award of the Association of University Radiologists in 1984. Wesbey, Moseley and Ehman's work subsequently served as catalysts for the French neuroradiologists Denis Le Bihan and Éric Breton, who issued a preliminary note on molecular diffusion in the brain a year later (*Comptes rendues de l'Académie des sciences*, 1985). Le Bihan and Breton had placed subjects inside an MRI machine. The machine consisted of a long tube, surrounded by a large, disk-shaped magnet. The subjects had a radiofrequency coil around their heads for optimal signal collection (see 41).

Figure 41 http://www.onemedplace.com/blog/archives/561; accessed 10-2-2009, 15:00 h.

Next, the subjects were exposed to a magnetic field, varied linearly by so-called pulsed field gradients. These short pulses can be identified by four characteristics: their axis, their strength, their shape, and their duration. When field gradients are varied, water protons begin to precess at different rates (Breton et al., 1988). Le Bihan and Breton subsequently applied gradient pulses in the same direction, but with opposite magnitude. Some protons had moved by that time, creating an imperfect refocusing and a loss of signal. The spatially discrete information at each voxel (*volumetric pixel*, the cubic unit in which measurements are carried out) was subsequently translated into macroscopic visual representations of the brain.

Soon after, Le Bihan and Breton filed for a patent. They claimed that their process was the first to reveal that diffusion is *isotropic* in gray matter tissue, i.e. homogeneous in every direction. According to the authors, this was not the case in white matter, where diffusion was 'anisotropic', or directionally dependent. White matter consists of elongated, tube-like structures called axons, that spread in particular directions. Le Bihan and Breton's results confirmed that the diffusion of water in axons is restricted in the direction perpendicular to their length.

Le Bihan and Breton's findings were considered crucial for a number of reasons. Contrary to earlier neuroanatomical work on cerebral white matter, their experiments were performed *in vivo*, and non-invasively. As we have seen in the previous chapter, axonal projections in humans had thus far only been traced *in vitro*. Although *in vivo* experimental methods were regularly used on animals, these methods are highly invasive. Comparable research in humans was therefore necessarily much more limited (and mostly depended on the instances in which for instance lesions permitted the analysis of white matter impairment). Expectations regarding the French findings were also high, because measurement of molecular diffusion brought about a new method for tissue characterization, such as cell geometry. In addition, because of its potential to discriminate between diseased and healthy tissue (the tissue resonates differently), diffusion-weighted imaging was quickly recognized as an important tool in clinical neuroradiology. Lastly, visualizations of living neuronal projections in the brain were expected to further the understanding of brain *function* as well. Today most researchers agree that behavior is regulated by extensive neurocognitive networks, connected by neuronal pathways. Structural information on the condition of white matter would potentially lead to early clinical evaluation of for instance neurologic disorders (Merboldt et al., 1985; Le Bihan 1991; Taylor & Bushell, 1985).

Le Bihan and Breton's patent was acknowledged on October 25, 1988. In the next few decades, Le Bihan in particular would play a leading role in the new field of diffusion-weighted imaging. The possibility of measuring diffusional properties in the brain for white matter tissue characterization received an important impetus when his research team put forth that the amount of diffusion not only strongly depends on the shape, but also on the orientation of the medium, in this case axons (Le Bihan, Turner, Moonen & Pekar, 1991). Le Bihan et al. found that water molecules preferentially diffuse toward and away from the cell body, parallel to the axons' length. The orientation of the restricted diffusion therefore became an additional factor in tissue characterization, next to its magnitude and shape. Measurement of the extent of anisotropy along multiple axes, at the level of individual voxels, became known as diffusion tensor imaging. In order to define the shape and orientation of the anisotropy, six parameters or tensors were measured. Le Bihan and his colleagues subsequently fitted the results to a symmetric 3D ellipsoid (see figure 42).

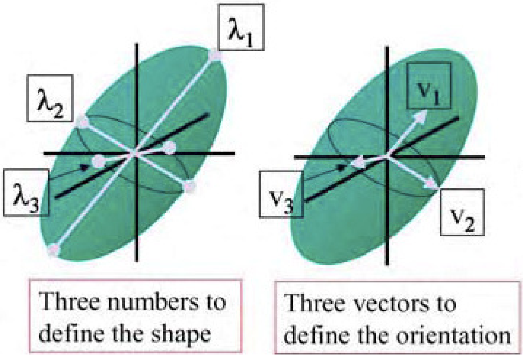


Figure 42 "Six parameters are needed to completely describe the magnitude and orientation of the 3D ellipsoid (...) and determination of these 6 parameters is the target of DTI. Thus, determination of the tensor elements requires measurement of diffusion constants along at least 6 spatial directions." (Mori et al., 2005, p. 2)

Until recently, it was customary to display the information from the six tensor parameters in two-dimensional vector pictures, visualizing the fibre orientation within the voxels of image planes (see figure 43).

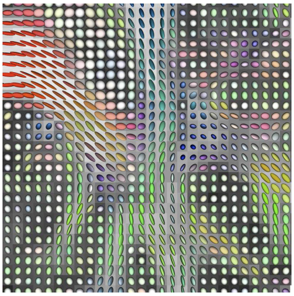


Figure 43 Portion of a DTI brain scan as visualized by ellipsoids (Reproduced from Vilanova, Zhang, Kindlmann & Laidlaw, 2006)

At the end of the 1990s, researchers began to translate the vector information into three-dimensional reconstructions of neuronal projections. Neuroradiologist Susumu Mori was one of the first to develop such a fibre tracking method, called FACT - Fibre Assignment by Continuous Tracking (Mori, Crain, Chacko, Van Zijl, 1999). Mori was also first author of the white matter atlas that I will discuss in the following sections. The tracking method was based on complex mathematical reconstructions in each voxel of the principal fibre orientation (the largest eigenvector, as seen in figure 44b), and calculations of the probability that fibres continue their track in neighboring voxels.

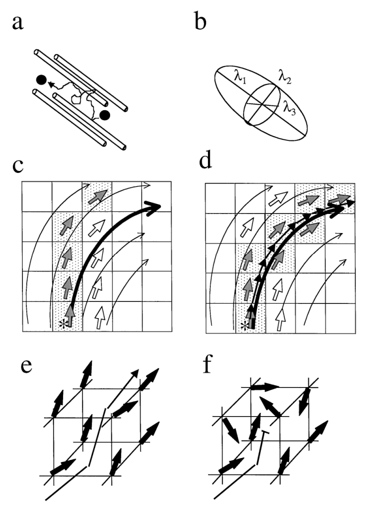


Figure 44 Reproduced with permission from Mori et al., 1999. Their explanation of FACT reads: “The most intuitive way to perform this tracking is by connecting each voxel to the adjacent one toward which the fibre direction is pointing. However, when using this approach, the tracking (indicated by the dotted

voxels) often deviates from the true fibre orientation, because the choice of direction is limited to only eight angle ranges (26 in the case of 3D) (see Fig 1c). This problem is avoided when tracking a continuous rather than a discrete vector field (see Fig 1d). Here, tracking is initiated from the center of a voxel and proceeds according to the vector direction. At the point where the track leaves the voxel and enters the next, its direction is changed to that of the neighbor. Due to the presence of continuous intercepts, this tracking now connects the correct voxels and the actual fibre (bold straight arrows in Fig 1d) can be assigned. We therefore dubbed this approach FACT. The end point of the projection is judged based on the occurrence of sudden transitions in the fibre orientation (see Fig 1e and f)." (p. 266)

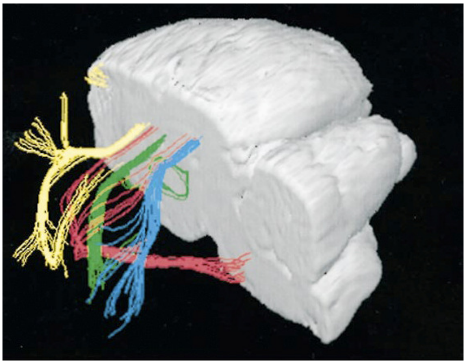


Figure 45 3D tracking of prominent axonal tracts in the rat brain (Mori et al., 1999, p. 268)

Benchmarks of reality

In the following years, the development of diffusion tensor imaging (DTI) brought about an avalanche of studies on the characterization of tissue microstructure.[69] At the beginning of the new millennium, DTI's most highly developed application was that of fibre tracking in the brain (Le Bihan & Van Zijl, 2002).

The dissemination and use of DTI largely took place in the context of clinical radiology. The parameter was developed by and for clinical users, and was appropriated as a new window on (human) white matter anatomy. This was also explicitly the case in the first *MRI Human White Matter Atlas*, published in 2005 by Susumu Mori, Setsu Wakana, Lidia Nagae-Poetscher, and Peter van Zijl. At the time, they were all affiliated with the Johns Hopkins University School of Medicine, Department of Radiology (Division of MRI Research), and the F.M. Kirby Research Center for Functional Brain Imaging (Kennedy Krieger Research Institute), in Baltimore. Principal researcher Susumu Mori has been one of the key players in the relatively new field of DTI since 1999, the year he published his FACT tracking method. He also co-designed the image processing software package that will be the centre of my analysis in the next section.[70]

As we have seen in previous chapters, atlases both reflect and shape a discipline's research objects. The atlases draw boundaries for 'proper' observation and visualization that practitioners can fall back upon (Daston & Galison, 1992; 2007). In this case, I will argue, the pictorial and observational conventions of clinical radiology act as important filters to the digital images, even though quantitation and digitality are highly constitutive of diffusion tensor imaging.

Mori's research group targeted their atlas to a readership of radiologists and surgeons mainly, who deal with connectivity impairment on a regular basis. This immediately becomes apparent on the first pages of their atlas. The authors 'enroll' their envisioned audience, most of them new to diffusion tensor imaging, through short verbal descriptions and comprehensible visual juxtapositions. When practitioners learn how to read the new images properly, Mori et al. argue, they will be "capable of assessing the status of specific white matter tracts in patients with developmental abnormalities, tumors, or neurodegenerative diseases, to name a few." The authors subsequently provide their readers with an explanation of the water diffusion process in the brain. Instead of a detailed account of the complex mathematics behind the images, water diffusion is explained more metaphorically, by comparing the process to the growth of an ink stain on paper, which, again, is a sign of the status of their audience: novices to diffusion-weighted imaging:

"Usually the drop turns into a circle (Gaussian distribution) that grows over time. The faster the diffusion is, the larger the diameter of the circle, and the extent of the diffusion can be estimated from this. Because the extent of the stain is equivalent in all directions, the diffusion is called isotropic. However, if the paper consists of a special fabric which is woven with dense vertical fibres and sparse horizontal fibres, the stain will have an oval shape elongated along the vertical axis. This is called anisotropic diffusion. A similar process happens in the brain where water tends to diffuse preferentially along axonal fibres. If ink were injected inside the brain white matter, the shape of its distribution would be elongated along the axonal tracts. Inside gray matter, the diffusion process is more random because of the lack of aligned fibre structures, and the shape of the ink would be more spherical." (Mori et al., 2005, p. 1)

The *MRI Human White Matter Atlas* presents images from a new MRI imaging parameter that practitioners are not used to. Daston & Galison (2007) convincingly demonstrated that in these cases "everyone in the field addressed by the atlas must begin to learn to 'see' anew." (p. 22) Mori and his colleagues carefully set the stage for DTI, by contrasting it to other, more familiar visualization technologies and parameters. Not only do they claim that DTI can visualize white matter structure in the living brain (as opposed to histology), they also argue that the technique is capable of doing something "conventional MRI" cannot do: provide good contrast on the level of individual voxels, so that variations in white matter structure are properly displayed. "From an MRI point of view, the white matter generally appears as if it were a fluid-like homogeneous structure, which, of course, is not the case." (Mori et al., 2005, p. 1) Applying a phrase from media theorists Bolter and Grusin to my case study: The authors justify the use of diffusion tensor imaging "because it fulfills the unkept promise of older technologies." (cf. Bolter & Grusin, 1999, p. x) To fully advertise the impact of the new parameter, Mori's group presents their readers with a visual comparison (see figure 46). This triptych, displayed on the first pages of the atlas, defines the boundaries for understanding the subsequent two- and three-dimensional atlas images in the second and third chapter. I will therefore analyze the triptych in some detail, before turning to the actual atlas images.

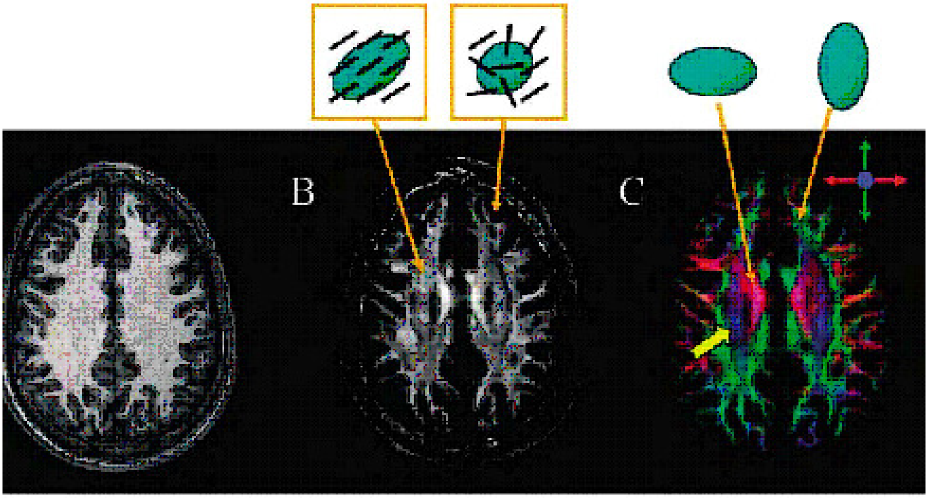


Figure 46 T1-weighted MRI, and two representations of anisotropy levels in the same slice. The color-coded image displays the direction of the measured anisotropy. (Courtesy of Susumu Mori)

From left to right, we see a T1-weighted magnetic resonance image, followed by a representation of variations in anisotropy levels, in the middle, and a color-coded version of the same information on the right, displaying the directionality of the restricted diffusion. The ellipsoids drawn above the middle and right image are connected to a specific area in the brain through yellow arrows. The green shapes are meant to facilitate the visual appreciation of brain areas in which the diffusion is either anisotropic (uni-directional, shades of gray, elongated oval) or isotropic (random diffusion, black, round).

The image on the left is the only one that does not have explanatory features. Significantly, this is what the authors refer to as a “conventional” MRI. It has this label for a reason: In the world of the practitioners Mori et al. had in mind as their audience, this MRI speaks for itself. It is carefully selected as part of the triptych to serve a twofold purpose.

First, and most obviously of all, the triptych demonstrates a deficiency. A large portion of the field of view is occupied by the flat, rather homogeneous mass the authors were referring to in the accompanying text. This “fluid-like structure” represents a segment of the brain’s white matter. The authors deliberately chose an axial (horizontal) brain slice at a level where white matter dominates gray matter in terms of percentage. At no other slice level is this so obviously the case. The image is meant to conjure up a strong contrast with the diffusion-weighted anisotropy image in the middle. This “unique new MRI modality” provides a very different view on white matter structure. Instead of a flat field, the viewer is treated to a much more detailed, rather mountainous landscape. Obviously, one of the purposes of placing the two images side-by-side is to demonstrate that diffusion tensor imaging presents much more information on white matter structure. On the other hand, the juxtaposition has more complex effects. The MRI on the left is not only selected to boost the new parameter; it also serves another purpose.

Interestingly, all three of the images displayed above represent a constructed or simulated “digital space” (Lynch 1991, p. 63). They are all visualizations of a rendering algorithm. Wright pointed out that these algorithms have “the power to externalize in quite arbitrary forms.” (1990, p. 69) It is therefore interesting to pay a little more attention to the exact visual form these images have been given.

Despite major differences in data processing, the first and the second image are visually not that dissimilar, compared to the third image. What we see here, is a break between two different imaging traditions. Both black-and-white images evoke the immediacy and mechanical objectivity of a photographic realism, while the color-coded image on the right explicitly reveals its digital mediatedness. The left and middle image explicitly call to mind a long-standing radiological visual culture, in which for instance x-ray photography and CT scanning technology have long since created familiar cultural objects. The latter visualization technologies have a lot in common with ‘regular’, analog photography. For one, x-ray images and CT scans look a lot like photographs. As Anne Beaulieu pointed out, the use of gray for structure in the MRI images is probably part of an x-ray and CT aesthetic, which may have derived from the use of certain types of chemicals to coat plates (Beaulieu, 2000, p. 110).

In addition, x-ray and CT also derive their status from a mechanically induced point-to-point correspondence between the image and the object. Thirdly, contrary to MRI images, both x-ray photography and CT scanning are dependent on the wavelength of light or other electromagnetic waves (Prasad, 2005).

MRI images are computer-generated, and therefore intrinsically different from images that do have a “reference to the position of the observer in a ‘real’, optically perceived world,” (Crary, 1990, p. 1, 2). However, it is this particular ‘opticist’ pictorial tradition that the “conventional” MRI is rooted in, and its aura of an unmediated access to reality is transferred to the diffusion-weighted image in the middle. In the words of Michael Lynch (who in turn refers to Roland Barthes and Walter Benjamin), the left and middle image exploit “the ‘mystique’ associated with photography and related forms of ‘mechanical reproduction’.” (Lynch, 1991, p. 72)

In the image on the right, on the contrary, no attempt is made to create an illusion of realism through for instance a photographic appearance, or through ‘natural’ coloration. In fact, the use of colors is quite extravagant. The purpose of the colors is not to invoke an ‘optical’ truth. The colors are meant to be read as codifications of the direction of the largest eigenvector in each particular voxel: red for right to left, green for anterior to posterior (front-back), and blue for superior to inferior (top-bottom). The color-coded image is ‘hypermediated’ (term coined by Bolter & Grusin, 2000). It makes the viewer aware of the medium and the acts of representation that created it. I will get back to this point in the next section.

Sociologist of science Amit Prasad recently finished a study on the use of MRI in radiological analysis in the US and India (Prasad, 2007). In the laboratories he analyzed, the results of particular scanning sessions were usually compared to the images in existing MRI body atlases, in order to “zero in on the pathology.” Inspired by Bruno Latour, Prasad refers to this process as “cross-referencing.” (p. 296) These MRI body atlases “through experience and instruction, become a part of the radiologists’ memory.” (p. 297). In the body atlases Prasad refers to, all MRI slices are paired with diagrammatic representations of the same slice (see figure 47).

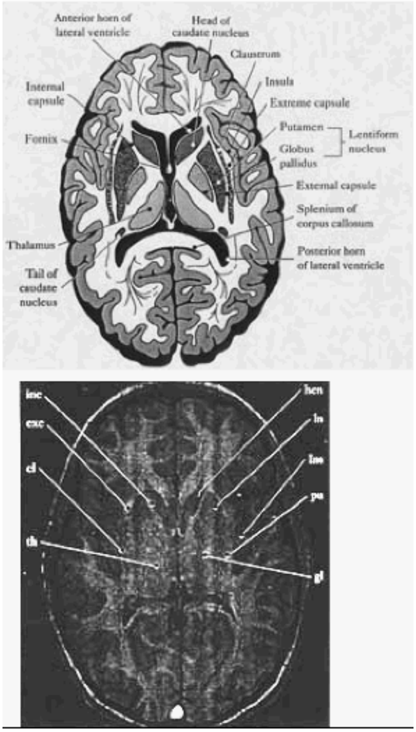


Figure 47 “The image on the top is a diagrammatic representation of an axial view of basal ganglia. Notice how each part of basal ganglia is clearly marked out and identifiable in the picture. The image at the bottom is the axial Mr image of the basal ganglia. (…). SOURCE: Kelly and Petersen (1997, 53). Reprinted from L. Kelly and C. Petersen, Sectional Anatomy for Imaging Professionals (53), copyright 1997, with permission from Elsevier.” Reproduced from Prasad (2005, p. 306)

Prasad argues that trust in MRI images gets established by comparing them with “already archived knowledge about the human body,” as represented in the diagrammatic representations of anatomical structure. (p. 305) As we have seen in chapter 2, this practice of paired presentation goes back at least as far as Jules-Bernard Luys’ *Iconographie photographique des centres nerveux* (1873). This practice also finds its way to the white matter atlas published by Susumu Mori’s research group. But the “already archived knowledge” of anatomical structure takes a different shape.

With the publication of their atlas, Mori and his colleagues aimed to further diffusion tensor imaging as a promising new imaging parameter. The atlas was a strategic document, comparable to Luys’ 1873 photographic atlas of the nervous system. But in contrast to Luys’ atlas, their findings were not at issue here, the technology itself was. In order to succeed in fortifying diffusion-weighted imaging, Mori and his colleagues chose to demonstrate that the method was capable of visually representing knowledge of white matter anatomy everyone in the field agreed on. After the introduction and a seven page methods section, the central part of the atlas consists of two chapters with three-dimensional and two-dimensional visualizations of 17 major white matter tracts (see figures 49-51). The existence of these white matter structures was first established in the 19th century. One of the classic sources researchers today often refer to, is the *Anatomie des Centres Nerveux*, published by Jules and Augusta Dejerine in 1895. Mori et al. use as a reference a more recent source: the digital photographs of postmortem preparations presented online at the University of Iowa's Virtual Hospital (www.vh.org) (see figure 48).

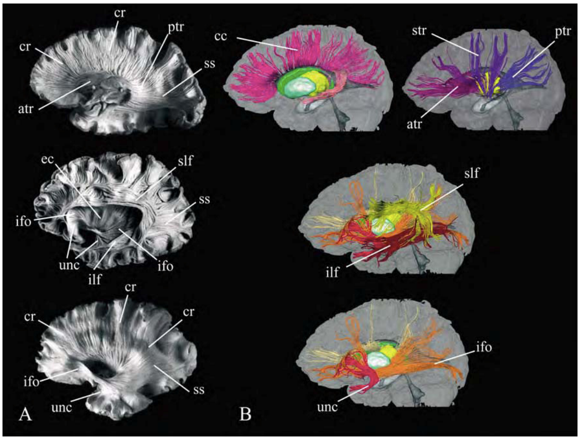


Figure 48. "Comparison between postmortem preparation (A) and DTI-based reconstruction results (B). Abbreviations are atr: anterior thalamic radiation; cc: corpus callosum; cr: corona radiata; ec: external capsule; ifo: inferior fronto-occipital tract; ilf: inferior longitudinal fasciculus; ptr: posterior thalamic radiation; ss: sagittal stratum; slf: superior longitudinal fasciculus; str: superior thalamic radiation; unc: uncinate fasciculus. Copyright protected material (postmortem tissue images) used with permission of the authors and the University of Iowa's Virtual Hospital, www.vh.org." Reproduced with permission from (Mori et al., 2005, p. 5)

The colorful white matter tracts in the image above are the results of fibre reconstruction via statistical algorithms. The calculations were performed on the 'raw' data acquired by the MRI scanner. Most likely, this "digital objectivity" will be what the authors fall back upon when they are called on to validate their claims - most neuroscientists downplay the visual side of their work, and favor its statistical underpinnings (Beaulieu, 2000, 2001). But for present purposes, the visual appeal of the paired presentation is certainly relevant. The triptych discussed above already revealed that photorealistic “conventional” MRIs acted as a frame of reference for the new diffusion tensor images. This framing is repeated in figure 48: actual photographs of postmortem preparations are placed side-by-side with DTI-based white matter tract reconstructions. Again, it is the aura of a photographic immediacy that acts as legitimation for the DTIs. The fact that it has proven possible to visualize these known white matter tracts in 3-D, and have them look identical to digital photographs of preparations, serves as proof of the accuracy of the new imaging parameter.

In the rest of the atlas, the three-dimensional reconstructions of white matter tracts are displayed on T-1 and T-2 weighted reference images. Here, the “conventional” magnetic resonance images have *themselves* become the benchmarks of anatomical ‘reality’. This is also the case with two-dimensional visualizations of white matter structure. In the final chapter of the atlas, these 2D images are presented consecutively, in the following sequence (see figures 50 and 51). On the top of each left page, we see a coronal and a sagittal MRI slice, marked by a horizontal red line. Through the red lines, the workings of the MRI scanner are visibly inscribed on the images. Below, an axial slice is displayed, at the level of the horizontal lines in the two images above. This image bears the marks of the three-dimensional tracts at that particular slice level. They are indicated by the same color as in the three-dimensional images. On every right page of the atlas (in the 2D chapter), we see a color-coded image of the same axial slice level. The choice of colors in these images is not based on the 3-D tractography in the previous chapter, but on the direction of the largest eigenvector in each individual voxel (as explained in the previous section). The colors of the 3-D tracts in chapter 2 (figure 49), do however match the colors of the anatomical labels in the 2-D color-coded images (figure 51).

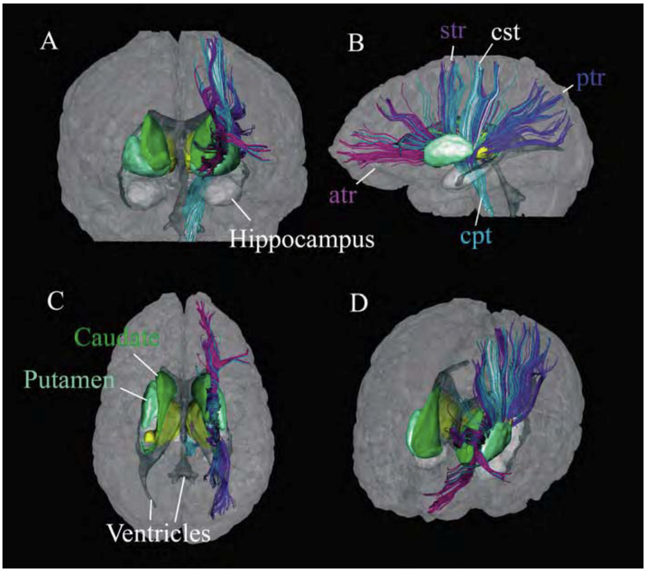


Figure 49 (Mori et al., 2005, p. 20)

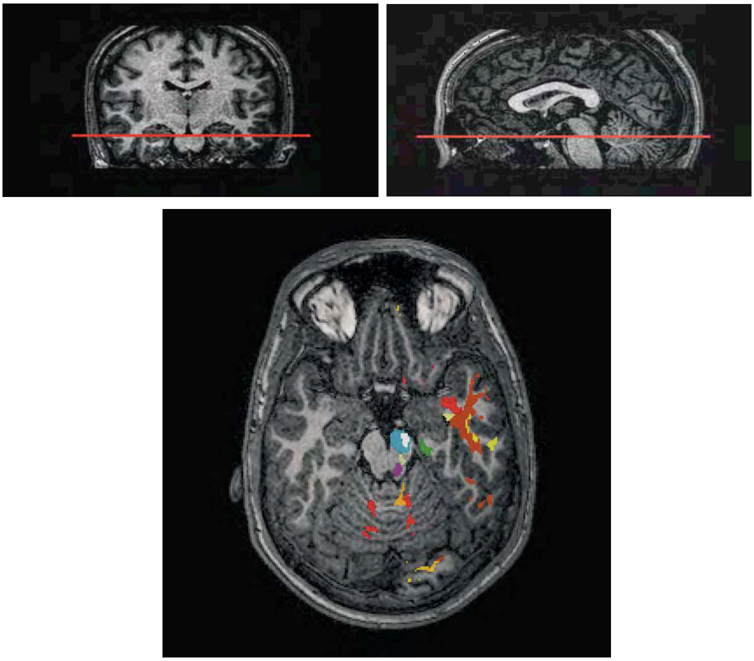


Figure 50 (Mori et al., 2005, p. 56)

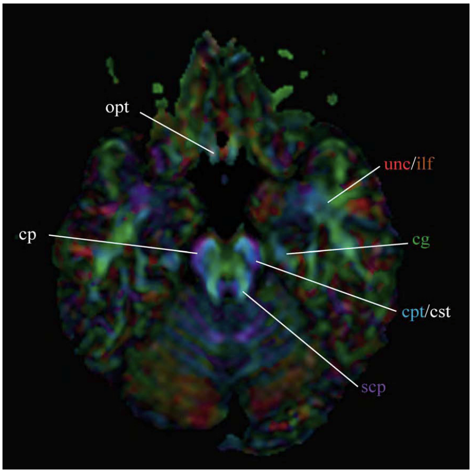


Figure 51 “cg: cingulum cp: cerebral peduncle cpt: corticopontine tract cst: corticospinal tract, ilf: inferior longitudinal fasciculus opt: optic tract scp: superior cerebellar peduncle unc: uncinate fasciculus” (Mori et al., 2005, p. 57)

Mori and his colleagues acknowledge that the color-coded image may be challenging for practitioners, “because the contrast and anatomic information it provides is complex and requires detailed knowledge of the 3D architecture of white matter.”

“[F]or many radiologists, the color map is still a novel imaging modality and training is required to read them. To fully evaluate the color maps, it is essential to understand white matter anatomy three-dimensionally as well as how each tract is revealed in a given 2D observation plane. In this atlas, color maps are presented at multiple slice levels and orientations and the white matter structures are identified, assigned, and annotated by comparison with their reconstructed 3D trajectories.” (Wakana et al., 2004, 77)

The complexity of the color-coded DTI images might partly explain why the authors have chosen the (apparent) immediacy of the “conventional” MRI to put forward the new, hypermediated images. The transformation of MRI to DTI convinced the viewer to trust DTI as a new imaging parameter, while at the same time ‘easing’ the viewer into the color-coded images, which - according to the authors - contain the “greatest amount of information regarding white matter anatomy.” (Mori et al., 2005, p. 4) DTI is presented as an improvement of older MRI parameters in the visualization of white matter, but in the end, the new parameter never really challenges status of the "conventional" MRI.

In the Studio[71]

One of the purposes of the publication of the *MRI Human White Matter Atlas* was to persuade radiologists to integrate diffusion tensor imaging in their own radiological laboratories. But the concrete incorporation of DTI also required technological changes in the trajectory from image data acquisition by a scanner, to the visualization of white matter tracts on a computer screen. In the current section, I will analyze the user manual of a software package designed specifically for this purpose by Susumu Mori and another colleague at Johns Hopkins University's radiology department: Hangyi Jiang. DtiStudio is a diffusion tensor image processing program for tensor calculation, color mapping, fibre tracking, and 3D visualization. All the images in the *MRI Human White Matter Atlas* were produced using this particular software. In addition, the software also prescribes how the 'raw' diffusion-weighted image data should be experienced, and organized for further scrutiny. Therefore, it not only provides a unique look 'behind the scenes' of the production of the atlas images, but also creates an opportunity to further study the precise characteristics of DTI's new way of seeing and knowing.

Diffusion tensor imaging is not an easy task: It requires "involved post-processing" behind a desk for image production, after the 'raw' data has been gathered by the scanner (Jiang, Van Zijl, Kim, Pearlson, Mori, 2006, p. 106). This is one of the ways in which DTI differs fundamentally from 'conventional' MRI, where users have less influence on image production. The manual takes the reader through all the steps implicated in the "involved post-processing" procedure - but not before emphasizing that DtiStudio is the requisite image processing tool for practitioners and researchers interested in brain connectivity (disorders):

"As the importance of DTI technology in clinical practice is expected to grow in the near future, the need for resource programs that researchers and clinicians can use to rapidly process DTI data, obtain fibre tracts, and view images in three-dimensional [sic] with user-friendly interfaces, becomes paramount." (ibid)

A 'user-friendly interface' in this case means a Microsoft Windows interface. Not only does the program mimic the interface of other Microsoft applications such as Outlook, Word, or PowerPoint, it is also said to combine smoothly with these programs.

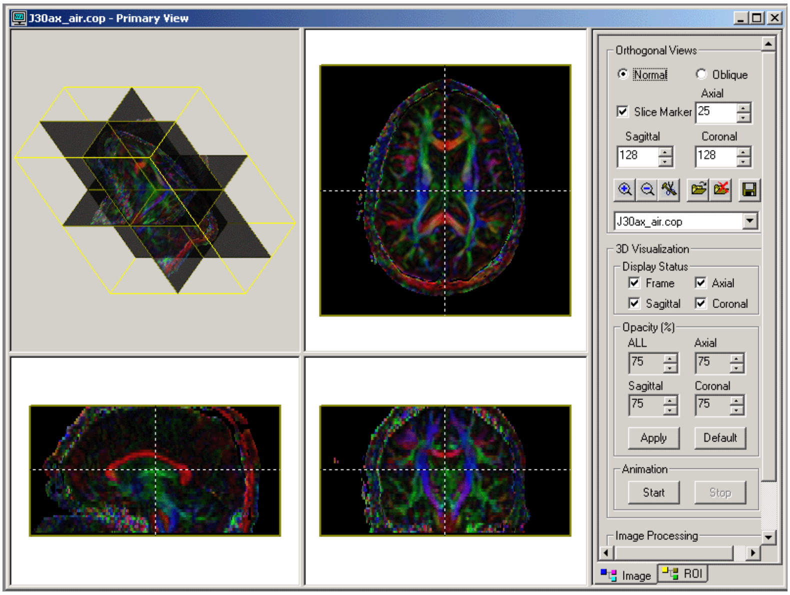
As was the case with Mori et al.'s atlas, the DtiStudio user manual speaks to its intended audience in a highly straightforward manner. The whole manual breathes fun and effortlessness. The authors stress that the software takes up only a few hundred kilobytes to install. After installment, users are immediately "ready to play with the program." Provided that there is enough memory for the (much larger) data-sets, the software can be used on virtually any computer, at home, or in the laboratory. In addition, "most operations can be done with only a few clicks." (Jiang & Mori, 2005, both quotes on p. 1) Ideally, users will cut and paste images from DtiStudio to the Word file of an article in progress, upload images to their web pages, or e-mail interesting findings to colleagues after data processing. These and other elements of the software (discussed below) imply a very different – interactive, dynamic - process of image production and dissemination than we have seen in previous chapters.

DtiStudio is divided into three main sections: a '3D image viewer', a 'DTI mapping section', and a 'fibre-tracking module'. The 3D image viewer loads the imaging data, acquired by the scanner, and facilitates the observation of this data as it is translated into multiple visualizations of white matter structure. Most diffusion-weighted imaging experiments require multiple image acquisitions with different gradient directions. Therefore, the 'raw' data consists of a series of brain volumes (Clayden 2008). The DTI mapping section is where new visualizations such as color-coded images can be created from the data gathered by the scanner. Lastly, the 'fibre tracking module' serves to create three-dimensional visualizations of white matter tracts.

Both Mori's white matter atlas and the DtiStudio software are firmly rooted in clinical neuroradiology. The data processing software aims at unproblematic integration into clinical practice. As such, it differs from other analysis software with a distinctive research orientation (Beaulieu, 2000, p. 111).

One of the software's striking features is that the role of visual and manual processing is not marginalized, nor fully incorporated into the technical equipment and/or software.

After loading the information from the scanner into the 3D image viewer, an image parameter window appears. In this window the user specifies, among other things, the number of slices per volume, the slice orientation, the slice sequencing (inferior-superior or superior-inferior, for instance), the matrix-size in pixels and the slice thickness (matrix-size and slice thickness determine the voxel size in mm). After all parameters have successfully been processed, DtiStudio reconstructs all 3D objects in the image file, one after the other (see figure 52).



Orthogonal Views: ● Normal ○ Oblique

☑ Slice Marker

Axial: 25

Sagittal: 128

Coronal: 128

J30ax_air.cop

Display Status: ☑ Frame ☑ Axial ☑ Sagittal ☑ Coronal

Opacity (%): ALL 75, Axial 75, Sagittal 75, Coronal 75

Figure 52 "Image view window for three orthogonal views, a 3D view, and a control-panel." Reproduced with permission from Jiang & Mori, 2005.

One of the unmistakable differences with the visualizations I have analyzed in previous chapters, is that diffusion tensor images are much more malleable. The image panel, for instance, has several interactive features. Users can move between slice locations, either by typing in the desired slice number in the editor box on the right, or by double-clicking on the location in one of the images. In addition, they can adjust the opacity of the generated images, or chose for 3D visualization and/or animation. The dotted lines on the orthogonal views are so-called slice markers. They indicate the positions of the image slices in other orthogonal views. The space on the top-left of the window represents a 3D view on the brain. This tri-plane image is also interactive: users can rotate the image by moving the mouse over the 3D area.

It is typical for the way the software is developed that new buttons appear only after finishing with the previous action. After loading the raw image data, the "image processing" button appears on the primary image window's control panel. The image processing button serves to create color-coded orientation images from already existing statistical information on the magnitude of the anisotropy, through image intensity, and the direction of the principal eigenvector (mathematical reconstructions in each voxel of the principal fibre orientation), through the use of different colors. After creating the color-coded images, users can save both 2D image slices or complete 3D image volumes.

Subsequently, a "DTIMap tab" appears at the lower-right corner of the window. At this moment, Jiang and Mori suggest that the user inspects the images, because image quality problems are very common. Errors can be due to for instance shifts of brain position ('co-registration errors'), or even complete disappearance of images. Interestingly, these problems are not result mechanically or through quantitation alone, but must be identified by visual inspection. The process of image quality problem solving points to the co-production of diffusion-weighted images by both pre-programmed digital courses of action and (practiced/expert) human decision making processes. A new window appears, specifically designed to aid in this visual inspection process (see figure 53). Each row represents a different repetition of the scan, while each column corresponds to a different gradient. Users are encouraged by Jiang and Mori to inspect the fourth row for clues as to which images should be excluded from further processing. This row displays the standard deviation images. Too much deviation is represented by increased intensity in the images, the authors explain. If users see corrupted images, they can exclude them by clicking on them. The images are subsequently marked with a red cross.

After the (optional) removal of low-quality images, users can start the tensor calculation. The output of these processes are, among other things, the six components of the diffusion tensor, the anisotropy images, and the color-coded images.

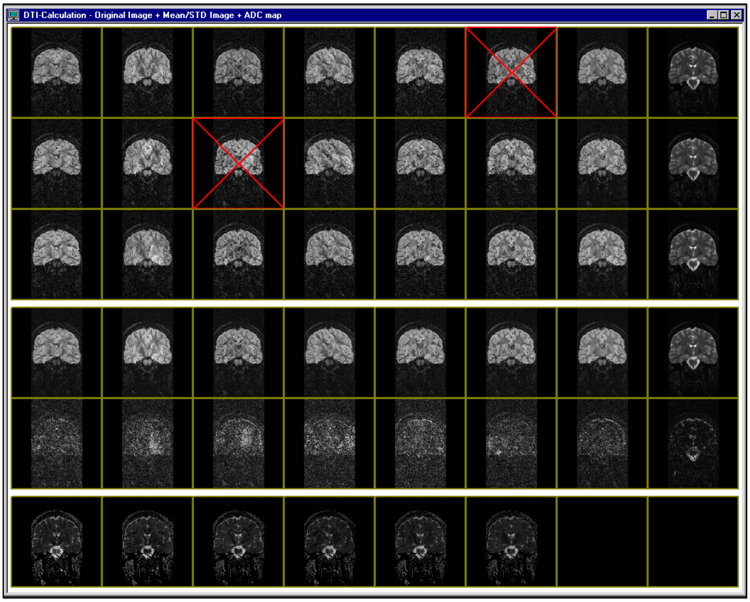
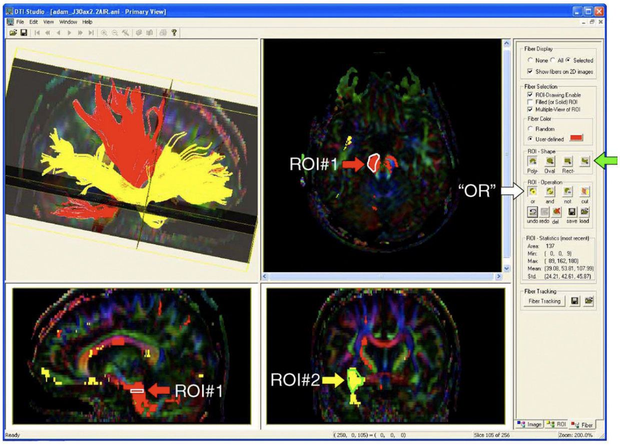


Figure 53 “DW-images view” - Reproduced with permission from Jiang & Mori, 2005.

At this particular moment in the diffusion tensor analysis, a ‘fibre tracking button’ becomes active. This is the last section of the software package, in which 3-D reconstructions of fibre tracts are created. Jiang and Mori chose for what they label ‘brute-force’ fibre tracking, based on Mori’s FACT method. Most other programs initiate fibre tracking from user-defined regions of interest, the authors argue, whilst in brute-force fibre tracking, fibres are reconstructed from all pixels above a certain threshold.

Evidently, the huge amount of data, first acquired by the MRI scanner, and processed by the software, needs further selection to be made meaningful. This is an interesting process, for it again reveals the interplay between the digital objectivity of the computer and the expert judgment of the user. The latter's knowledge of basic anatomy for instance shapes the compulsory choices made as to which particular fibres are selected for visualization. Compulsory, because a visualization of all fibres would be incomprehensible. The choices can be processed in the 'fibre orientation control panel'. The panel appears after fibre tracking is finished. With this panel, users can choose the color of the reconstructed fibres, or have it generated randomly by the computer. They can also define which regions of interest are to be selected for visualization of tracts passing that region. The regions of interest (ROI) are actually drawn on the images with a series of mouse clicks (notice the combination of manual and technical labor). Users can choose three different shapes for defining the regions of interest. In addition, when users draw more than one ROI, they have four options for how these regions should or should not interconnect. If the option "and" it is selected, all fibres that penetrate the ROI's are selected. If the option "or" is selected, the regions are treated as independent. The third option, "not", is used to remove fibres that penetrate a certain region of interest (see figure 54).


DTI-Studio - [adam_J30ax2.2AIR.ani - Primary View]

Fiber Display: ○ None ○ All ● Selected
☑ Show fibers on 2D images

Fiber Selection:
☑ ROI-Drawing Enable
☐ Filled (or Solid) ROI
☑ Multiple-View of ROI

Fiber Color: ○ Random ● User-defined

ROI - Statistics (most recent)
Area: 137
Min: (0, 0, 9)
Max: (89, 162, 180)
Mean: (39.08, 53.81, 107.99)
Std. (24.21, 42.61, 45.87)

Ready
(250, 0, 105) = (0, 0, 0)
Slice 105 of 256
Zoom: 200.0%

Figure 54 Three types of ROI drawing tools (Jiang, Van Zijl, Kim, Pearlson, Mori, 2006, p. 114)

In effect, the images that the user generates are amalgamations of standardized calculations and physical interventions, i.e. mouse movements and the like. Although the process is partly standardized, the user is also actively engaged in a dynamic relationship with the image displayed on a computer screen. As we have seen, the images themselves are not static either: users can for instance generate and isolate various white matter tracts, have them appear and disappear on the screen, change the color of the tracts, or play with the three-dimensional renditions of white matter structure. The researchers or clinicians behind the screen can choose certain modifications on the basis of their experience with brain anatomy, or bring to life visualizations of white matter that have never been created before. Conversely, the software can also enrich the experience of the viewers by presenting them with a sheer indeterminate range of imaging modalities and options for further processing, depending on the purpose of the study.

Compared to mechanically produced ‘optical’ images such as photographs, which are also heavily dependent on decisions as to lighting, framing, type of film, etcetera, the production of diffusion tensor images leaves the image makers with much more time and opportunity to intervene. As such, this particular practice of observation and visualization fits neatly into a trend described by Lorraine Daston and Peter Galison in the last chapter of their recent book *Objectivity* (Zone Books, 2007).

According to Daston and Galison, contemporary atlas (image) makers in general have become more involved in images that function as malleable tools, instead of images that aim at faithful representation. They describe these developments from the perspective of the emerging field of nanotechnology, an “engineering-inspired, device-oriented” field in which images “function less for re-presentation than for *presentation*.” (p. 389) The authors dropped the re- in ‘representation’, because they found the emphasis on repetition or re-making inappropriate for these particular images. Nanotechnology serves as an exemplar of a host of other fields in which image makers are no longer merely interested in re-presenting existing objects (or body parts for that matter), following the adagium of for instance spatial fidelity and faithful coloration. Instead, increasingly more malleable, hypermediated, interactive, virtual images come into being (and can also disappear with one mouse click). In my analysis of the software and of the relation between the atlas and the program, I have shown how these interactive practices become entwined in the relatively new field of diffusion tensor imaging. In this field too, users are invited to engage in a process of continuous intervention, add or extract colors, define regions of interest in the image, or subject parts of the image to several algorithmic filters. Here, too, "nature emerges with artifact," presenting images that can be made, remade, altered, and saved (Daston & Galison, 2007, p. 388, 397). The shift from immediacy to hypermediacy clearly also has an effect on the images themselves. We already saw the effect at work in the previous section, where I discussed the images in the *MRI Human White Matter Atlas* (Mori et al., 2005). In these images, many of the traces of the mediation were brought to the surface. In hindsight, they may even have laid bare more than thus far assumed. For if we look back at figure 50, we can see that the three images look very much like the 'windowed' presentation of the images in DtiStudio. In addition, the process of understanding the white matter structures on the consecutive atlas images requires constantly shifting backwards and forwards between the visual (the images) and the verbal (anatomical labels, explanatory text). This viewing experience is not unlike the interactive process of observation in DtiStudio.

Conclusion

In the current and previous chapters of this thesis, I have described various practices of objectivity, in relation to the production and use of neuroscientific visual representations of the brain. In chapters 2 and 3 for instance, Jules-Bernard Luys and Santiago Ramón y Cajal's neuroanatomical work was discussed. Both men aimed at objectification through non-intervention, Luys by way of the photographic camera, and Cajal by turning his hand into a precision instrument. In the first MRI human white matter atlas, the conventions for proper observation and visualization seem to move back and forth between the (utopian) immediacy of a mechanical objectivity, and an emphasis on hypermediated re-presentation. On the one hand, the apparent transparency of the 'conventional' MRI, and the already archived anatomical knowledge gained through classical histology, served to frame diffusion-weighted imaging in existing and accepted visualization techniques and knowledge structures. On the other hand, DTI's 'actual tract reconstructions' were meant to overcome the drawbacks of classical histological techniques, and what representatives of the new field label as 'conventional' MRI, in order to create more realistic visualizations of white matter. In conclusion, the white matter atlas images appeared to oscillate between transparency and hypermediacy.

Another way of characterizing this flux was described by Michael Lynch in 1991, and taken up by Anne Beaulieu in her work on brain mapping and cognitive neuroscience (Beaulieu, 2000, 2001, 2002a, 2002b, 2003; Lynch 1991). In an article on laboratory practices and topical contextures, Lynch defines topical spaces as spatially ordered sites of knowledge production, which comprise both symbolic and material space. He discusses two such spaces: 'opticism' and 'digitalism'. In epistemic contexts where opticism is predominant, Lynch argues, the transparent, lensed instrument and the probing eye are the paradigms of this particular epistemic condition. Digitalism, in contrast, centres around the digit, rather than "the ocular-centric image of the realm of light." (p. 62) Among other things, it can be characterized by a focus on mathematization, arbitrary code, and manipulable details. Beaulieu discusses Lynch's article in some detail (2000, p. 15-17). Her work is revealing of how novel dynamics of digital technologies link functional neuroimaging to changing epistemological frameworks (Beaulieu 2000, 2001, 2002a, 2002b, 2003). Both Lynch and Beaulieu repeat that opticism and digitality do not necessarily exclude one another, but can co-exist.

In the current chapter, I have argued that we see this coexistence at work in the first MRI human white matter atlas. The structural atlas images have evolved "in a digital context, sometimes in opposition, sometimes in interaction with the optical mode, and the possibility of particular types of construction it enables." (Beaulieu, 2000, p. 17) Although quantitation and digitality are highly constitutive of diffusion tensor imaging, the pictorial and observational conventions of clinical radiology continue to play a fundamental role in this field. These conventions act as an important filter to the digital and reconstructed images. They are part and parcel of its representational routines, experimental methods, technologies, and knowledge claims.

One could argue that the flexibility of the colorful DTI images makes it very hard to determine which visualization of white matter tracts is intrinsically closer to 'reality' than any other. Perhaps this is what Santiago Ramón y Cajal would say, were he alive today. But with the advancement of diffusion tensor imaging, established stances on what is considered as an 'objective' representation, or 'representation' for that matter, may well have started to change. Adherents to the ideal of a mechanical objectivity strived for the unmediated registration of nature by nature. If not empirically, ideally, researchers were detached, self-negating registrators, who left the brain untouched. In diffusion tensor imaging, the manual dexterity, expert visual judgment, and anatomical proficiency of researchers and practitioners are equally important as, or even mutually dependent on the computer's digital data processing features. Knowledge of white matter structure, function and malfunction is generated in full recognition of the involvement of the researchers and clinicians. The resulting images are made highly flexible for analytical purposes. In this process, 'real' or 'objective' images are probably the images which succeed best in making something available for further processing. They are better thought of as flexible tools than as static representations of brains.

On Making Visible

In the previous chapters, four practices of objectivity were analyzed, spanning over five centuries. I have specifically focused on the role of images in relation to different ways of knowing the brain. Two of the most important outcomes can be summed up as follows. First of all, to recognize that the objectivity of scientific images is a result of mediations, to see objectivity as an *achievement*, is not to discredit the images. Quite the opposite: it is what makes them stand out vis-a-vis other ways of knowing. In the words of Bruno Latour: "[W]ithout huge and costly instruments, large groups of scientists, vast amounts of money, long training, nothing would be visible in those images. It is *because* of so many mediations that they are able to be so objectively true." (2002, p. 19) Secondly, the objectivity of the images indicated different things at different times. Lorraine Daston first demonstrated the layeredness and historical variability of the notion of objectivity (Daston, 1991). As we have seen, we also came across this layeredness and variability in the brain imaging practices at hand. All images of the brain I have analyzed, were directly involved in the articulation and objectification of neuroscientific facts. But how the images were constituted to be meaningful, how they were objectified, and how the brain or parts of the brain were made real, varied in each case.

Before reviewing some of the other outcomes of my analyses, I would first like to give some more consideration to the use of the word 'image' in the preceding chapters.

In 1998, the edited volume *Inscribing Science. Scientific Texts and the Materiality of Communication* was published by historian of science Timothy Lenoir. One part of the volume was dedicated to visual representation and aesthetics, and contained a chapter by Alex Pang on the introduction and development of astrophotography at the Lick Observatory between the 1880s and the 1920s. Building on Edgerton and Lynch's 1988 work on astronomical images, Pang convincingly argued that the aesthetics of astrophotography was part and parcel of its representational realism. His chapter aided my analysis of the introduction of photography in macroscopic neuroanatomy. Among other things, I used Pang's work to compare contemporaneous notions of objectivity with the material practices of proofing and retouching photographs. His work also helped me think through the continued relevance of aesthetics and graphic skill in neuroanatomy, and the enduring collaborations between scientists, printing agencies, engravers, and lithographers. But at the heart of Pang's analysis lies a distinction I decided not to adhere to: a distinction between an 'image' and a 'picture'.

According to Pang, an *image* is a visual message, i.e. information, while a *picture* is an image that has taken a certain shape in specific print media (Pang 1998, p. 228). In contrast, I have treated print medium and visual message as co-constitutive. Combined with artistic conventions on 2- and 3-D projection and on perspective, technologies like the engraver's burin, the microscope, the camera, and the scanning device have helped shape observations and visualizations of the brain, or parts of the brain. Indeed, it makes a great difference if the brain is carved out in wood, captured on film, lithographed, or scanned. The photographs George Luys made for his father Jules-Bernard Luys, for instance, contained more information than required. The level of detail was too overwhelming. Furthermore, the lithographs his father made to accompany the photographs, made things visible that were invisible or hardly visible in the photographs. The same can be said of the wood engravings Jan Stephan van Calcar carved out for Andreas Vesalius, or the copper engravings done for Thomas Willis by Christopher Wren. The images I studied played active roles in various ways of knowing the brain. In each of the cases, the material conditions of the print medium were inextricably tied to other material, technological, cultural, and social factors. These factors came in different guises in each case – as did the accompanying epistemological and ontological issues that they brought forth.

As a first case study, the copper engravings in Thomas Willis' *Cerebri Anatome* were discussed. Along with three other prominent members of the Royal

Society, the Oxford physician Thomas Willis had since the mid-1650's been venturing "to unlock the secret places of Man's Mind," which he believed to reside in the cortex. In 1664 their cooperation led to the publication of the *Cerebri Anatome*. The book was translated into English in 1681 as *The Anatomy of the Brain and Nerves*. It was one of the first books in which the brain's structural components were treated as relevant material entities.

Thomas Willis was a member of the recently founded *Royal Society of London for Improving Natural Knowledge*. The Royal Society members were developing a distinctive empirical focus towards the investigation of nature. The observational and experimental strand was promoted amongst others by Francis Bacon, followed by William Harvey in the 1630s and '40s, and taken up by many of the future members of the Royal Society in the '50s and '60s. This new natural philosophy, based on firsthand observation and experiment, had a large influence on subsequent generations of English scholars.

'Naturalistic' visualization played a pivotal role in the Royal Society's natural philosophy. Thomas Willis presented Christopher Wren's engravings as realistic accounts of the inside of the human skull. Interestingly, what Willis and his colleagues understood as realistic took a very particular, local shape. With Svetlana Alpers (1985), I argued that the engravings were carefully constructed in a typically northern, seventeenth century style – a clear break with what happened elsewhere in Europe, but also with what until then was understood as naturalistic representation. These differences are best illustrated visually. See page 39 for the comparison between one of Wren's engravings and one of the woodcuts in Andreas Vesalius' *Fabrica*, published a century earlier.

Vesalius' wood engravings are perfect representatives of "the geometric matrices of spatialization and individualization [that] emerged from the instrumental, epistemological, and esthetic innovations of perspectivism, which became prominent in (…) the Renaissance." (Haraway 1998, p. x) Conceiving the world from the standpoint of the human, perspectivism places the individual at the centre of epistemological and ontological truth-making (Harvey 1989). Vesalius selected a single, privileged view on his brain in succeeding stages of dissection. This was a typically humanist philosophical stance towards nature. His main aim in the *Fabrica* was to expose the magnificence of God's creation. In Thomas Willis' days, religion was still an important part of scholarly practice. His focus on the cortex entailed a literal materialization of the brain as "Chapel of Deity." But due to, for instance, the growing use of lenses in the practices of many natural philosophers, they increasingly displaced Vesalius' standpoint in favor of a view that relativized fixed proportions or human measures.

In contrast to Vesalius' woodcuts, the copper engravings Willis published in 1664 have no explicit reference to a human scale (for instance by including parts of the dissection table, the anatomist's hand, or the dead person's skull in the image). In the engravings, the focus is not on the suggestion of a concrete, particular, individualized perception. In this case, the images do not represent relations between objects, as is the case with 'classical' perspectival (one-point perspective) images – in which size and scale are captured through juxtaposition. Instead, we see highly symmetrical, almost architectural, visualizations of the brain. That Christopher Wren made the engravings for the *Cerebri Anatome*, seems to make perfect sense. Wren, a natural philosopher with a keen eye for structural design, would not much later be asked to devise another chapel of Deity: St. Paul's cathedral.

A materialistic focus on the cortex, a specific physical site of action, broke new ground for typically modernist, 'naturalistic' images of the brain, such as the engravings in the *Cerebri Anatome*.[72] Willis sacrificed the humanistic perspectival view, empowered to summarize knowledge as beautifully as possible, and called in an eye that claimed to observe nature instrumentally, empirically. By embracing certain key elements of the New Baconian Philosophy, Willis ascribed to an unadorned, undogmatic, though deeply spiritually inspired, practice of observation and visualization.

Willis had come to his anatomical viewpoints by working with other anatomists like William Petty, Robert Boyle and Richard Lower. He had familiarized himself with the intravenous injection of colored liquids, the possibility of preserving organs in pure alcohol, and the relatively unknown method of dissecting heads from the base of the brain upwards. These and other material and social interactions (such as the calibration of observation between Royal Society members) helped shape Willis' particular way of knowing the brain. But the interactions also had an additional effect. Interfering with the brain itself through social, technical and material interventions, literally led the brain to stand on its own, as an independent organ. Prioritizing structure in this particular way had ontological consequences: what the brain 'truly' looked like had changed after Willis published the *Cerebri Anatome*. The use of alcohol as preservative, and the dissection from below, had turned the brain into a more solid mass, a structure that could now be more easily manipulated. The circle of veins that got fleshed out when Willis injected the brain with colored liquids, is another example of how interactions between social, material, and technological elements constituted the brain.

After these ontological reflections, a bit more can be said of Thomas Willis's epistemic routines. For Willis, achieving truth to nature was a complex collective process, in which the mobilization of expert witnesses was crucial. A first hint of collective empiricism can be found on the frontispiece of Willis' book (see figure 2). What immediately catches the eye is how the scene of this particular dissection is set. Compare this to much older dissection practices, in which the anatomist usually stood *ex cathedra*, watching over a surgeon cutting open the corpse in a public anatomy theatre. On Willis' frontispiece, the number of experts has multiplied: a group of distinguished gentlemen, all equal in eminence, discuss their empirical observations in one of their work spaces. The same approach can be found on the book's first pages. In the preface, Willis graciously acknowledges the help of two "most famous Men:" dr. Thomas Millington and dr. Richard Lower. A third tribute was paid to Wren, who made the atlas "more exact" with the copper engravings of the brain. This was not just courtesy. Both the frontispiece and the preface communicate that the anatomical findings in Willis' book were the result of a collective process of fact-making.

As we have seen, Royal Society members had built a culture of expert witnessing, adapted from the legal arena, as a way to cope with the complexity of human experience. At the same time, the personal interventions of image makers were ostensibly brought into the limelight. Around mid-19th century, a different practice of objectivity became prevalent: nature increasingly came to be perceived as ontologically separate from humans. According to Daston & Vidal (2004), this neo-Kantian world-order drew a line between a neutral, passive world on one side of the ontological divide, which supposedly lent itself to objective analysis only through the passionate eradication of the self, on the other side of the divide (see also Levine, 2002). As a consequence, much more weight was given to methodological criteria that stressed non-intervention and mechanical rigor.

The introduction of the ideal of a mechanical objectivity roughly coincided with photography's inauguration in 1839 (Daston & Galison, 1992, 2007). Values of disinterestedness, rigor, and precision were readily attributed to the new technology. Much like today's brain scanning techniques, the photographic 'faithful depictions of reality' held the promise of considerable contributions to neurology. However, it took more than thirty years for the first photographic atlas on the brain and nervous system to appear: Jules Bernard Luys' *Iconographie Photographique des Centres Nerveux* (Paris, 1873). Interestingly, after the publication of the first photographic atlas, the use of the new technology remained rare in macroscopic neuroanatomy. Most neuroanatomists maintained an ambiguous stance toward using photographs in their works. Why was this so? Why did photography fail to offer a satisfactory alternative to drawing or engraving? Inspired by Daston (1991, 1992, 2008), I analyzed how notions of 'objectivity' and 'subjectivity' were re-shaped in the 19th century, and how these distinctions were acted out in neuroanatomical practice. Photography promised images uncorrupted by judgment. In the words of Donna Haraway, they were fetishized as "non-tropic, metaphor-free representations of previously existing 'real' properties of a world that are waiting patiently to be plotted." (Haraway, 1999, p. 184) Photography, a mechanical technology, was perceived as a means to get at the 'real truth' of things (Mitchell, 1992). Jules-

Bernard Luys provided his readers with a large amount of photographs of brain slices. The idea was to use a mechanical, impersonal visualization technique, and leave the 'burden of representation' with the audience (Daston and Galison, 1992, 2007). But in neuroanatomy, photography was also deemed problematic. Its images did not schematize, were not selective, unlabeled, and either were too detailed or too nebulous. By switching over to photography, neuroanatomists would do away with the power of discernment that is so characteristic of the use of drawings.

Although photography never really caught on in neuroanatomy, one could argue that the technique did have an effect on how scientific research was organized, and may even have created a new 'visual regime' (Prasad, 2005). To begin with, photography had profound effects on lithographic practice. Whereas lithographs of the brain had thus far been rooted in mimesis (albeit the neo-classical, ideal-typical version), they progressively took photographs as their benchmark of reality (cf. Van Dijck, 2006). Lithographs were not abolished, but they increasingly came to resemble photographs of brain slices (as can be seen in Jules-Bernard Luys atlas, for instance). In addition, the demands of photography most likely also influenced neuroanatomical dissection techniques, and vice versa. As was the case in the days of Vesalius and Willis, contemporary dissection practices were intimately tied to other material and social interactions. The practice of making very thin brain slices, for instance, probably also played a role in the diminishing interest in broader morphological criteria. Moreover, the practice of cutting thin slices still affects present-day brain imaging techniques. We are still accustomed to seeing very thin slices of the brain. But this time around, the slices are digital.

Above, the ideal of a mechanical objectivity was used to describe certain epistemic and moral imperatives in nineteenth century neuroscientific practice. As we have seen, these imperatives created a newly felt tension around intentionality in neuroanatomical research: although *in theory* neuroanatomists were determined to decrease intentional latitude, they soon found this to be unattainable *in practice*. Intriguingly, this was already manifest in the first atlas of the brain and nervous system to contain photographs - the very images praised by their author for their ‘précision photographique’.

With the introduction of the ideal of a mechanical objectivity in the nineteenth century, scholars began to draw ontological boundaries between themselves and their research subjects. And this is where mechanical instruments came in: they served to rein in on the idiosyncrasies of observation and visualization (Daston & Galison 1992, 2007). The notion of mechanical objectivity worked well to describe the introduction and impact of a particular epistemic ideal in neuroanatomy. However, the analysis also revealed that we need to look beyond that notion if we want to describe the concrete co-dependence of embodied knowledge, material and culture, social customs, practical skills, and visualization techniques. This was the purpose of the third case study, on Santiago Ramón y Cajal ’s practices of observing and visualizing the neuron.

The lives of Jules-Bernard Luys and Santiago Ramón y Cajal partly overlapped: Luys was born in 1828 and died in 1897, while Cajal lived from 1852 until 1934. Both Luys and Cajal were scientifically active in the second part of the nineteenth century. And both embodied the ideal of a mechanical objectivity. Like Luys, Cajal too, tried hard to rule out any preconceived notions, biases, or wishful thinking. He was very unsympathetic to colleagues who - in his eyes - failed do the same. He, too, believed there was a causal relationship between the image and the object represented. A drawing or a photograph of a nerve cell could serve as proof of the existence of the nerve cell, and also as evidence of the particulars of its visual appearance (although in the case of the drawing this was under the condition that the hand was turned into a ‘precision instrument’). And there is a final similarity. Like Luys and many of his contemporaries, Cajal too, failed to see photography as a satisfactory alternative to drawing. I have expanded on why this was the case, but also tried to further the analysis by explicitly focusing on interactions between material, methodological, and social elements in his practices of observation and visualization. The goal was to describe how these elements bear upon scientific objects – in this case nerve cells.

Cajal and his contemporaries wanted to understand the structure and the position of nerve cells, in relation to other cells. In order to do so, they stained them, observed them closely, and transferred their shape onto paper. After Cajal had first seen a preparation stained black in 1887, his early experiences with the chrome silver method immediately led to a number of articles. An analysis of the papers he published between 1888 and '89 revealed that he tried hard to improve the technique. On the basis of these early experiments, Cajal initially agreed with colleagues that the stain was unreliable. But the consistency of his findings finally made him argue against anastomoses (structural connections) between nerve cell endings. The stain - rather coarse compared to other histological techniques - isolated only a small number of apparently discrete nerve cells, leaving its neighboring cells untouched. Cajal took this abstraction further in his excellent diagrammatic visualizations of brain tissue.

There were methodological and material reasons for the excellence of Cajal's drawings. Nerve cells spread out in many directions and over long distances. Therefore, brain sections stained with the Golgi method needed to be relatively thick. But even after the tissue was stained, it took a lot of practice and very strong magnifying lenses, to abstract discrete forms out of these complex structures. Observing relatively thick slices of nervous tissue entailed repeatedly shifting the focal distance. It was a process that stretched out over a certain amount of time. A complete image of a cell was arrived at by putting together the pieces of the puzzle in the observer's mind, as it were. Drawing aided this analysis. Cajal turned out to be particularly good at bringing out fundamental aspects, and playing down superfluous detail. He quite literally reduced highly complex, dynamic viewing experiences into flattened, diagrammatic, comprehensive drawings of neurons.

From Cajal's own perspective, there was also another side to drawing. He saw drawing as an essential tool in disciplining the eye. The crux of the matter was a difference between ordinary and expert observation, or so it was thought at the time. To a degree, this difference depended on talent. But, more importantly, histologists believed it was a matter of discipline and willpower. Ideally, Cajal reasoned, the artist and the microscopist were united in one person. Although the microscopist has learned how to interpret what is seen, the artist had the experience of intense, repeated, observation.[73] He was trained to pay close attention to the world around him, and to translate his observations into visual images. Incidentally, this proved difficult, if not impossible, to achieve with photography. According to Cajal, photography was unsuitable for visualizing nervous tissue, because it sliced up the mental phenomenon that abstracted over multiple focal planes. This explains Cajal's commitment to drawing, despite the fact that he was also an excellent photographer.

Drawings are sometimes assumed to be mobile embodiments of knowledge claims, figurative 'mobiles' that have an intrinsic power autonomous from other sorts of arguments and demonstrations (cf. Latour, 1987). In Cajal's case, what needed to circulate was a way of seeing, that had spatial, temporal, theoretical, and material dimensions. After having spent years doing histological research in his Spanish laboratory, Cajal wanted to immerse himself into a larger neuroanatomical network. In order for him to enforce an international breakthrough, sending out illustrated articles was not enough. Cajal's physical presence at a conference in Berlin, his commentaries, his painstaking demonstrations and explanations, were conditions of possibility for his drawings to become understandable embodiments of knowledge claims.

Many of Cajal's colleagues would in the end corroborate his findings on the individuality of nerve cells. The web of nervous tissue got entangled, and the number of histologists who believed cells and fibres formed a continuous network diminished. In the process, histologists used each other's images to guide their own visualizations. The drawings calibrated the appropriate perception and attention. The production and usage of visualizations of nerve cells transformed the neurohistological field. But not permanently. In the final stages of Cajal's career, the field shifted its focus. A new generation of histologists became more interested in cytoarchitectonics, the study of the arrangement of neuronal cell bodies in the cerebral cortex, and its functional subdivisions. Their ways of interfering with the microscopic world, and the questions they asked, differed from Cajal's. Unfortunately for him, this implied that people lost interest in the ways of the neuron. This revealed that the question of neuronal connectivity was strongly tied to particular practices of observation and intervention, with its specific representational techniques, epistemological approaches, modes of communication, and ontologies of the nervous system.

The careers of Jules-Bernard Luys and Santiago Ramón y Cajal came into full stride in the heyday of mechanical objectivity. Today, however, we can still relate to the firm realism behind their take on visualization. According to Bruno Latour, "for most people, [scientific images] are not even images, but the world itself. There is nothing to say about them except learning their message. To call them image, inscription, representation (…) is already an iconoclastic gesture." (2002, p. 19) Donna Haraway had already countered this widespread assumption a couple of years prior to Latour, by claiming that "all eyes, including our own organic ones, are active perceptual systems, building in translations and specific *ways* of seeing, that is, ways of life. There is no unmediated photograph or passive camera obscura in scientific accounts of bodies and machines; there are only highly specific visual possibilities, each with a wonderfully detailed, active, partial way of organizing worlds." (Haraway, 1999, p. x)

Incidentally, stressing the mediation behind scientific images only makes sense in a specific regime of objectivity. Latour aptly argued that while elaborations on the layeredness of art works only contributes to their status, the same does not – yet - hold for scientific images (Latour, 2001. See also Elkins, 1999). Most likely, the reason for this discrepancy is that some of the epistemic virtues and vices of mechanical objectivity are still with us today. We can see the dynamics at work in the reliance on the experimental practices of randomized controlled clinical trials (Dehue, 2001, 2002) and the faith in statistical significance testing (Hacking, 1984; Porter, 1996), but also - most relevant for present purposes - in the automated authority ascribed to brain scanning technologies (Beaulieu, 2001; Cohn, 2004; Dumit, 2004; Joyce, 2008).

To a degree, brain scans are appealing sources of knowledge because of their apparent transparency. We usually take them to be mechanically objective windows on the brain; figurative snapshots that are often assumed to follow culturally familiar conventions of photography (De Rijcke & Beaulieu, 2007). But there is a tension in this comparison with photography. As already been pointed out by Joyce (2008) and Prasad (2007), brain scans are not the result of reflections or absorptions of light onto a sensitive surface, as is the case with photographs. Arguably, they are 'abstractions of the visual' (Crary, 1990). They do not have an 'optical' relationship to the anatomical structure they represent, nor to the position of an observer. I have described some of the implications of this abstraction by analyzing the first human white matter atlas created via magnetic resonance imaging (MRI). The atlas served as a bridge between Cajal's work on nerve cell endings and more recent attempts to visualize white matter structure.

With MRI, a very powerful computer processes nuclear magnetic resonance signals, represented as mathematical data, and translates them into visual representations of the body (part) in the scanner. In the early 1980s, researchers began to use MRI for the visualization of the diffusional properties of water molecules. This image parameter is grounded on the principle that in bounded areas of the body, such as cell tissue, the displacement of water molecules is not free but restricted. Researchers use the image data to define the shape of these bounded spaces by measuring the shape and magnitude of the diffusion, and the orientation of the medium - in this case axons. The information is subsequently translated into reconstructions of neuronal projections.

Because of its potential to discriminate between diseased and healthy tissue, diffusion-weighted imaging was quickly recognized as an important tool in clinical neuroradiology. In 2006, the first MRI human white matter atlas was published by a group of neuroradiologists, headed by Susumu Mori. The atlas consisted of high resolution two- and three-dimensional diffusion-weighted visualizations of the major human white matter tracts.

Mori's research group had an audience of radiologists and surgeons in mind for their atlas: like the authors themselves, they deal with connectivity impairment on a regular basis. From the first atlas pages onwards, the authors 'enroll' their audience by explicitly placing their images in the pictorial tradition of neuroradiology. The authors chose what they label as "conventional" MRI as a point of reference for DWI, the "unique new MRI modality." (Mori et al., 2005, p. x)

In the visual culture of neuroradiology, MRI borrows its authority from a 'myth of photographic truth' (term borrowed from Sturken & Cartwright, 2001). Radiology's visual culture leans heavily on a photorealistic x-ray and CT aesthetic. By placing the atlas images in a tradition that hinges on the aura of an unmediated access to reality, the authors opened the door to the hypermediated diffusion-weighted images, key to the atlas. These new images are much more malleable, interactive, and color-coded than their 'conventional' counterparts. They do not attempt to mimic the natural appearance of nervous tissue, and it seems that unmediated representation is gradually left behind as most important epistemic aspiration. In these diffusion-weighted images, a sense of immediacy is achieved not by *erasing* but by *accentuating* their mediatedness.

The complex nature of the diffusion-weighted images might be the reason why Mori and his colleagues chose for the visual comparison of MRI with DWI. This convinced viewers to trust DWI as a new imaging parameter, while at the same time 'easing' the viewer into the color-coded images, which - according to the authors - contain the "greatest amount of information regarding white matter anatomy." (Mori et al. 2005, p. 4)

Further analysis of the precise characteristics of DWI's new ways of seeing and knowing was done by analyzing the software used to create Mori et al.'s white matter images. One of the software's salient characteristics is that visual and manual processing is neither marginalized, nor fully integrated into the technical equipment and/or software. There is a continuous interplay between the digital objectivity of the computer and the expert judgment of the user. Compared to mechanically produced 'optical' images such as photographs, which are also heavily dependent on decisions as to lighting, framing, type of film, etcetera, the production of diffusion-weighted images leaves the image makers with much more time and opportunity to intervene. Users are invited to engage in a process of continuous intervention, add or extract colors, define regions of interest in the image, or subject parts of the image to several algorithmic filters. The shift from transparency to hypermediacy deeply effects both image production and the images themselves. In this process, ‘real’ or ‘objective’ images are not static visualizations of brains, but flexible tools that enable further processing.

In conclusion, it should be noted that the phrase ‘visualizations of brains’, as used in the sentence above, already presupposes a distinction I have tried to historicize in this thesis. The phrase presupposes a modernist divide between an object-represented (the brain) and a subject that does the representing, via the images. I have described how this divide entered 19th century scholarly practice, and have analyzed how neuroscientists in France, Spain and abroad dealt with the representational anxieties the distinction brought about. These scientists came to believe that human decisions, their interventions, their intrusive eyes and hands, were no longer congruent with objective ways of knowing the brain. At present, however, this singular vision of a mechanical objectivity is gradually replaced by partial, situated knowledges (cf. Haraway, 1999). This can be concluded from the analysis of Susumu Mori’s diffusion-weighted white matter images. In neuroradiology, practices of objectivity are progressively hinging on hypermediacy and explicit mediation. If we assume this to be part of a broader movement, as described by Daston and Galison (2007), this means that analyses that describe, and try to understand, the mediation in scientific visualization will gradually become redundant. These analyses are themselves artifacts of specific practices of objectivity: in the case of this thesis, for instance, a practice dominated by a materialist-realist epistemology. Putting an emphasis on mediation is only pertinent when the practices of knowledge production *themselves* lack a sensitivity for the analytical issues raised by for instance scholars in STS, media studies, or history of science. Recognizing that this is gradually changing, leaves more room for studies that take the reflexive relationship between visualization and other material, technological, and social factors as starting points for further inquiry. With this thesis, I hope to have demonstrated that scientific visualization is not about representing reality, but about making visible (Norton Wise, 2006). And making things visible implies making them real – in wonderfully situated and historically specific ways.

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Samenvatting (Summary in Dutch)

In de neuroanatomie zijn zowel observeren als afbeelden al eeuwen intiem verweven met een scala aan technieken en conventies. Een belangrijke ontwikkeling was bijvoorbeeld de komst van geografische kaarten in de Renaissance. Het gebruik van schaal, perspectief en andere projectietechnieken hadden een grote invloed op Westerse afbeeldingspraktijken, een invloed die je vandaag nog terug ziet in bijvoorbeeld hersenscans. In diezelfde periode werd het gebruikelijker om afbeeldingen te gebruiken als onderdeel van wetenschappelijke bewijsvoering. Onder de bekendste voorbeelden van afbeeldingstechnieken bevinden zich in de neurowetenschappen de camera obscura, de microscoop, de fotocamera, het elektro-encefalogram (EEG), en uiteraard de hersenscanner. Dergelijke technieken maken het mogelijk om de hersenen op een bepaalde manier waar te nemen en weer te geven. De traditionele wetenschapshistorische opvatting ziet technische innovatie alleen als vooruitgang (cf. Clarke & Dewhurst, 1972). Volgens deze opvatting zijn hedendaagse afbeeldingen van hersenen objectiever dan bijvoorbeeld 19^{e}-eeuwse litho's en foto's, of 17^{e}-eeuwse tekeningen en gravures van de hersenen. In dit proefschrift beschouw ik 'objectiviteit' echter niet als een *a priori* categorie, maar als een historisch variabele uitkomst van bepaalde empirische onderzoekspraktijken (Barad, 2007; Daston, 1991, 1992; Daston & Galison, 1992, 2007). De dissertatie is gebaseerd op een theoretisch georiënteerde wetenschapsgeschiedenis. De nadruk ligt op de toepassing van theorieën uit de historische epistemologie op afbeeldingen van het brein. Historische epistemologie is het onderzoeksveld dat zich bezighoudt met de historische ontwikkeling van kennis, met inachtneming van relevante technische, sociale, intellectuele en culturele invloeden. Zelfs de meest centrale onderdelen van wetenschappelijke rationaliteit, zoals het begrip 'feit', of 'bewijsvoering', of 'objectiviteit' zijn historisch gegroeid; de geschiedenis van deze begrippen kunnen we het best bestuderen door tegelijkertijd te kijken naar abstracte ideeën (bijvoorbeeld filosofische debatten over wat wordt verstaan onder ‘bewijs’) en concrete praktijken (bijvoorbeeld hoe afbeeldingen worden gemaakt en gebruikt).[74] Vergelijkingen tussen verschillende historische periodes en culturen zijn essentieel voor dit type geschiedschrijving. Een consequente analyse vanuit functie en gebruik laat zien dat de hersenen niet alleen zijn afgebeeld met behulp van opeenvolgende nieuwe technieken, artistieke en medisch-wetenschappelijke conventies, maar dat omgekeerd de hersenen tot op zekere hoogte ook het product zijn van uiteenlopende afbeeldingspraktijken.

In dit proefschrift analyseer ik vier verschillende periodes in de geschiedenis van het verbeelden van de hersenen, verspreid over vijf eeuwen. Centraal staat de rol van afbeeldingen in relatie tot verschillende manieren van kennisverwerving en kennisverspreiding over de hersenen. Twee van de belangrijkste uitkomsten zijn als volgt samen te vatten. Ten eerste: de objectiviteit van wetenschappelijke afbeeldingen is een uitkomst van wetenschappelijke technologische, sociale, culturele, en historisch specifieke mediatie. Dit betekent niet dat de afbeeldingen of de kennis die ermee wordt gecommuniceerd minder betrouwbaar zijn. Het tegenovergestelde is eerder het geval (Latour, 2002). Zonder de aandacht die onderzoekers in de productie en verspreiding van het beeldmateriaal steken, zonder hun instrumentarium, samenwerkingsverbanden met tekenaars, foto- en lithografen, zonder druktechnieken, hun onderzoekservaring, dure scanapparatuur, financiële middelen, en hun liefde voor het brein, was er niets te zien op de afbeeldingen. Deze mediaties zijn essentieel voor hun objectiviteit.

Ten tweede: mijn analyse van neuroanatomische afbeeldingen en afbeeldingspraktijken heeft bevestigd dat objectiviteit een gelaagd en historisch veranderlijk begrip is (Daston, 1991, 1992; Daston & Galison, 1992, 2007). Alle afbeeldingen die ik heb onderzocht speelden een actieve, belangrijke rol in het vorm geven aan neurowetenschappelijke kennis. Maar hoe hun betekenis tot stand kwam, hoe ze objectief werden gemaakt, wat objectiviteit betekende, hoe het brein ontologisch vorm kreeg, verschilde per casus.

In de eerste casus stonden de kopergravures van de hersenen in Thomas Willis' *Cerebri Anatome* centraal. Willis, een natuurfilosoof uit Oxford, deed sinds het midden van de jaren 1650 onderzoek naar de hersenen, samen met drie andere prominente leden van de pas opgerichte Royal Society. Deze samenwerking resulteerde in 1664 in de publicatie van de *Cerebri Anatome*. Het boek verscheen in 1681 onder de titel *The Anatomy of the Brain and Nerves*. Het was een van de eerste boeken waarin de cortex als een relevante materiële structuur werd gezien.

In het hoofdstuk laat ik zien dat de kopergravures in Willis' boek een belangrijke strategische rol vervulden. Willis was zoals gezegd lid van de Royal Society, een groep natuurfilosofen wier empirische insteek duidelijk afweek van andere onderzoekstradities. In hun natuurfilosofie benadrukten deze onderzoekers onder andere het belang van het experiment, en de aanwezigheid van getuigen bij experimentele demonstraties. Natuurgetrouwe afbeeldingen speelden een cruciale rol in deze onderzoekstraditie. Wat deze onderzoekers onder natuurgetrouwheid verstonden, week echter erg af van eerdere natuurfilosofische verhandelingen over het lichaam. In het hoofdstuk vergelijk ik Willis' kopergravures met de houtsnedes uit een belangrijke 16^e^-eeuwse atlas, waarin het brein in steeds gevorderder staat van ontleding is afgebeeld. Deze atlas, in 1543 gepubliceerd door Andreas Vesalius, is een product van een typisch humanistische visie op de natuur. Natuurfilosofen als Vesalius presenteerden zich als doorgeefluik van God. Zij bezaten de expertise en het privilege, zo dachten zij, om de natuur weer te geven als het prachtige resultaat van Gods schepping. Dit zie je terug in de afbeeldingen van hersenen, waarin de schoonheid van het menselijk lichaam centraal staat, en oneffenheden geen plaats hebben. De toeschouwer kijkt als het ware mee over de schouder van de anatoom. Het geometrische perspectief benadrukt deze invidualistische benadering nog eens.

In de tijd van Thomas Willis, ruim een eeuw later, werd dit centrale gezichtspunt van de natuurfilosoof vervangen door een perspectief waarin vaststaande proporties en ook de menselijke maat steeds meer werd gerelativeerd. Dit was onder andere het gevolg van het toenemende gebruik van lenzen (microscopen, telescopen, etc.). Toen het menselijk oog hulpmiddelen bleek nodig te hebben om het heel nabije of het oneindig verre in beeld te brengen, werd een centrale kentheoretische rol voor de individuele mens onhoudbaar, zo vond men.

Deze verschuiving zie je terug in de afbeeldingen in *The Anatomy of the Brain and Nerves*. De kopergravures van Willis verwijzen niet expliciet naar een menselijke maat (bijvoorbeeld door delen van de snijtafel, de hand van de anatoom, of een deel van de schedel te tonen). De suggestie van een concrete, individuele perceptie staat niet centraal. In plaats daarvan lijken de hersenen te zweven, en zijn de gravures zeer symmetrisch, haast architecturaal. Dat Christopher Wren de gravures maakte lijkt logisch. Deze natuurfilosoof, met oog voor structureel ontwerp, werd niet veel later, na de Great Fire in 1666, gevraagd om St. Paul's Cathedral in Londen te ontwerpen.

Wat verder opvalt aan de kopergravures, is hoe ze de anatomische vindingen van Willis heel gericht lijken samen te vatten. Willis was tot zijn standpunten gekomen door een aantal materiële, technologische en sociale interventies. Het conserveren van anatomisch materiaal in alcohol was in die tijd nieuw, net als het injecteren van bloedvaten met kleurstof, en het ontleden van de hersenen van onderaf in plaats van bovenaf. Deze en andere technologieën (zoals het kalibreren van observaties door Royal Society-leden) gaven mede vorm aan Willis' ideeën over de hersenen. Maar de interventies hadden daarnaast ook een ander effect: ze zorgden er letterlijk voor dat het brein op zichzelf kwam te staan, als een autonoom, manipuleerbaar orgaan. Een dergelijke prioritering van materiële structuur had dus ontologische consequenties.

Ten slotte nog iets over Willis' kentheoretische gewoontes. Voor hem en andere Royal Society-leden was het bereiken van natuurgetrouwheid een complex collectief proces, waarin het mobiliseren van getuigen een cruciale rol speelde. Ook dat zien we terug in *The Anatomy of the Brain and Nerves*, bijvoorbeeld in het voorwoord. Daarin bedankt Willis zijn collega's dr. Thomas Millington en dr. Richard Lower, die hem op anatomisch vlak te hulp kwamen. Een derde collega waar uitgebreid dank naar uitging was Wren, die het boek volgens Willis 'exacter' had gemaakt door de kopergravures te maken. Deze uitvoerige dankbetuigingen schreef Willis niet slechts uit beleefdheid, maar ook om duidelijk te maken dat de anatomische vondsten het resultaat waren van een collectief proces van feitenproductie. In de onderzoekscultuur van de Royal Society-leden speelden getuigenverklaringen van betrouwbare experts een cruciale rol. Deze traditie was voortgekomen uit de juridische wereld, en fungeerde als vangnet om de complexiteit van de menselijke ervaring te ondervangen.

De interventies van natuurfilosofen zelf werden in de tijd van Andreas Vesalius en ook Thomas Willis uitgebreid voor het voetlicht gebracht. In het midden van de negentiende eeuw begonnen zich andere epistemische praktijken af te tekenen. Het ideaal van mechanische objectiviteit deed haar intrede, een onderzoeksstrategie waarin mechanische technieken en statistische analyses een belangrijker rol gingen spelen, en – idealiter - menselijke interventie zo veel mogelijk naar de achtergrond werd gedrongen. Dat zo'n onderzoeksmodel opgeld deed, was mede het gevolg van een neo-Kantiaans wereldbeeld. Daarin werd een lijn getrokken tussen een 'neutrale' wereld aan de ene kant van de ontologische grens, een wereld die slechts objectief bestudeerd kon worden door een zichzelf wegcijferende onderzoeker aan de andere kant van de grens (Daston & Vidal, 2004, Levine, 2002).

De invoering van mechanische objectiviteit viel ongeveer samen met de introductie van fotografie in 1839 (Daston & Galison, 1992, 2007). Al snel werden waarden als neutraliteit, grondigheid, en precisie toegeschreven aan de nieuwe technologie. Net als in het geval van hedendaagse hersenscans, werden de fotografische 'rechtstreekse verbeeldingen van de realiteit' geacht belangrijke bijdragen te kunnen leveren aan de neurologie. Toch duurde het meer dan dertig jaar voordat de eerste fotografische atlas van de hersenen verscheen: Jules Bernard Luys' *Iconographie Photographique des Centres Nerveux* (Paris, 1873). Gek genoeg bleef het gebruik van fotografie in de neuroanatomie heel zeldzaam na publicatie van die eerste atlas. In de tweede casus heb ik onderzocht waarom veel neuroanatomen sceptisch waren en bleven ten opzichte van fotografie. Uit mijn analyse kwam naar voren hoe de begrippen 'objectiviteit' en 'subjectiviteit' een nieuwe invulling kregen in de negentiende-eeuwe neuroanatomie. Jules-Bernard Luys nam een grote hoeveelheid foto's van hersenplakken op in zijn atlas. Zijn idee was om een mechanische, onpersoonlijke afbeeldingstechniek te gebruiken. Dit zou de oordeelsvorming niet voorafgaand aan het afbeelden laten plaatsvinden, maar juist achteraf, en niet door hem maar door zijn publiek. Maar, zo bleek, fotografie werd ook als problematisch beschouwd. Foto's schematiseerden niet, waren niet selectief, waren soms te gedetailleerd en soms niet gedetailleerd genoeg. Overstappen naar foto's, zo dachten de meesten, zou betekenen dat neuroanatomen de kracht van oordeelsvorming kwijt zouden raken die zo karakteristiek was voor tekeningen. Daartoe waren de meesten niet bereid.

De kentheoretische en morele verplichtingen van een mechanische objectiviteit creëerden in de neuroanatomie een nieuw spanningsveld rond intentionaliteit: hoewel anatomen in theorie hun eigen speelruimte wilden inperken, vonden ze dit in praktijk niet haalbaar. Dit spanningsveld was al zichtbaar in de eerste fotografische atlas van de hersenen, waarin Luys zowel foto's opnam als verklarende lithografieën.

Het begrip mechanische objectiviteit werkte goed om de introductie en impact van een bepaald kentheoretisch ideaal in de neuroanatomie te beschrijven. Maar de analyse legde ook bloot dat het begrip minder goed werkt voor een analyse van de concrete wederzijdse relatie tussen kennis, materie, cultuur, sociale gebruiken, praktische vaardigheden en afbeeldingstechnieken. Dit was het doel van de derde casus, waarin de observatie- en visualisatiepraktijken in het werk van Santiago Ramón y Cajal (1852-1934) centraal stonden.

Cajal en zijn tijdgenoten waren geïnteresseerd in de structuur en positie van zenuwcellen, in relatie tot andere cellen. Ze kleurden de zenuwcellen, bestudeerden ze onder de microscoop, en vertaalden hun observaties in tekeningen en foto's. In 1887 zag Cajal voor het eerst een histologisch preparaat bewerkt met de Golgi kleuring (een techniek die ongeveer 5% van het aantal zenuwcellen in een preparaat zwart kleurt, met gebruikmaking van chroom en zilver). Niet lang daarna paste hij de kleuring in zijn eigen laboratorium toe, wat al snel leidde tot een stroom publicaties. Tot die tijd stond men - Golgi incluis – op het standpunt dat zenuwcellen zich in een ononderbroken netwerk bevonden, en dus geen afzonderlijke eenheden waren. Uit mijn analyse van de artikelen die Cajal publiceerde tussen 1888-1889 blijkt dat hij heel hard probeerde de kleurtechniek te verbeteren. Op basis van zijn eerste experimenten moest Cajal zijn collega-histologen echter gelijk geven over de onbetrouwbaarheid van de kleuring. Maar omdat hij toch geïntrigeerd bleef, en de kleuring steeds meer naar zijn hand zette, vond hij uiteindelijk vrij consistent bewijs tegen het bestaan van structurele verbindingen tussen zenuwcellen. De Golgi kleuring isoleerde een beperkt aantal ogenschijnlijk onafhankelijke cellen tegen een transparante achtergrond. Cajal maakte van zijn observaties vervolgens nog abstractere, soms haast schematische tekeningen van hersenweefsel. Maar de kleuring bleef controversieel, en daarmee zijn anatomische claims en tekeningen ook. De tekeningen gingen echter een rol spelen in het overtuigen van andere onderzoekers. Dat ging zeker niet vanzelf.

Tekeningen worden soms beschouwd als tegelijkertijd verplaatsbare en stabiele belichamingen van kennis. Ze zouden een intrinsieke kracht hebben, en autonoom kunnen fungeren ten opzichte van andere typen argumenten of demonstraties (cf. Latour, 1987). In Cajal's geval lag het gecompliceerder. Hij wilde collega-onderzoekers uiteraard overtuigen van zijn gelijk. Daarvoor moesten niet alleen de tekeningen, maar een hele manier van zien gaan circuleren onder collega's. Zijn doorbraak kwam niet nadat hij een reeks artikelen met afbeeldingen opstuurde naar collega's, maar pas toen hij een congres in Berlijn bezocht. Cajal's fysieke aanwezigheid, zijn eigen microscoop, zijn preparaten, zijn uitleg, en zijn demonstraties vormden de noodzakelijke condities voor het begrijpen van de tekeningen en van zijn beweringen.

Cajal zelf dichtte tekenen trouwens ook nog een heel andere eigenschap toe. Tekenen was voor hem een essentieel middel om het oog te trainen, en het kijkproces te sturen. Idealiter, zo redeneerde hij, waren kunstenaar en histoloog verenigd in één persoon. Het was weliswaar de *histoloog* die had geleerd hoe hij moest interpreteren wat hij zag, maar het was in eerste instantie de *kunstenaar* die überhaupt wist hoe echt goed kijken in zijn werk ging. De kunstenaar had immers geleerd hoe hij met aandacht moest observeren, en deze observaties vervolgens te vertalen in afbeeldingen. Tekenen was voor Cajal cruciaal voor begripsvorming.

De kracht van Cajal’s tekeningen had een methodologische en een materiële kant, zo beargumenteer ik verder in het hoofdstuk. Zenuwcellen vertakken, en verspreiden zich over vrij grote afstand in hersenweefsel. Voor histologisch onderzoek naar zenuwcellen moesten hersenplakjes daarom vrij dik zijn. Het vereiste enorm veel oefening en heel sterke lenzen om vrij concrete vormen te onderscheiden in die complexe wirwar van vertakkingen, zelfs nadat slechts een deel van de cellen gekleurd was. Een histoloog moest voortdurend de focusafstand van zijn microscoop aanpassen en het hersenplakje onder de lens heen en weer bewegen. Het kostte dus letterlijk enige tijd. Een compleet beeld van een zenuwcel bestond eigenlijk alleen in het hoofd van de histoloog, die als het ware steeds de afzonderlijke puzzelstukjes bij elkaar moest brengen tot één beeld. Tekenen hielp bij dit ingewikkelde proces, zo blijkt uit Cajals werk. Hij bleek erg goed in het abstraheren van de informatie die hij zag terwijl hij keek. Het eerste niveau van abstractie vond dus plaats bij de kleuring van het weefsel, waarin 95% ongekleurd en dus onzichtbaar bleef. Vervolgens benadrukte hij bepaalde details, en liet andere weg, terwijl hij tekende. Een dynamisch en complex kijkproces werd dus als het ware gevat in één beeld. Dit was absoluut niet mogelijk met fotografie, en verklaart ook waarom Cajal zo’n toegewijde tekenaar bleef, hoewel hij ook een succesvol amateur-fotograaf was.

De carrières van Jules-Bernard Luys en Santiago Ramón y Cajal speelden zich af in de hoogtijdagen van een mechanische objectiviteit. Toch kunnen wij ons tegenwoordig nog redelijk goed vinden in hun perspectief op realistisch verbeelden. Het gezag van hersenscans is daar een goed voorbeeld van (Beaulieu, 2001; Cohn, 2004; Dumit, 2004; Joyce, 2008).

Tot op zekere hoogte dichten wij hersenscans de kwaliteit toe om mechanisch objectieve blikken op het brein te werpen. We beschouwen ze als spreekwoordelijke ‘snapshots’ van de hersenen, als afbeeldingen die de conventies van de fotografie volgen (De Rijcke & Beaulieu, 2007). Maar de vergelijking met fotografie gaat niet helemaal op. Zoals al werd opgemerkt door Joyce (2008) and Prasad (2007), komen hersenscans helemaal niet voort uit reflecties op of opname van licht op een gevoelige plaat. Het zijn ‘visuele abstracties’ (Crary, 1990). In de laatste casus heb ik een aantal gevolgen van deze visuele abstractie beschreven. In het hoofdstuk analyseer ik de eerste digitale atlas van witte stof in de hersenen, gemaakt met behulp van 'magnetic resonance imaging' (MRI). De atlas verscheen in 2006, en werd gemaakt door een groep Amerikaanse neuroradiologen.

In de visuele cultuur van de neuroradiologie ontleent MRI als het ware zijn status aan de 'mythe van de fotografische waarheid' (een term die ik leende van Sturken & Cartwright, 2001). De esthetiek van Röntgenbeelden en CT-scans maken al heel lang de dienst uit in deze visuele cultuur. Mori en zijn collega's plaatsten hun atlas bewust in deze traditie, een traditie die zwaar leunt op het idee dat het mogelijk is om via visualiseringstechnieken onmiddellijke toegang te krijgen tot de 'realiteit'. Dit doen de auteurs bewust, zo betoog ik in het hoofdstuk, om ruimte te maken voor 'hypergemedieerde' verbeeldingen van het brein. Deze afbeeldingen zijn veel flexibeler en interactiever. Ook uit het overdadige kleurgebruik blijkt dat de makers niet de intentie hebben om een 'optische' werkelijkheid na te bootsen. In deze afbeeldingen wordt het idee van een rechtstreekse ervaring niet bereikt door allerlei interventies weg te poetsen, maar juist door deze te benadrukken en te stimuleren. Dat blijkt ook uit de software waarmee de afbeeldingen uit Mori's atlas zijn gemaakt. Eén van de opvallende eigenschappen is namelijk een continu samenspel tussen de digitale objectiviteit (Beaulieu, 2001) van de computer en de expertise van de gebruiker. Vergeleken met mechanisch geproduceerde 'optische' afbeeldingen als foto's, waarin uiteraard ook allerlei beslissingen worden genomen over belichting, compositie en dergelijke, is er in de productie van de witte stof-afbeeldingen veel meer tijd en gelegenheid om te interveniëren. Onderzoekers die de software gebruiken worden uitgenodigd om kleuren toe te voegen of juist weg te halen, bepaalde gebieden in de afbeelding te markeren, de afbeelding te laten bewegen, of op bepaalde onderdelen algoritmische filters toe te passen. Deze verschuiving van een nadruk op transparantie richting hypermediatie heeft grote gevolgen voor zowel de produktie en omgang met het beeld, als voor het beeld zelf. Met de komst van dit nieuwe type afbeeldingspraktijk zijn bepaalde vooronderstellingen over wat we verstaan onder 'objectief' verbeelden gaan verschuiven. Voorstanders van een mechanische objectiviteit streefden naar een onmiddellijke registratie van de natuur door de natuur zelf. Idealiter waren onderzoekers zichzelf wegcijferende registratoren, die de hersenen zelf ongemoeid lieten, voor zichzelf lieten spreken. Maar in de nieuwe afbeeldingen van witte stof kwamen de handvaardigheid, de expertblik en de anatomische bedrevenheid van onderzoekers epistemologisch op gelijke hoogte staan met de digitale dataverwerkingscapaciteiten van de scanner en computer. De afbeeldingen zijn bewust heel kneedbaar en flexibel. ‘Objectieve’ afbeeldingen van witte stof zijn juist die afbeeldingen die verdere bewerking mogelijk maken. We kunnen ze het beste zien als flexibel gereedschap, in plaats van statische representaties van hersenen.

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Endnotes

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[1] Visual and verbal excerpts of the debate can still be found on YouTube in 2009, for instance by using Terri Schiavo as search words. (http://www.youtube.com/results?search_type=&search_query=terri+schiavo&aq=0&oq=terri+s ; accessed 21-05-2009 12:29 h.)

[2] See for instance Wikipedia (http://en.wikipedia.org/wiki/Terri_Schiavo#Schiavo.27s_end-of-life_wishes_.E2.80.93_Schiavo_I ; accessed 21-05-2009 12:47 h).

[3] Beaulieu (2000) and Dumit (1994) both analyse how these paired presentations often bring into play the culturally familiar 'before' and 'after' photography. See also De Rijcke & Beaulieu (2007).

[4] On cultural differences in brain-death diagnostics, see Lock (2002).

[5] The term 'mechanical objectivity' was coined by Lorraine Daston and Peter Galison in their article *The Image of Objectivity* (1992). See also chapter 2 of the present dissertation.

[6] This is part of the research agenda of the Max Planck Institute for the History of Science, department II, as formulated on the website (http://www.mpiwg-berlin.mpg.de/en/research/projects/department2; accessed May 15 2009, 12:31 h).

[7] Title is derived from the conclusion of The Anatomy of the Brain and Nerves, in which Willis states the following: "Thus much for the Anatomy of the Brain and Cerebel, and of their Appendix, both Medullar and Nervous, and of the Uses and Offices of all the several Parts, of which we have largely treated. There yet remains, after we have viewed, not only the outward Courts and Porches of this Fabrick, as it were of a certain Kingly Palace, but also its intimate Recesses and private Chambers, that we next inquire into, what the Lady or Inhabitant of this Princely place may be, in what part she doth chiefly reside, and by what Rule and Government she disposes and orders her family." (Feindel 1965, p. 192)

[8] All quotes from Hall & Hall 1966, Vol. III, pp.122-125. They have been modernized to facilitate reading.

[9] Quote: *The Authors Dedicatory to his Grace Gilbert Archbishop of Canterbury, & c*. (Feindel 1965, no page)

[10] Wallis's letter reveals that the very act of cutting open a gentleman's head was no longer a violation in England. This is not to say that English people in general assented to the handling of corpses. In her abounding book *Death, Dissection and the Destitute* (Routledge/Kegan Paul, 1987), Ruth Richardson demonstrated that ordinary people did not want their dead bodies to be dissected, because they believed that a recently deceased remained sentient.

[11] Founded on 28 November 1660 after a lecture by Sir Christopher Wren. The first Charter passed on 16 July 1662. From then on the Society existed with the name of "The Royal Society." On 23 April 1663 it was renamed as King Charles II became the official founder of "The Royal Society of London for Improving Natural Knowledge." (http://www.royalsoc.ac.uk/royalsoc/) In 1665 Oldenburg was officially appointed editor of the *Philosophical Transactions*, the Society's journal, to appear on the first Monday of every month. Sir Robert Moray put the motion to the Council of the Society on 1 May 1665.

[12] Harvey had enjoyed the latter part of his medical training at the University of Padua, home of Andreas Vesalius and his successors Realdo Colombo, Fabricius of Aquapente (Harvey's teacher) and Constanzo Varolio. His new physiological ideas were not easily adhered to by his own generation and older. He was lucky enough to live to see his work accepted and extended by a younger generation of natural philosophers, both in Oxford and in London (Frank, 1980).

[13] Changeling is an archaism for mentally handicapped.

[14] Quote taken from the preface of *The Anatomy of the Brain and Nerves* (Feindel 1965, no page number)

[15] Willis in Feindel 1965, p. 59 and 63.

[16] His *Omnia Opera* for example was published in Venice in 1541. It was translated into Latin by at least four famous anatomists: John Caius, first master of Gonville and Caius College; John Linacre, founder of the Royal College of Physicians; Jacob Syvius, Vesalius's teacher in Paris, and Vesalius himself. (Gross 1998, pp. 31.)

[17] Blood was the most important humor, although the word "blood" could refer to a mixture of the substance with the other three humors as well as to pure blood.

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[18] In Latin, not in Greek, because Galen's work was translated into Latin in the 1540's.
[19] In the original text the two parts of the brain Burton referred to were not called the anterior and the posterior part, but the fore and hinder part of the brain.
[20] Term borrowed from Ford 1992, pp. 83. He used it in a description of developments in botanical painting around 1400.
[21] See for example Clarke & Dewhurst 1982, pp. 27-29; Ford 1992, pp. 83; Rose & Bynum 1980, pp.483-484.
[22] The drawing is a good example of the dynamic element that was added to the doctrine around 1000 A.D.: from then on the impressions created by the sensations in the first cell were thought to be manipulated in the second cell (the process of reasoning), and what was left over was stored in cell three (memory). (Clarke & Dewhurst 1972, pp. 10). The labels above the head read (from left to right) "sensus communis, fantasia, ymaginativa, vermis,[22] cogitativa, estimativa, memorativa." Reference: Sudhoff, W. (1913) Die Lehre von den Hirnventrikeln in textlicher und graphischer Tradition des Altertums und Mittelalters. Archiv. Fur Geschichte der Medizin VII, 149-205, figure 14. (Institute of the History of Medicine, The Johns Hopkins University)
[23] According to Clarke & Dewhurst (1972), Vesalius still considered the ventricles as storage site of the animal spirits.
[24] Descartes had given up on mathematics as a method for understanding nature in the late 1620's See Gaukroger, S. (2002). *Descartes's System of Natural Philosophy*. Cambridge: Cambrigde University Press; pp. 6-10.
[25] Willis in Feindel 1965, Preface to the reader.
[26] 'Epistle Dedicatory' *Two Discourses* (1672), sig. A3.
[27] Previously it was compulsory to start dissecting the head from top to bottom, because of the brain's watery makeup. A perfect illustration of this approach can be seen in Vesalius's *Fabrica*.
[28] Elsewhere a case is made for the exact opposite statement. In a lecture held in Leyden in 1915 on the occasion of Vesalius's tercentennial birthday, Jan Veth argued that no southern artist could have accomplished what Van Calcar accomplished in the *Fabrica*, because Van Calcar was originally a northern artist. Only a northern artist possessed the all-encompassing abundance of realism needed to record Vesalius's penetrating dissections as exactly as possible, Veth claimed. (Veth 1915, p. 270). Veth may be partly right: Van Calcar was trained in the workshops of Titian. The latter was criticized by Michelangelo for focussing solely on drawing after nature while disregarding *disegno*, or drawing with a focus on design and beauty.
[29] Many English virtuosi for example incorporated Aristotelian concepts in their work. (Shapiro, 2000)
[30] Related is a phenomenon Simon Schaffer recently labeled the "double vision of all iconophiles:" an insistence on immediacy while at the same time devoting oneself to social facts, artificial devices and the strength and number of linkages between natural philosophers and sponsorship, commerce and the monarchy. (Schaffer in Latour & Weibel 2002, p. 501-2)
[31] See for example the work of Lorraine Daston and Katherine Park, *Wonders and the Order of Nature, 1650-1750* (Cambridge, MA: Cambridge University Press, 1997), Peter Dear, "Miracles, Experiments, and the Ordinary Course of Nature," *Isis, 81* (1990), 663-683 and Simon Schaffer and Steven Shapin, *Leviathan and the Air Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985).
[32] Reputation and status were important, but not exclusive criteria. Witnesses of the facts were not necessarily gentlemen, or scholarly specialists. (Inwood 2002, p. 26, 29) Thomas Sprat emphasized in his *History of the Royal Society* (London, 1667) that "plain, diligent and laborious observers: such, who, though they bring not much knowledg, yet bring their hands, and their eyes uncorrupted (…)." could also play a valuable part. (Hunter 1989, p. 18).
[33] Willis in Feindel, 1965, p. 70.
[34] At another level Willis deliberately turned *The Anatomy of the Brain and Nerves* into a group effort by calling on Richard Lower, Thomas Millington and Christopher Wren as his witnesses.
[35] In the days of William Cheselden's *Osteographia* (1753) the camera obscura had become an aid in anatomical treatises. Janson & Janson 1992, p. 374.
[36] Jules Jansen was not the first to refer to the camera as "la rétine du savant." This was Jean Baptiste Biot, who announced the daguerrotype in the *Compte Rendus* on January 1 1839 (Wilder, 2003).
[37] Martensen (2004, p. 193) reasoned that "the predominant eighteenth century version of empiricism, which held, *pace* Locke and Sydenham, that deep investigations of structure were not capable of understanding the causes of nature, including human health and sickness, gradually gave way to the idea

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that observation, deep investigation of structure, and experimental manipulation had their place in medicine."

[38] Luys prepared his sections as follows: first, the tissue was hardened with chromic acid and a small amount of glycerine, was cut with a relatively large microtome, and then placed between two cover glasses. The tissue was subsequently stained in a porcelain bowl containing a concentration of soda, and immersed in alkaline, after which it was bathed in diluted chlorhydric acid (Trutat, 1884, p. 67-69). The exact photographic procedure utilized by Georges Luys is unknown. It took until the late seventies for Luys to familiarize himself with photography. One of his articles in the first edition of *L'Encephale* (1881) was accompanied by his own photomicrographs.

[39] Incidentally, Levine pointed to the inconsistency of the trope 'dying to know': Only by way of the "indescribably ambitious" goal of self-annihilation could a scientist attain "pure knowledge, human enrichment, and material progress." (Levine, 2002, p. 21, 23-4)

[40] Thanks to Fernando Vidal for suggesting this formulation.

[41] There was indeed cross-fertilization between Cajal's pastime and his work as a neuroanatomist: he replaced Golgi's famous staining method with a procedure he called the 'double impregnation' procedure, by using his knowledge of the action of developing reagents, and by transporting this knowledge to the realm of preparing brain tissue.

[42] Nothnagel , H. (1885) *Traité clinique du diagnostic des maladies de l'encéphale*. p. xxi.

[43] Classic sources on visualization include Daston and Galison, 1992, 2007; Elkins, 1999; Goodwin, 2000; Latour and Weibel, 2002; Lynch and Edgerton, 1988, 1990; Lynch and Woolgar, 1990; Lynch, 1990, 1991, 1998; Mitchell, 1992; Pauwels, 2006.

[44] Incidentally, for Cajal, Rudolf Virchow's studies on cellular pathology (Virchow, 1859|1971) were perhaps even more important than Schwann's work. Virchow's concept of the cell differed from Schwann's primarily with regard to his focus on the cell's responsive and functional qualities (Mazzolini, 1992).

[45] Soon after Golgi had announced the technique, Boll summarized Golgi's notes in the *Centralblatt für die medicinischen Wissenschaften* (Boll, 1873). In the following years, Golgi published at least three papers describing results obtained with the chrome silver method, [on the cerebellum (1874), the olfactory bulb (1875), and the spinal cord (1881)]. In an attempt to promote his work outside Italy, he also issued extended versions of his results in *Archives Italiennes de Biologie (*Golgi, 1883, 1886). The black reaction was also described in other brief reports.

[46] Cajal regularly used apochromatic Zeiss lenses with a numerical aperture (N.A.) of 1.30 (Cajal, 1891). A high N.A. indicates a high resolving power of the lens, which provides a more detailed and brighter view.

[47] provided that they are looked at with the appropriate glasses.

[48] The camera lucida was patented in 1806 by William Hyde Wollaston (Fiorentini, 2005).

[49] Around 1885, Cajal had written twelve "semi-philosophical" stories. They were published as the *Cuentos de vacaciones: Narraciones seudocientífica*s. Professor Laura Otis translated the stories into English. They were published in 2001 as *Vacation stories: five science fiction tales.* I thank Laura for pointing me to the fifth of Cajal's stories at the Educated Eye workshop (Max Planck Institute for the History of Science, February 2008).

[50] "todos los grandes observadores suelen ser habilísimos dibujantes. Los estudios morfológicos, la histología, la anatomía y la embriología serían incomprensibles sin el arte del diseño."

[51] "[A]quellos representan el factor objetivo, es decir, la naturaleza, y éste el subjetivo, ó sea el autor, cuya inteligencia, por fatalismos de organización cerebral, tiende constantemente á deformar y simplificar la realidad exterior. El buen dibujo como la buena preparación microscópica, son pedazos de realidad, documentos científicos que conservan indefinidamente su valor y cuya revisíon será siempre provechosa, cualesquiera que sean las interpretaciones á que hayan dado origen."

[52] Cajal believed there were two means to fulfil his cinematographic ambition. He first referred to a simplified device similar to "Marey's" old photographic revolver - although it was actually Janssen who invented the photographic revolver in 1874 (Coissac, 1927). With this device could project multiple photomicrographs that were recorded on a turning glass disc. I get the impression that the project remained at a conceptual stage, perhaps because adding a microscope to the technical set-up highly complicated matters. But Cajal had a second option: the Gaumont cinematograph, at once camera and projector. The results from experiments with this device were reasonably favourable, according to Cajal. He did suggest adapting the device to his particular requirements. In the conclusion, Cajal mentioned writing a follow-up article, pending the special cinematograph "now under construction at the Laboratorio

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de automática of the distinguished engineer senor Torres Quevedo." Cajal was convinced that the cinematographic method was "the only solution to the problem of photographing thick slices, projected through powerful lenses." (Cajal, 1917)

[53] Firstly, the standard film had a width of about two centimeters, but this width had to be at least doubled (perhaps determined by the diapositives he used in lectures). He also suggested that the length of the film could be reduced to two or three meters. Secondly, two devices needed to be installed: one to link the movement of the cinematograph's lever to the micrometric view of the microscope, and the other to permit the film to stop for several minutes in front of the photomicrographic camera. This was necessary for longer exposures to the light. In a footnote he wrote that " one succeeds very well when turning the cinematograph handle slowly, so that each image is exposed to the light for 1/8 or ¼ of a second. Naturally the projection of the positive film is done more rapidly, in order to obtain a smooth speed similar to the normal focusing. It goes without saying that when taking the negative an assistant slowly turns the micrometric view." (Cajal, 1918) It is beyond the purpose of this chapter to discuss the role of assistants in the work of Cajal. For a richly detailed set of historical studies on various engagements between scholars and craftsmen see the first section of Robberts, L. & Schaffer, S. & Dear, P. (eds.) (2007). *The mindful hand. Inquiry and invention from the late Renaissance to early industrialization*. Amsterdam: Koninklijke Akademie van Wetenschappen.

[54] (Leonardo Torres Quevedo (1852-1936) was inventor of electrodynamic and -mechanic devices. He founded the said laboratory in 1901, the same year that he entered the Academia des Sciencias in Madrid.) http://historico.oepm.es/museovirtual/gi_plantilla.asp?acc=1&idioma=es&xml=Torres%20Quevedo,%20Leonardo.xml, accessed 15-02-2007; http://canalsocial.net/GER/imprimir.asp?id=3170, accessed 15-02-2007.

[55] By comparison: before Cajal appropriated the Golgi method, he also published, but on a much smaller scale. Between 1880 and 1888 he issued 12 monographs.

[56] For instance, the existence of cells with markedly short axons in the outer cerebral layer of small mammals. Up to that point, it was believed that the outer layer only contained nerve fibres and neuroglial cells (nutritional and physical support cells). The same layer, Cajal noted, also contained cells with long horizontal dendrites, that run over enormous distances of the cortical surface. He coined the term cells of association. These cells were confirmed by Retzius, who designated them cells of Cajal. Cajal also demonstrated that the axons of medium and large pyramidal cells penetrate the white matter, where they sometimes branch off (DeFelipe and Jones, 1988, 5-6).

[57] There was not yet agreement about the number of layers in the cortex, nor on their nomenclature. Cajal decided to follow Meynert and Schwalbe's nomenclature of the human cortex, because to him the layers in the cortex of the small mammals displayed the same layers and elements (DeFelipe and Jones, 1988, 10).

[58] Ultimately, Cajal distinguished six cortical cell layers. The pyramids were located in Cajal's second, third and fifth layer. As one descends the phylogenetic scale, Cajal argued, the less differentiated, numerous and branched the cells are (Cajal, 1894a).

[59] The law was formulated independently by Cajal and the Belgian neuropathologist Arthur Van Gehuchten in 1891.

[60] His renewed attempts were not entirely unproblematic. He wrote to Cajal that he had treated delicate tissue of "les moelles d'embryos de poulet" in vain. He asked Cajal for specific instructions on the required thickness of the tissue ("de 3 a 4 mm de longueur?") (9 December 1890)

[61] Kölliker, *Handbuch der Gewebelehre des Menschen*, 6th ed., vol 2, part 2, 1896, p. 647.

[62] Cajal, *Neuron Theory or Reticular Theory?*, p. 99.

[63] This chapter deals with developments in anatomical imaging. This should be distinguished from brain mapping, which refers to efforts to label the brain in terms of its functions, and from functional imaging, a term used to highlight activities focused on the technological possibilities of brain scanning (Beaulieu, 2000)

[64] The atlas was published under the auspices of the Human Brain Project (HBP), an initiative of the U.S. National Institutes of Health. According to the information on the project's website, the goal of the HBP is "the development of neuroscience informatics: the creation and federation of web-based databases, analytical tools, and computational models to facilitate the open sharing and utilization of primary research data for all of neuroscience." The MRI Human White Matter Atlas fitted this profile for at least a three reasons. First of all, next to a paper edition, the atlas was also made freely available online. Secondly, Van Zijl's research group also published accompanying image processing software, which they also made

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freely available for downloading. I will discuss the software in the next section. Thirdly, the atlas can be seen as a first step in the creation of large-scale, population-averaged white matter atlases. In 2008, the same group of researchers published a so-called 'stereotaxic' white matter atlas (stereotaxic refers to the use of a three-dimensional frame of reference based on the Cartesian coordinate system). This atlas was based on information obtained from 81 'normal' subjects, and it was fused with a reference template: the widely used three-dimensional ICBM-152. Simply put, the ICBM-152 template combined the magnetic resonance imaging data of 152 subjects at three different laboratories, to form an averaged 'model' brain. It serves as a probabilistic reference system for researchers working under the heading of the consortium. At the time of writing this last chapter, the stereotaxic white matter atlas was only just published. It was therefore not possible to provide a careful consideration of these initiatives. I refer the reader to Anne Beaulieu's work for a detailed study on the installment of large-scale probabilistic brain atlases, in relation to issues of standardization and objectification.

[65] Derksen (2001) demonstrated the usefulness of using manuals to analyze the "microphysics" of research practices, in his case psychological test administration.

[66] The term 'hypermediacy' was coined by J.D. Bolter and R. Grusin in their book *Remediation. Understanding New Media* (MIT Press, 2000). It refers to a "style of visual representation whose goal is to remind the viewer of the medium" (p. 272).

[67] This frequency of resonance is called the Larmour frequency.

[68] Prasad coined the term 'image data' in 2005. It signals the status of MRI data as flowing between numerical and visual form (Prasad, 2005).

[69] e.g., Clark et al., 1999; Conturo et al., 1999; Virta et al., 1999; Werring et al., 1999; Basser et al., 2000; Barker, 2001; Coenen et al., 2001; Krings et al., 2001; Stieltjes et al., 2001; Mori and van Zijl, 2002)

[70] Mori received his Bachelor's and Master's degree at Tokyo University of Fisheries in the mid-1980s. He then moved to the United States, where he obtained his doctorate in Biophysics at Johns Hopkins in 1996. Today, Mori is professor of radiology at the Johns Hopkins University School of Medicine, and at the Kennedy Krieger Research Institute. He publishes widely on human and animal white matter, but also on mathematical and technological aspects of DTI. Recently, he also issued a textbook *Introduction to Diffusion Tensor Imaging* (Mori, 2007).

[71] Thanks to Anne Beaulieu for suggesting the comparison between DtiStudio and a photographic studio.

[72] "Casey (1996) has addressed the phenomenon of singularity or particularity in respect to certain prevalent (Euro-American, modernist) views about places. The particularity inherent in the idea of "a place" lends itself to naturalistic or scientistic descriptions that suppose it is carved out of an encompassing and generalized "space." (p. 90) Strathern, Marilyn. On Space and Depth. In: Law, John & Mol, Annemarie (eds.) (2002). *Complexities. Social Studies of Knowledge Practices.* Durham and London: Duke University Press, 88-115.

[73] Incidentally, the factor of experience was a recurring ingredient in the thesis. The collective empiricism Willis aimed at, depended on the enrolment and mutual calibration of experienced observers (see also Daston, 2008). Experience played a double role for Cajal. On the one hand, it referred to disciplining the eye, getting experienced in observing nature. On the other hand, it also referred to teaching the other how to see, in order to share the experience of observing the same thing. In Luys' case, systematic observation was also highly disciplined. Photographic seeing did not come naturally, but needed to be taught. Susan Sontag argued that prior to the invention of photography, seeing was not cut off from the other senses, but was polysensual. Photographic seeing introduced a kind of dissociative seeing. In addition, the human eye and the camera have different ways to focus and judge perspective. According to Sontag, the public was still very much aware of this distortion in the early days of photography. It was only after people started to become more experienced in thinking photographically that the deformation was no longer perceived (Sontag, 1977).

[74] See endnote 6.