Thinking with the Body: How Morphine, Alcohol, and Other Intoxicants Intersected Bodies and Minds in the Emergence of the Biological Subject

by

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Dalhousie University is located in Mi'kma'ki, the
ancestral and unceded territory of the Mi'kmaq.
We are all Treaty people.

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Abstract

Substances of intoxication can be found quietly skulking in every nook and cranny of our society, not only guests of honour in the peaks and troughs of our cultural imaginary, but constant companions in the everydayness of modern living.. Yet, little thought has been given to their formative role in the shaping of the modern body, of the biological subject. This is the untold story of how substances of intoxication interceded in shifting perceptions of "embodiment" found in the emerging sciences of the body and mind toward the end of the 19th century, giving rise to a dynamic conception of the biologistic subject within the scientific, philosophical, and sociological milieu of the late 19th century. Specifically, I posit that the history of the modern subject relied on a novel epistemic modality, which I have termed "intoxicated ways of knowing." With this, I identify the state of intoxication as a tacit form of *thinking and knowing with the body*. Intoxicants force us to feel, intervening directly in our perceptional awareness. Here, they bring conceptual associations concerning the nature of the body and mind into the foreground of our awareness, where these concepts are made real in our perceptions. Framing Brunonianism as a response to the demands for reformation in both medicine and pharmacy, I identify John Brown's own intoxicated experiences with the development of the "vital substance" concept, which quickly takes hold among late-18th and early-19th century German physicians and *Naturphilosophen.* As intoxicating substances were deeply intertwined with both Romantic science and vitalism, neo-mechanistic revolutionaries in physiology would make a concerted effort to undermine the validity of scientific encounters with intoxicants, both attacking Romantic Brunonians and invalidating the methods of perceptional science. By the time that the neo-mechanistic doctrine takes hold in neurophysiology, this emphasis on the physiological body as the singular locus of the real is realized as a forgetting of the lived experience of embodiment, as "brain-psychiatrists" strive, and fail, to equate mental states with neural states. This is what makes space for the parallelistic concept of the mind as the foundation of Wundtian experimental psychology, as a new science of embodiment. Here, Emil Kraepelin's extensive pharmapsychological research program overcomes parallelistic ambiguities by mobilizing the experimental methods and concepts of psychology to understand the structures of the mind as reflected in different states of intoxication, making the physicality of the body real in the perceptional world of the mind. Ultimately, it is the Kraepelinian connection between mental life and intoxication that is taken up in the works of Nietzsche, Weber, and Freud, bringing the biological subject out of the lab and into the world.

Chapter 1—Introduction

They each downed between 7.5 and 60 gr. of pure alcohol—the bitterness just barely masked by a smattering of raspberry syrup. Those who took the smaller amount were only lightly affected, perhaps a touch excited. Those who consumed the full 60 gr. of alcohol, meanwhile, were abuzz, softly nestled in a quiet dizziness. The subject at hand heard the letter *o,* followed by another, and yet another—recording each occurrence with a press of the hand. The besotted were participating in experiments on the influence of various intoxicants on different forms of reaction time.

Elsewhere, a tad short of 300 km (and 67 years) away, a young pharmaceutical chemist gathered three of the local teenage boys. They all agreed to consume half a grain of a new isolate, rendered by the junior chemist, in a weak alcoholic solution. When nothing happened, the group took another 0.5 grains, and yet another still, all in 45 minutes. Soon fading fast, and fearing that this hastily-devised experiment may very well prove fatal, the chemist quickly saw to it that everyone drank vinegar. This had been the first human encounter with a morphine isolate.

What both of these vignettes share is much more than the mere presence of intoxicants. Both exemplify what I have termed “intoxicated ways of knowing.” For, intoxication is not a state of intellectual vagrancy, as some would have it. Nor is it a dream state, out of which the creative unknown ruptures into conscious awareness. Everywhere around us, the presence of intoxicants is, in some cases, so everyday that they are hardly worth mentioning. And yet, their everydayness is sharply contrasted by their uncommon ability to force us to feel. It is in this emotive element of intoxication, this seizing upon perception, that intoxication’s epistemological significance lies, for the scientist and the layperson alike.

Whether in the laboratory or in one's average, everyday surroundings, the entities and scientific objects which populate the space around us are gathering points for concepts, theories, and associations. Entities, once they pop into existence that is, have a certain degree of durability which can not so easily be attributed to the mercurial flux of concepts, theories, and associations. Any of these elements just listed can move a human subject to think, see, and behave in a different way. But intoxicants, as entities, can engender conceptual associations and make them "real" in our perceptions; they intercede directly in our lived experience of embodiment.[1] Far from the totalizing influence of epistemes, intoxication is a dynamic, epistemic attunement of the senses—an epistemic modality. In this sense, intoxication is a form of tacit knowing, an inexplicable interface between perceptions, concepts, and scientific objects gathered in the radical particularity of place and time, and made explicit in the embodied lifeworld.

Expectedly, the most apparent examples of intoxicated knowing as central to knowledge-production figure into questions surrounding the nature of the body and mind. Of course, "the body" has a history, with the seeming facticity of being-a-body being a relatively young notion. This doctrine of biologism—and, with it, the biological subject—arose in a specific place and at a specific time. To be biological—contrary to perceptions of some bio-genetic reductionists—entailed, and still entails, so much more than mere self-identification with a clockwork of flesh and bone (Lewontin, 2003). If being biological refers to nothing more than a subject which reductively understands itself in bodily terms, then what is to be made of theories of mind, psychological treatment modalities, and genetic epistemologies? Biologism is a chimera—its many faces visible in the diverse ways that it is practised, across competing, even contradictory,

[1] "Embodiment," here, refers to the lived experience of being in a body, as opposed to the factical state of having a body.

sub-disciplines. How did this come to be, and how is biologism distinct from mid-19th century mechanism? It is at the centre of precisely this narrative—the historical emergence of the biological subject toward the end of the 19th century—that I place intoxicated ways of knowing as having primary significance. Here, substances of intoxication are called upon to testify, and compel testimony, on the ultimate nature of the body and mind. From the development of the Brunonian system out of opium therapy to the passage of alkaloids into being as objects of scientific inquiry, or the first ventures into pharmapsychology—intoxicated ways of knowing hold a crucial place in this story.

The starting point of this narrative is the appearance of what I have called "vital substances." These intoxicating vital stimulants, therapies grounded in the Brunonian system of medicine, would sweep across the German-speaking world, taking root in *Naturphilosophie* during an age when new and old intoxicants were stepping into daily life like never before. Inextricably linked to the experimentalist reorganization of the *materia medica* into the European pharmacopoeia, these vital substances were also central to the very principle of what it meant to be a body at the beginning of the German 19th century. For the Romantic Brunonians, the vital principle of "excitability" did not merely reflect, but directly flowed from the perceptional encounter of intoxicated embodiment. In Sertürner's identification of morphine as the first alkaloid too, the vital substance concept would participate directly in the determination of the alkaloid as a novel kind of thing to be. So entrenched would the notion of vitally stimulating intoxicants be that, even as he critiqued the Brunonians, Johannes Müller would experimentally validate the vital substance concept as a physiological fact.

This factors into the history of the biological subject as much through the tenacious response of its opponents, the neo-mechanists, as it does through its own claims. Through

Müller's own students, in particular Hermann von Helmholtz and Emil du Bois-Reymond, the "vital substance" concept would be stalwartly opposed, entangled as it was with vitalism and the Romantic science that Helmholtz and du Bois-Reymond so eagerly sought to supplant. Their opposition to the vital stimulus concept would be so virulent that, in effect, any introduction of intoxicants into the experimental negotiations surrounding the body would come to be met with scrutiny. The conceptual opposition to vitalism, and the vital substance concept, saw the elevation of the methodologies of physio-chemical reductionism to a doctrinal status. It would be this shift that led 'young' neo-mechanists, such as Eduard Hitzig, Paul Flechsig, and Theodor Meynert, to identify brain tissue as the locus of "the real" *à propos* the mind, equating even higher order mental states with neural states. As a result, the neo-mechanists would repudiate the validity of experiential empiricism. For psychiatrists like Meynert and Wernicke, this would mean leading a clinical life profoundly integrated with a rapidly growing armamentarium of newly isolated, and newly synthesized, intoxicants which, remarkably, never meaningfully figured into their conceptions of the body and mind. The neo-mechanist's rejection of the principles and methodologies underlying vitalistic and Romantic science would, thus, entail both a radical turn to the body, and a forgetting, or devaluation, of *embodiment*.

It is here that substances of intoxication intervene in the emergence of the biological subject. Hasty conflations between psychological concepts and neurophysiological features would help justify the formation of the separate, novel science of experimental psychology, a science of *embodiment*. At the core of this new science would be parallelistic critique of neo-mechanism, and bio-mechanical reductionism in all its forms. For Fechner, the principle of psychophysical parallelism had followed from his understanding of the duplicitous entanglement of the material and the psycho-spiritual that lay at the ground of the universe. But Wundt expelled the cosmic, spiritual element from psychological parallelism, instead emphasizing the parallelistic notion that psychological processes were irreducible to physical states. It is in this radically specific context that Emil Kraepelin would bring intoxicants into Wundt's psychological laboratory, in the hopes that experimentation on states of intoxication might be the key to finally understanding both the rudimentary structures of the mind and the nature of mental illness.

Through the intoxicated mind, Kraepelin's experiments would make the physicality of the body real in the psychical—experimentally grounding the psychological parallel in the body, while upholding the independent validity of psychology as a science. Kraepelin would counter the neo-mechanist's equation of mental states with neural states with his own identification of intoxicated states with mental states as such. In many cases, this would be accomplished through a process of self-experimentation not unfamiliar to Romantic experiential science, but now endowed with the validity, and critical distance, of an experimental science. This would be the emergence of a radical new conception of the body—biologism. Not of the body as a fleshy clockwork, but rather an approach to the body which was included an independent science of embodiment, and yet still aligned mental states with bodily events. This occasion did not go unnoticed in the developing philosophies of the late 19th century. On the contrary, looking to the burgeoning body philosophies of Friedrich Nietzsche, Sigmund Freud, and Max Weber, one finds that psychological research on the effects of intoxicants on the mind, as well as their own intoxicated lives, figured centrally into their conceptions of the bodily subject. In the end, it is worth pondering if the entirety of the biological subject hinges in some way on intoxicated ways of knowing.

This is a history of the biological subject, of the intoxicated subject, as well as an exploration of intoxicated ways of knowing, and the epistemological significance they hold for the biological sciences. To examine these elements in tandem means taking a look at both the subtle nature of changes in various conceptions of the body, while also taking seriously the idea of the body as a dynamic gathering point of perceptional phenomena. This entails a historicization of not only the body as a scientific object (and its relationship with the subject), but of the perceptional experience of intoxication, as a knowing relation given to fluid conceptual associations. Historical ontologies of various substances of intoxication, as they pass into being and immediately change the lives of those around them, will also be an important part of this story. By turning to the historical interactions between bodies and intoxicants, old ambiguities can be resolved, and new questions posed, particularly about where else the formative influence of intoxicated ways of knowing may have gone overlooked in the history of human thought.

Chapter 2—Vitalism, Scientific Medicine, and "Vital Substances"

Berlin 1748

In the year 1748, König Friedrich II graciously hosted a particularly scandalous Frenchmen in his Berlin court, in what Wilhelm Dilthey later characterized as a demonstration of Prussia's "boundless tolerance" (Dilthey, 1901/1927, 116).[2] This peculiar visitor was none other than the French physician and philosopher Julien Offrey de La Mettrie (1709-1751). That very year La Mettrie had anonymously published *L'homme machine,* extending the metaphorical equivalence between human body and machine body into an absolute description of the relationship between body and soul (La Mettrie, 1748). As Ian Hacking puts it, La Mettrie "conveniently forms one extreme edge of a framework of which Descartes forms another" (Hacking, 2009, 180). Hacking admits this is, of course, a grotesque simplification of a far subtler debate hinging on the 18th century conceptualization of substance, but Descrates and La Mettrie are useful signifiers for dualism on the one hand and strict materialism on the other (Hacking, 2009, 180).

Even by the standards of 18th century French mechanism, La Mettrie was a radical. This was perhaps even truer in the milieu of Prussian society. Prominent German physician, philosopher, and chemist Georg Ernst Stahl (1659-1734), an influential champion of a fundamental distinction between living and "dead" matter, had died in Berlin a mere 14 years earlier. And, while different degrees of naturalistic mechanism were widespread in Germany, devoted mechanism was something to be guarded against (Rumore, 2014). The repeated mention of La Mettrie's raucous hedonism, craziness, and buffoonery, including by would-be fellow

[2] […] "die Toleranz unbegrenzt sei."

French rationalists Voltaire and Diderot, speak to the depths of intellectual, and perhaps also personal, contempt held against him (Hacking, 2009, 182). Whether rumours of his debauchery were greatly exaggerated or true to life, these extravagant assaults on his personal character are reflections of the controversial nature of his published work. *L'homme machine* was sufficiently dangerous in the eyes of physiologist Albrecht von Haller (1708-1777) that he initiated a very public campaign against de La Mettrie's work, paralleling concerns raised in the broader community (Knabe, 1978, 121; Rumore, 2014).

But was La Mettrie's work truly as radical as his detractors claimed? After all, naturalistic and mechanistic attitudes had already taken root across Germany, and Friedrich II's court was replete with Francophiles, then a major source of materialist thinking (Rumore, 2014). La Mettrie argues from the perspective of a physician turned philosopher, someone primed to think gut-first. De La Mettrie makes no secret of this: "L'expérience et l'observation doivent donc seules nous guider ici; [e]lles se trouvent sans nombre dans les Fastes des médecins, qui ont été philosophes, et non dans les philosophes, qui n'ont pas été médecins" (La Mettrie, 1748, 25-26).[3] Rather than engage with metaphysical arguments against materialism, La Mettrie's argued for materialism from physiological evidence (Wellman, 1992, 195). Although there is something of a latent vitalism even in La Mettrie's materialism, it is clear that by "the soul" La Mettrie simply refers to the experience of animated consciousness and that he understands it be a product of the arrangement of parts of the brain. In making this case in *L'homme machine*, La Mettrie enlists a panoply of interlocutors, among them muscular irritability, brains, intestines, fingers, knives, and (most importantly for our purposes) intoxicants such as opium and spirits.

[3] […] "we should be guided by experience and observation alone [;] they abound in the annals of physicians who were philosophers, but not those philosophers who were not physicians."

Writing on the soul, La Mettrie remarks that “L’opium a trop de rapport avec le sommeil qu’il procure, pour ne pas le placer ici[;] Ce remède enivre, ainsi que le vin, le café, et chacun à sa manière, et suivant sa dose” (La Mettrie, 1748, 33-34). [4] Of its effects, “elle était en proie aux plus grandes douleurs; elle ne sent plus que le seul plaisir de ne plus suffrir et de jouir de la plus charmante tranquillité” (La Mettrie, 1748, 34).[5] Opium “change jusqu’à la volonté; il force l’âme qui voulait veiller et se divertir, d’aller se mettre au lit malgré elle” (La Mettrie, 1748, 34).[6] Coffee, as well as spirituous beverages, have effects on the soul, albeit different ones (La Mettrie, 1748, 70). Similarly, La Mettrie noted that food nourishes the soul, just as hunger weakens it: “tout dépend de la manière dont notre machine est montée[7]” (La Mettrie, 1748, 37).

The argument follows that if the soul is so easily affected by physical substances such as opium, alcohol, and coffee then the soul is likely physical itself, and that it is one with the body. A subtler point of interest is La Mettrie’s distinction between the nature of the effects of intoxicants such as opium and the nourishing effects of food and other sustenance. Just as disease and hunger weaken the soul by undermining the body, food replenishes the body’s resources. This sharply contrasts with the effects of intoxicants which, as La Mettrie himself attests, bring about sudden changes to the soul and even defy the protestations of the will. How La Mettrie understood this to be is unknown. But, whatever the case, he clearly understood intoxicants to have a unique purchase on the soul, distinct from the framework of nourishment/deprivation that is otherwise presented.

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[4] […] “Opium is too closely related to the sleep it produces, to be left out of consideration here. This drug intoxicates, like wine, coffee, etc., each in its own measure and according to the dose.
[5] […] “For the soul has been a prey to the most intense sorrow, but now feels only the joy of suffering past, and of sweetest peace.”
[6] […] “Opium even alters the will, forcing the soul which wished to wake and to enjoy life, to sleep in spite of itself.”
[7] […] “Everything depends on the way our machine is running.”

Even in the midst of La Mettrie's totalizing materialism, intoxicants are regarded as having a unique relationship with the body relative to both more banal consumables such as food and other medicines. For La Mettrie, this particular conception of intoxicants is marked by their soul-defying capacity and elsewhere by their vitalistic properties. This distinct status is irreducible to the pronounced pharmaceutical efficacy of intoxicants or the magnitude of their effects.

La Mettrie is not a particularly important figure here, though this tale of foreign medical ideas about intoxicants and the body taking root in German soil beautifully presages what is to come. Here, the ideas are not those of 18th century French materialism, but those of Scottish physician John Brown, whose wild notions about the deep significance that intoxicants like alcohol and opium had for the body found an eager audience in the crisis-stricken hearts of German physicians, in search of a scientific foundation. It is out of the morass of this very encounter that one finds the emergence of what I have provisionally called "vital substances," intoxicating substances which drive the vital forces at the heart of organic matter and ultimately participate in one's historical notion of what it means to be a body. Crucial to understanding the circumstances that gave rise to the fixation on vital substances is an awareness of the considerable developments in the study of plant and animal based therapeutic remedies over the preceding 17th and 18th centuries, expressed in the formation of national and regional pharmacopoeias.

Materia medica

The 17th and 18th centuries were periods of tremendous development in the theoretical and material conceptions of plant and animal based therapeutics, largely owing to a growing interest in experimentally validating the therapeutic value of existing remedies. There are any number of individual instances which help illustrate the experimental approach that was being extended to therapeutic remedies. Wren, Willis, and Boyle's attempt to deliver an opium tincture to a dog intravenously, for example, appears particularly pioneering in hindsight (Moon, 2021). But by far the greatest exemplar of the scope and influence of experimentalism in plant and animal therapeutics is in the sleepier history of the official pharmacopoeias.

Physicians of prior centuries had relied on the *materia medica*, both a collective name for the varied medieval and early modern compendiums of therapeutic remedies and a shorthand for the cumulative knowledge on medicinal substances. The term *materia medica* remained in use to refer to the body of knowledge on medicinal substances until the 20th century, before it was phased out by the term pharmacy (Lytle, 1906, 217). The use of that term, pharmacy, to refer to a collection of medicinal substances that have been studied and experimentally verified comes to prominence in the 18th century, with the proliferation of city pharmacopoeias. This change began with the sudden increase in the number of new city pharmacopoeias in the late 17th century and into the 18th century.

City pharmacopoeias were not without precedent. They sporadically cropped up in various cities throughout early modernity. The first pharmacopoeia was published in Florence in 1498/1499 (Lentacker, 2019, 225). This is followed more than half a century later with the publication of a pharmacopoeia in Nuremburg in 1546, after which a number of cities put together pharmacopoeias (Lentacker, 2019, 225). Florence published a new edition in 1550; followed by Mantua in 1559; Augsburg in 1564; and Cologne in 1565 (Crawford, 2019, 283; Urdang, 1946). The objectives behind the publication of these earlier pharmacopoeias varied greatly. The Cologne and Augsburg pharmacopoeias are at least partially a response to the perceived limitations of the Nuremburg text, and the opportunity for emoluments (Urdang, 1946, 46). Whereas the impetus for the issuance of the Mantua pharmacopoeia may have been nothing more than the rivalry between Duke Guglielmo Gonzaga of Mantua and Duke Medicean Cosimo I of Florence, whose pharmacopoeia had already reached its second edition (Urdang, 1946, 47).

The number of city pharmacopoeias in circulation began to increase in the 17th century with Venice in 1617 and 1667; London in 1618, 1621, 1632, 1639, and 1677; Amsterdam in 1636 (Marriott, 2010; Friedrich, n.d.). In 1698, the first German state pharmacopoeia was published in Brandenburg (Friedrich, n.d.). These earlier pharmacopoeias represented "official" prints of the *materia medica* and, though they were conceptually Gallenic, these earlier pharmacopoeias demonstrate an openness not only to iatrochemical remedies but novel plant remedies from the colonies of the New World, such as "ipecacuanha, guiac, and peruvian bark" (Maehle, 1999, 3). This demonstrates the successful integration of iatrochemical remedies into the Gallenic hegemony as well as the fluidity of the *materia medica*, however this is largely the extent of their innovativeness.[8]

This all began to change markedly in the 18th century. The number of city and state pharmacopoeias increases almost exponentially over the course of the 18th century and those cities which were already publishing pharmacopoeias produced new editions at a higher rate. Edinburgh produced its first pharmacopoeia in 1699 and printed new editions in 1722, 1735, 1744, 1756, 1774, 1783, and 1792 (Redwood, 1847). The Austrian Empire produced its first state pharmacopoeia in 1729, in addition to six new editions until 1770 (Redwood, 1847). Venice produced city pharmacopoeias in 1730, 1781, and 1790. Brandenburg's late 17th century state pharmacopoeia was updated with new editions in 1713, 1731, 1744, and 1781, now part of the

[8] Andreas Holder-Maehle's "Drug's On Trial", which I rely on for portions of this section, does an excellent job at unpacking how problem drugs came to be an object of experimentation.

Kingdom of Prussia. Württemberg produced its first pharmacopoeia in 1741, noteworthy for its inclusion of various details pertaining to the chemical properties of some of its ingredients (Friedrich, n.d., 6; Redwood, 1847). Later editions of the Württemberg pharmacopoeia were printed in 1750, 1754, 1760, 1771, 1786, and 1798 (Friedrich, n.d., 6; Redwood, 1847).

The frequent release of new editions and the development of new city pharmacopoeias speaks to the existence of a broadly held notion that the available literature on therapeutics was in need of revision, and that this process of revision was an ongoing process, with its initial foundations being laid in the 17th century. But what were the nature of these revisions? One of the starkest examples can be found in the fourth edition of the *Pharmacopoeia Londinensis,* published in 1721 with a new preface by President of the Royal College of Physicians, Sir Hans Sloane (1660-1753) (Sloane, 1721). Sloane explicitly advocated for the need to put an end to the use of superstitious therapeutics and compound remedies (Sloane, 1721, 38-39; Earles, 1961, 77). Gallenic ingredients such as "puppies" and "hedgehogs" were removed from compound remedies, while components containing ingredients such as human fat, skull material, and faeces remained (Sloane, 1721, 38; Earles, 1961, 75). The second edition of the *Pharmacopoeia Edinburgensis* followed suite in 1722, likewise retaining human materials as well as viper fat and a variety of insects as viable ingredients (Anonymous, 1722, 20). By the time a new edition of the *Pharmacopoeia Londinensis* was released in 1745, the extent of the changes made to the number of compounds and the lists of ingredients amounted to nothing less than a full-on reformation (Pemberton, 1746, 1). The 1745 edition still contained several of the trusted ingredients of medieval Gallenism, such as viper flesh and millipede; however, the preface itself contained expressions of doubt concerning the therapeutic value of these components, deferring to common practice in lieu of a better answer (Pemberton, 1746).

The crown jewel of 18th century pharmacopoeias, so to speak, may very well be Johann Christian Friedrich Scherf's (1750-1818) *Dispensatorium Lippiacum,* or Lippian Pharmacopoeia, of 1792, with the second part published two years later. Printed toward the end of the 18th century, it is naturally a culmination of the patterns developed over the course of the century, though it serves well to exemplify many of these themes. It contained notes on the water solubility of chemical ingredients and prescribed rules for the testing of remedies (Scherf, 1799; Redwood, 1847). Not only did it reflect the trends toward the simplification of Galenic remedies seen in other pharmacopoeias, Scherf's pharmacopoeia valued the role of the practical pharmacist as a participant in the experimental encounter with therapeutics. The second edition, published in 1799, was also the first pharmacopoeia to contain use of the German language (Scherf, 1799).

The relatively rapid proliferation across Europe of an interest in reforming the established therapeutic hegemony, expressed through the medium of the pharmacopoeia, speaks to the nature of the 18th century sea change in therapeutics. If it was the 17th century that saw the pharmacopoeia come to the fore as a possible site upon which medical authority could be gathered and constituted, the 18th century was the period in which the very ground of medical authority was experimentally rebuilt. This was overwhelmingly clear to figures of the period. Famed Scottish physician William Cullen (1710-1790) directly attributed changes in the content of the *materia medica* to advancements in chemical knowledge (Cullen, 1789, 24, 31). The distinguishing factor was the emergence of the experimental apparatus as the crucible through which therapeutic value was independently determined.[9] In this sense, Cullen's observation still

[9] The development of experimentalism in early modern Europe is a subject which has been widely discussed and explored. For our purposes, this discussion will briefly outline some of the changes in

rings true, though with an important shift in emphasis: improvements in chemistry may have affected the *materia medica,* but this is only because chemistry (outside of the iatro-chemical sense) had suddenly become relevant to their understanding of therapeutics.

What, if anything, does this have to do with the body? Any systematic approach to therapeutics entails a theory of the body as well as an associated theory of therapeutic action. In this sense, our conceptions of therapeutics are immanently participatory in how we conceive of our experience of embodiment. Unsurprisingly, then, the reorientation of the ground of therapeutic authority is reflected in the explosive effort to reimagine the nature of therapeutic action. Under the previous Galenic model, therapeutic efficacy was determined by a remedy's intrinsic characteristics and the physician's ability to marshal these qualities in their attempts to quell the humoral inequilibrium that was the cause of their patient's suffering. An excess of cold, damp phlegm, for example, called for a remedy that was warm and dry, such as coriander, cumin, or holy thistle (Bos, 1996, 229; Holmes, 2002). A remedy's curative potential was intrinsic to the real or perceived characteristics of the substances that made up the remedy. As many of these Galenic remedies were called into question with the rise of the experimental approach in therapeutics, there was, simultaneously, a rash of new theories of therapeutic action.

It is worth noting that although several new theories were proposed very few if any gained significant traction. For some, Sir Robert Boyle's (1627-1691) corpuscular theory provided a possible foundation for explaining the effects of remedies and poisons (Maehle, 1999, 4). One such figure was Johann Jakob Wepfer (1620-1695), who cited his study of the ulcers found in the vivisected gastrointestinal tracts of poisoned animals to advance the idea that the

respect to therapeutics, while leaving a summary of the broader developments in experimentalism to others.

cause of the damage was specially shaped, sharp particles found in poisonous substances (Maehle, 1999, 4). Meanwhile, investigations into a neo-iatrochemical theory of therapeutic and poisonous activity were advanced by Friedrich Hoffman (1660-1742) at the University of Halle (Maehle, 1999, 4). In 1687, Sir John Floyer (1649-1734) attempted to establish a theory associating a remedy's outward, sensory qualities with its effects, an idea that gained considerable traction (Floyer, 1687). None other than Carl Linnaeus (1707-1778) participated in an attempt to systematize a framework associating therapeutic action and sensory characteristic much later in 1751, a model which reflected the methodological rationale employed in his own classificatory system (Earles, 1961, 34). William Cullen came out in opposition to the value of this idea, primarily on the basis of the apparent subjectivity involved in ascertaining the characteristics of qualities such as taste and smell, and the resultant difficulty of classifying substances in this way (Cullen, 1789, 26). Cullen did, however, concede to the notion that only those remedies with a pronounced taste and smell were likely active (Cullen, 1789, 26). In spite of their differences, each of these models is paradigmatically aligned with the Galenic conception that therapeutic action is associated with the substance's intrinsic characteristics. Wepfer's theory of toxicity derived for Boyle's corpuscular philosophy was explicitly Galenic, relying as it did on Galenic language (Maehle, 1999, 4). Floyer, Cullen, and Linnaeus, meanwhile, were more theoretically conservative, but nonetheless Galenic in their approach.

The cornerstone of modern pharmacology is the idea that drugs are active principles, discrete, individual substances that engender bodily responses, and this perspective also first appears in the midst of these broad debates concerning therapeutic action. The active principle concept was likely first proposed by Alexander Monro Secundus (1733-1817) in 1771 via a study of how opium and other "powerful medicines" entered into the body of animals (Monro, 1771, 294). Monro endeavoured to do so by subjecting his frogs to a battery of tests, in the hopes of ascertaining by what means opium affected the body. Having dosed the frogs with an opium solution through the outer and inner layers of the skin, through muscular nerves, through various organs, and finally through a pipe directly into the heart, Monro identified two means by which opium affected the body (Monro, 1771). The first was through local absorption at the site of the nerve, after which, he theorized, the body responded *sympathetically* (Monro, 1771, 325). The second was through absorption into the blood stream which Monro argued carried opium to nerves surrounding blood vessels throughout the body and in the heart (Monro, 1771, 338).

Both suggestions rely on the idea of absorption via the nerves, a concept which Monro was not unique in supporting but which had been subjected to a great deal of contradictory experimental scrutiny (Monro, 1771, 337). Monro himself acknowledged the observation made by others that in cases where a high dose of opium had killed an animal subject the opium had usually stayed within the stomach and lost little of its weight (Monro, 1771, 339). These experimental findings appeared to deny the possibility of absorption being the cause of the overdose, as the opium remained in the stomach without a significant loss in mass. Monro was quick to dismiss the "erroneous supposition that the stomach is not provided with lacteal vessels" (Monro, 1771, 339). It was fully possible, Monro reasoned, for opium to be absorbed via vessels surrounding the stomach. As for the mass of the opium remaining in the stomach, Monro made a tremendous passing remark: "it seems very probable that its active part makes but a small share of its bulk" (Monro, 1771, 339). However brief the statement, there is little space to argue that Monro is suggesting anything other than the idea that opium consists of a combination of inert material and an *active principle*. This argument flies in the face of the entirety of Galenic medical knowledge and problematizes nearly all of the classificatory frameworks that formed the bases of the new pharmacopoeias. Particularly, it undermined the notion that a substance's medicinal effects corresponded with some outward or instrinsic characteristic of the substance itself, and therefore should be classified as such. Yet, Monro made little effort to push this point further. It is possible an awareness of the uncertainty of his claim, as well as its controversial nature, dissuaded Monro from leaving it as anything more than a comment. But the suggestion was made.

Thus, the latter half of the 18th century was a period of tremendous upheaval in the long standing classificatory and conceptual language of therapeutics. A formative force here, one which undoubtedly charted the course for therapeutics toward the end of the century, was the heretofore unseen growth in the availability of intoxicants. Colonial expansion had discovered, invented, and made accessible intoxicants in a radical new way, at the heart of which was the emergence of an economy surrounding the international production and sale of intoxicants for the purposes of mass consumption.

Spoils of Empire

The European colonial enterprise brought untold wealth flowing into Europe. There were the Spanish galleons, riding low in the water under the weight of all the gold they were schlepping from the New World. Hard- and softwood was in constant demand from the Royal Navy, towering softwoods in particular were required for new masts and North American white pine swiftly became a prized commodity (Knight, 1986, 221; Gwyn, 2001, 1). So valuable were these pines that by the mid-18th century the law forbid anyone from felling a white pine without a license, all the way from Nova Scotia to New York (Gwyn, 2001, 2). There were other useful metals and textile materials, all of which helped make enrich the European continent. But arguably none of these commodities were as profitable or socially formative as the colonial trades in Chinese tea, sugarcane for alcohol, and Bengali poppies for opium. These were the spoils of empire.

Sugarcane of course was useful for so much more than just alcohol. Sugar had begun in the medieval period as an object of inordinate luxury, a fine spice. This naturally created in interest in expanding sugar production and before 1550 three thousand sugar mills had been built in the Americas (Benitez-Rojo, 1996, 93). The value of sugar slowly declined as even more sugar plantations were built across South America and the Caribbean, an expansion that also saw the wholesale transition to stolen African slave labour to carry out the hot and gruelling work (Wood, 1996, 89). The sugar processing method of the time was inefficient, not only producing sugar but also 3 parts molasses for every four parts sugar (Ostrander, 1956, 82). Molasses was a costly waste by-product of sugar production. Thus, as demand for sugar drove ever further cultivation, the molasses surplus grew with it.

So much molasses was taken in by the American colonies that it found its way into all manner of colonial cooking. As early as 1685, molasses was the base of the majority of the beer brewed in the American colonies (Ostrander, 1956, 82). If Herman Melville is to be trusted, molasses even made its way into pints of gin dispensed at local inns and taverns well into the 19th century (Melville, 1892, 19). But one the most desirable uses for molasses was the brewing of rum. By 1750, Massachusetts alone supported 63 rum distilleries, producing approximately 700 000 gallons of rum per year (Ostrander, 1956, 83). This was followed closely by Rhode Island, reaching 500 000 gallons annually by the 1770's (Ostrander, 1956, 83). As the prevalence of molasses meant there were no shortage of smaller, local distilleries, much of this New England rum was prepared for export, where it went for half the price of West Indian rum on the market (Ostrander, 1956, 83). A significant portion of these exports were directed through the “triangle trade” to Africa, accounting for 1/8 of total annual production from 1768-1772 or about 300 000 gallons of rum (Ostrander, 1956, 83). This tremendous flushing of rum into the market was ultimately what saw the Royal Navy transition from brandy to rum, where tots of rum became part of a sailor’s daily ration (Tannahill, 1973, 273).

The opium trade was a similar exercise in imperial commerce, with perhaps even further reaching implications. Before Britain entered into the opium trade, it was already a prized commodity in the portfolios of Dutch and Portuguese traders, who shipped opium from India to the ports of Canton, Java, and Macau, among others (Bailey and Truong, 2001, 174). However, such shipments were naturally limited by the relatively minor scale of poppy cultivation, despite consistent demand for the product. The East India Company overcame this limitation, and ultimately gained dominance, by gaining control not only of the sale of opium but of its production, which they accomplished by seizing hold of poppy cultivation centres in Bengal (Bailey and Truong, 2001, 174). The year 1773 was the start of the Bengal Monopoly, and by 1797 the EIC outlawed the cultivation of opium poppy for those who did not hold their license (Bailey and Truong, 2001, 174). Chinese cultivation of opium did not begin until the 1860s and only became competitive with Indian opium by the 1870-1880s (Bailey and Truong, 2001, 174). From the last quarter of the eighteenth to the first half of the nineteenth century, the British had all but total control over the opium trade, and such dominance saw opium elevated to one of the top commodities in the empire.

The dizzying scale of opium imports into 19th century China are well known. Though of course opium was bound up in its own triangle trade structure (Derks, 2012, 50). The first tea was imported from China to England in 1668 (Derks, 2012, 51). The volume of tea imports grew almost exponentially in the 18th century, which of course led to a trade deficit as the Chinese market had no interest in European manufactures. At some point, some shrewd traders realized that Indian opium was something that could be sold to Chinese buyers, and thus began the trade of opium from India to China, of tea from China to England, and British manufactures to India (Derks, 2012, 51). The opium trade rapidly showed itself to be far more than a means of covering a trade deficit. The EIC's net profit from trade with China grew from ~234000 pounds in mid 1770 to just shy of ~1000000 by the year 1800 (Derks, 2012, 52). This still only reflected imports of around 200 tonnes of opium into China. By 1835, Chinese imports of opium swelled to ~1390 tonnes, 7x the 1800 number, and this would continue to grow almost exponentially across the 19th century, eventually covering over a third of Britain's visible global trade deficit (Deming, 2011, 11).

What becomes clear is that the period from the latter 17th to the early 19th centuries was a time strongly characterized by a shift in the social and conceptual relationship between substances of intoxication and the people of Western Europe (James and Withington, 2022). Cannabis, opium, tobacco, and spirits had come in from the "New World" since the earliest vestiges of a lasting colonial enterprise. In the 1670's, Robert Hooke sported about London while experimenting with laudanum, chocolate, tea, cannabis, and tobacco, nearly all of which would not have been available 60 years earlier (James and Withington, 2022, 2). But what had been a relative minor trade in exotic consumables had, by the second half of the 18th and into the beginning 19th centuries, become titanic, socially formative, industries. Intoxicants such as opium, spirits, and tea had risen to count among the most desired and profitable commodities on the globe, more or less freely passing between southern and eastern Asia, Europe, and the Americas where they spurred human subjects to action. From the genesis of England's tea obsession to the emergence of east-Asian opium culture, many of these influences were far from subtle. They meaningfully altered the average everydayness of common people around the globe. Nor were such influences merely social: the availability of intoxicants on mass scale profoundly changed the discussion surrounding medicinal therapeutics.

While pharmacy had rapidly developed into one of the most conceptually sophisticated and empirically rigorous aspects of medicine, the connection between physiology, therapeutics, and the practical art of medicine remained murky and fractured. Now, colonial trade only deepened these ambiguities, through both the introduction of new substances and the increased access to those already known. Efforts to systematize medicinal therapeutics, and make use of this newfound wealth of plant remedies, was rapidly making apparent medicine's desperate lack of empirical systematicity. Different therapeutics had been gradually passed through the experimental crucible. But what of their application? How did this affect theories of therapeutic action, bodily function, or etiology? Such questions amount to nothing less than an inquiry into the foundations of medical validity. For many German physicians and philosophers, the solution to this crisis in medicine came in the form of a Scottish physician named John Brown (1735-1788) (Tsouyopoulos, 1990, 107). Brown would reconceptualize the content of the *materia medica* in a new way which favoured intoxicants like opium and spiritous drink, the spoils of empire, over more established therapeutics, all in the service of systematicity.

John Brown's Body

Dr. John Brown was sick. A youth of "generous living" had been cut short by a bad episode of gout at the ripe old age of 36 (Brown, 1795, xvi). Much to his despair, Brown's efforts to secure lasting reprieve from his condition reaped little in terms of results. Medicine, he felt, was failing him. Finally, deliverance came neither by way of some new therapeutic nor through the efforts of a gifted physician, but in the form of opium. Brown discovered in "opium and some other stimuli, the secret of repelling the fits of the gout as often as they returned, and, at the same time, of reestablishing the healthy state, a secret that has hitherto been so much wanted and despaired of" (Brown, 1795, xxiii). Opium, spirits, and other "stimulants" seemed to have unique curative powers, then unrecognized. With the publication of *Elementa Medicinae* in 1780, John Brown made his new, totalizing system of medicine, derived from these extraordinary insights, available to the European public. Brown argued that the energy that animated organisms was created through the internal and external stimulation of excitability, the basic vital force (Bynum and Porter, 1988). Life was then "the product of constant reactions between stimuli consuming specific amounts of excitability," a unending balancing act between external inputs and bodily function to maintain a stable amount of excitability (Risse, 2003, 165). Any inequilibrium in this vital force, either an excess or shortage of excitability, would result in a pathological state on the physiological level (Tsouyopoulos, 1990, 107).

This is a remarkable example of intoxication as thinking with the body, about the body. It is understandably popular to conceive of intoxication as a filter. Some temporary lens, or film, through which sensations and stimuli are processed and then forgotten. But what if intoxication itself was understood as way of thinking? In Brown's recounting of events, the consumption of intoxicants becomes an avenue for a tacit awareness of embodiment. He is not merely reporting on the curative effects of opium and spirits: Brown is recognizing that the experience of intoxicating stimulation imparts information about what it means to be a body, that intoxication calls attention to the perceived nature of embodiment. It engenders a tacit awareness of embodiment which has the potential to reorient or reorganize existing frameworks, bringing latent conceptual associations into the foreground of perceptional experience. In this sense, intoxication is a way of knowing. In Brown's case, intoxication with opium and spirits revealed these substances to be powerful stimulants of the vital force, capable of righting any inequilibrium that may be the cause of disease. Intoxicating substances, you could say, became vital substances, all because Brown was thinking with the body.

In terms of the theoretical structure of his framework, Brown's system was quite unremarkable insofar as it mirrored other vitalistic medical theories circulating at the time. Brown's system drew heavily on the medical system of his teacher William Cullen, and so was more essentially derived from Albrecht von Haller's theory of irritability (Dyde, 2015). Cullen had followed Von Haller's work in defining all disease as the excess or lack of sensibility, the modulation of which was the objective of therapeutics (Rocca, 2007, 94). Brown's system undoubtedly derived the notion of a medical system which linked disease to vital inequilibrium from Cullen, although Brown's points of departure from his former mentor would prove to be the most determinative of his success.

Whereas Von Haller's and Cullen's medical systems were grounded in experimental physiology, Brown's system claims to derive its validity from Brown's own conversion story of having his body redeemed through the use opium and other stimulants. Brown appears to argue that the evidence for vital substances is implicit to the experience of intoxicated embodiment. This shift in focus opened up Brown's system to several possibilities which were unavailable to Cullen. As Brown understood it, a logical entailment of the reduction of aetiology to a discrepancy in the quantity of a given force meant that illness itself became quantifiable. This leap was possible precisely because of Brown's focus on stimulating therapeutics: it is difficult to quantify vital principles *in vivo* but far simpler to quantify external inputs. This allowed Brown's system to aspire to the Newtonian value of mathematical quantifiability while also appealing to therapeutics as the most robustly empirical aspect of practical medicine. Arguably, it is because of these factors that Brown's theory would enjoy a degree of popularity, if short lived, which vastly outstripped its competitors. His framework was especially impactful in two ways: (1) it wedded pathology to physiology and (2) it shifted the emphasis from qualitative observations to quantitative measurement (Tsouyopoulus, 1982, 14).

Brown's vitalistic theory of excitability also provided a simple explanation for the difference between animate and inanimate objects using a single principle, easily applied to explain physiology and pathology (Risse, 2003, 165). With its central emphasis on quantifiability, Brown's *Elementa Medicinae* (1780) "represented the culmination of all eighteenth-century efforts at medical system building" (Risse, 2003, 166). One is hard pressed to find remnants of excitability theory in the language of modern medicine, but it is in Brown's attempt to make excitability mathematically quantifiable and, in this way, help medicine become an exact science that we can see his lasting impact (Risse, 2003, 166). That excitability could not be concretely defined was not a problem: excitability was no more or less describable than Newtonian gravity. Although fundamentally simple, Brunonian theory becomes deeply intricate when used to explain specific phenomena (Tsouyopoulos, 1990, 108). Many of these difficulties can be traced to what is easily one of the most rhetorically attractive components of the Brunonian system, which was that excitability was a measurable force. Brunonianism seemed, in some ways, an ideal foundation for a Newtonian revolution in medicine. The obvious problem was the difficulty of measuring this vital force in the body.

Brown's solution was to shift focus away from the body itself and instead track those things entering the body, a perspective that would also determine the nature of Brown's therapeutic approach. Brown's system for grading levels of excitability consisted of a vertical chart with 80 degrees, with a baseline set at 40 degrees (Brown, 1795, 27). The baseline of 40 degrees represented the ideal state of health while deviation either above or below 40 degrees was a quantification of disease (Brown, 1795, 27). “Exciting power,” the energy needed for all the activities of life, caused an equivalent deduction in excitability which was then recouped through stimulus (Brown, 1795, 27). Given that existence itself puts constant strain on the vital force, excitability could only be liminally observed, being as it was in a state of unending flux. This dialectical opacity of Brunonian excitability meant that it could only be observed indirectly. The task of a Brunonian diagnostician was to rigorously track a patient’s activities before and after falling ill, as well as any possible stimuli. Once the relevant factors had been dutifully recorded, the level of excitability could be determined relative to a baseline of 40. Thus, all disease, regardless of their otherwise perceived causes, were reduced to a singular issue, that of an imbalance in excitability.

This made therapeutics extraordinarily simple: “every disease, that debilitating powers remove, is sthenic, every one, that is cured by stimulant means, asthenic” (Brown, 1795, 92). A patient who was “sthenic” (having excitability levels exceeding 40 degrees) merely required a swift and precise reduction in excitability. “Asthenic” patients (having less than 40 degrees of excitability) required stimulant therapy. Treatment for sthenics effectively amounted to deprivation of the body. Cooling, abstaining from meat, eating unseasoned foods, and bloodletting were all alleged to be curative in the sthenic. However, Brown himself considered such cases exceedingly uncommon, reporting an average of 3 sthenic patients for every 97 asthenic patients (Brown, 1795, 96). This meant that the vast majority of diseases could only ever be cured with stimulant therapy, calling upon the physician to marshal vital substances in curing the diseased.

Although a healthy level of excitability could be maintained by stable environment, appropriate exercise, and proper diet, once an individual becomes asthenic more drastic remedies would be required to save their life and, for John Brown, there were no stronger medicines than liquor and opium. “Opium,” Brown wrote, "is the most powerful of all the agents that support life, and that restore health, and a truly blessed remedy," which "cures any of the diseases depending upon debility" (Brown, 1795, 237). Strong spirits were less powerful than opium in their curative potential, but they were still regarded as essential stimulants (Brown, 1795, 104). Musk and powerful alkalis, examples of the enduring influence of both Galenic and iatrochemical remedies, were also listed as effective, although it is unclear how much stock Brown actually placed in their curative potential given the amount of attention he allocates to alcohol and opium (Brown, 1795, 104). Prescriptions of opium and spirituous beverages were so frequently reported that they came to be synonymous with Brunonian therapeutics. These recommendations on Brown’s part were absolutely influenced by his own heavy use of opium and spirits in treating his severe gout (Maehle, 1995, 52).

That Brown structured his aetiological model first in terms of how diseases were cured was not accidental. Nor was it incidental that Brown relied as heavily as he did on powerful intoxicants. John Brown was even proximal to many of the ongoing discussions concerning how remedies were to be classified and how they functioned, close as he was to William Cullen. But the Brunonian system is not merely a totalizing theory of medicine and the body guided by a scientific interest in therapeutic remedies; it is a claim on the *potentia vitalis* of intoxicating substances.

Brown conceived of any given body as the product of a dialectic between the vital force and the world. In this sense, Brown did not treat intoxicants as the singular source of vital stimuli, any more so than roast beef or black pepper. However, intoxicants are nevertheless accorded a special place. With opium heralded as the vital stimulus *par excellence* and spirits trailing closely behind, intoxicants are almost inseparable from Brown's conception of vitalism. It becomes tempting to suggest that in the Brunonian framework intoxicants themselves become something akin to a physical manifestation of the vital force. This conceptual association of intoxicants with the vital force is what I haphazardly term "vital substance." This is an idea, which, though vaguely present in the writings of earlier figures such as La Mettrie, is first fully realized here, in John Brown's medical system.

Brown died in a London debtor's prison 8 years after his magnum opus was first published, having initially failed to gain a following in Britain. However, the new system soon became extremely popular amongst the Germans and, by 1795, Brown's system was known throughout Germany, Austria and Switzerland, crossing borders and engendering discussion in every sphere of educated society.

Kant, the Foundations of German Science, and the Brunonian Promise

Amongst German physicians, physiologists, and natural philosophers, it was already a commonly held notion that medicine was in a state of crisis. As in other parts of Europe, there was a desire to see medicine realized as a science, reconciling medical practice with empiricism and physiology. However, the authority of Kant's definition of a proper science posed a significant barrier to any aspirations of a scientific medicine in Germany.

Of Robespierre (1758-1794) and Immanuel Kant (1724-1804), Heinrich Heine (1797-1856) remarked that the men "Im höchsten Grade jedoch zeigt sich in beiden der Typus des Spießbürgertums—die Natur hatte sie bestimmt, Kaffee und Zucker zu wiegen, aber das Schicksal wollte, dass sie andere Dinge abmögen, und legte dem Einen einen König und dem

Anderen einen Gott" (Heine, 1834, 86).[10] There is little space to doubt who Heine saw as having tussled with a king, and who with a god.

Immanual Kant's critique of rational psychology had already proven to be immensely influential in nearly every corner of the German world. In the wake of Prussia's defeat at the hands of Napoleon, Kant's political thought weighed heavily on the minds of Reformers Freiherr Karl vom Stein and Karl August von Hardenberg, weaving Kant's legacy into the very fabric of the social and constitutional order (Levinger, 1998, 243). For those concerned with philosophy, Kant's critical works would become defining, foundational texts, against whose arguments new thinkers would inevitably be weighed. So too was it for those studying the natural world, where Kant's attempts to unite investigations of the outer and inner worlds on a shared ground of apodictic certainty directly affected scientific practice (Hacking, 2002, 3, 24; Aldea & Allen, 2016, 1-2).

Kant understood his own work as an extension of the broader Baconian project. There are defined boundaries fencing off those things which are certainly knowable from those which are ultimately unknowable through the powers of perception, and reason. The work of philosophy and science is to define those limits and function within them. Keeping with this basic premise, Kant would develop his own comprehensive, systematic account of the categories necessary for the existence of consciousness, which would form the foundation of his epistemology of science. The cornerstone of this foundation is Kant's *Kritik der reinen Vernunft* (1781), which primarily

[10] […] "they represented in the highest the type of provincial bourgeois; nature had destined them to weigh coffee and sugar, but fate determined that they should weigh other things and placed on the scales of the one a king, on the scales of the other a god."

functions as a refutation of Descartes and his disciples. Kant reconfigures the question concerning the Cartesian relationship between the mind and body into an analysis of the extent to which representations can be equated with necessary truths.

The importance of this issue for any systematic study of the natural world is so apparent that it is hardly worth stating. How are we to be sure of anything we know about the physical world if we cannot differentiate between representations and necessary truths at the mind-body level? Kant is famously sparse in his direct discussion of the nature of the relationship between the mind and the body. This is to the credit of his critical approach, which assumes the notion that human beings are unable to speak meaningfully about what goes on beyond the realm of human experience. Turning against his rationalist forbearers and contemporaries, he recognized that "the real goal of rational psychology is to settle the question of the possibility of the communion (Gemeinschaft) of the soul with the organized body" and that this doctrine posed very real epistemic problems (Powell, 1988, 404). This is the position against which Kant poised his ultimate critique of Cartesian rational psychology.

From the outset, the crux of Kant's critical engagement with Descartes rests on his assessment of the viability of Descartes' argument from doubt. At its most basic, Descartes' argument suggests that, while the mind can conceive of not having a body, the mind cannot doubt its own reality as that which is thinking (Descartes, 1993, 14). It is on these grounds that he proposes that there is an essential difference between the mind and the body. This deceptively simple argument draws its strength from a position of radical doubt, challenging the reader to set aside all preconceived assertions in the search for the truth. Truth can only be discerned by parting with any and all *a priori* assumptions that might shape, and thereby predetermine, the outcome of a course of reasoning (Descartes, 1993, 13). In this way, lasting truths could be developed from the ground up, providing the basis for more complex scientific questioning. Although he presents a forceful, even revolutionary, idea, Kant is quick to recognize that Descartes' purported argument from doubt relies fundamentally on a confusion of apparent representations with necessary truths.

It is apparent to Kant that the notion of thought divorced from external stimulus is impossible, as far as can be proven. For Descartes' argument from doubt to hold water, Descartes must be able to demonstrate that the relationship between thinking and sensing is subject to doubt. Kant undermined this assertion by establishing a connection between thinking and sensing as a foundational principle. Critiquing the tendency to isolate the mind from the body in his "Refutation of Idealism," Kant argues that "the very temporality of mental life becomes inexplicable if mental contents have no abiding background to manifest the temporal unity of their succession" (Winfield, 2011, 228). All mental contents are necessarily experienced in a temporal succession. Thought void of external context, if such a thing were conceivable, they would be insufficient to provide the frame of reference necessary to phenomenologically temporalize a series of ideas. Rather, "only the appearance of something *nonmental* can provide a persisting backdrop sufficient to connect past, present, and future" (Winfield, 2011, 228). This argument alone is sufficient to affirm that many of Descartes' central arguments require further investigation, but it fails to introduce any major claims that are not included in Descartes' meditations. The primary thrust of Kant's critique of Cartesian dualism focuses on what Kant perceives as a conflation of differences in representation with differences in essential composition.

Kant readily concedes that "Ich unterscheide meine eigene Existenz, als eines denkenden Wesens, von anderen Dingen ausser mir (wozu auch mein Körper gehört)" (Kant, 1781, B-

409).[11] One experiences themselves as a thinking subject and identifies first and foremost with that subjectivity, over and against the external world and to a certain extent their own body. The problem arises when this perception is treated as a necessary truth rather than a representation. The mind operates in such a way that it appears to be distinct from the physical world. The difficulty in addressing this line of questioning forms the bedrock of Kant's entire methodological approach. Only that which can be experienced can be spoken about meaningfully. Thus, speculative theories addressing "der vorher bestimmten Harmonie [oder] der übernatürlichen Assistenz" that makes the mind-body relationship possible are set aside, as they cannot be confirmed through experience (Kant, 1781, A-390).[12] Since the self cannot take itself as an object of intellection, the only meaningful way to discuss the relationship between the mind and the body is in terms of their respective forms of representation (Kant, 1781, A-386). Kant argues that before we can begin to entertain the Cartesian argument for a distinction between mind and body we must understand how we experience the mind and body as representations.

> Die Materie, deren Gemeinschaft mit der Seele so grosses Bedenken erregt, ist nichts anderes als seine blosse Form, oder eine gewisse Vorstellungsart eines unbekannten Gegenstandes, durch die-jenige Anschauung, welche man den äusseren Sinn nennt (Kant, A-385).[13]

As Kant suggests, every interaction we have with matter, including our own bodies, is strictly through complex representations. The body takes on qualities which render it distinct from the

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[11] "I differentiate my own existence, as a thinking being, from other things outside of me (to which my body also applies)".

[12] "The pre-established harmony or the supernatural assistance".

[13] "Matter, whose connection with the soul rouses such great concern, is nothing more than its mere form, or a certain style of representation of an unknown object, through intuition that one calls outer sense."

mind because the body can only be represented by way of the outer senses. As a result, the subject finds the mind and the body represented so differently that there is an apparent lack of coherence in understanding their relationship. Yet, insofar as both mind and body remain merely representations, they cannot be thought of as distinct entities with any certainty. In this way, Kant is able to dismiss the mind-body "problem" as scarcely more than a poorly posed question, acknowledging the appearance of a distinction between mind and body while affirming that the appearance of a distinction is not evidence of true heterogeneity.

Kant's model has been referred to as empirical psychology, in contrast to Descarte's rational psychology (Sturm, 2009, 183-185). Yet, this transition is to be understood as far more than a passage from rationalism to empiricism. In Kant, the mind-body problem undergoes a radical transformation, one which is not a reduction of mind to body. Rather, Kant keeps both the mind and body at a distance by retreating into the factic character or the psychological self. The result is the assumption of an agnostic position which renders the mind- body problem irrelevant by treating both sides of the conflict within the bounds of cognitive representation.

Kant, Scientific Epistemology, and Vitalism

It is precisely this tension that forms the foundation of Kant's scientific epistemology. Kant invokes Francis Bacon no later than the opening preface to the second edition of the *Kritik der reinen Vernunft*, immediately leading readers to identify his project of critical philosophy with Baconian epistemology (Kant, 1787, B-xii). The *Kritik der reinen Vernunft* is thus intended to represent an experimental analysis of pure reason (Kant, 1787, B-xviii). If there were any questions about whether this were merely a rhetorical flourish on Kant's part, Kant writes in a letter to Ludwig Borowski, dated March, 1790, that to follow "den einzigen Weg der Naturforschung" is to engage in "Experiment und Beobachtung, die die Eigenschaften des

Objects äusseren Sinnen kenntlich werden lassen" (Kant, 1790, Brief 411).[14] For Kant, the experimental apparatus is a privileged site wherein the unseen becomes sensible and thus knowable. As the study of the natural world cannot rely on synthetic *a priori* judgements to establish any form of certainty, experiments make knowledge possible by making phenomena available to the senses and understanding.

This does not mean that Kant is unreflectively Baconian. Alberto Vanzo and Larry Laudan have attempted to define Kant's epistemology of science relative to the anti-hypothetical doctrine of Bacon-Hooke-Boyle. Laudan makes a strong case for the argument that Kant upholds the anti-hypothetical position of Bacon-Hooke-Boyle (Laudan, 1981, 10). Kant does, after all, invoke Bacon directly and asserts that experiment is the singular means by which we can develop our knowledge of the natural world, by making that knowledge available to the external senses. It would appear that Kant should seem skeptical of the speculative foundations of hypothesis. The issue with this reading is that Kant's empirical psychology establishes that a robust cognitive and conceptual framework lays at the foundation of all perceptions. Hypotheses then are not merely speculative. From the outset, a hypothesis's validity is at least partially established by its theoretical conformity with our *a priori* understanding of the world, which in turn guide an experiment's design. It would seem that, for Kant, experiments themselves are extensions of hypotheses, a position which Kant later assumes for himself in *Weiner Logik.* Vanzo, meanwhile, is critical of the position taken by Laudan's argument, seeking to present Kant's understanding of the relationship between hypothesis and experiment as a subtle interplay in line with his critical philosophy (Vanzo, 2012, 76-77). As Vanzo points out, Kant provides no

[14] "The only path for scientific research is to engage in experimentation and observation, which makes the properties of the object knowable to the external senses."

definition of an experiment nor does he provide any sophisticated analysis of how experiments function, beyond a common perception that experiments *intervene* in nature (Vanzo, 2012, 76-77). Whether this is a result of Kant's own naïveté or a lack of direct experience, it is clear that Kant is primarily interested in situating experimentation within a broader system of knowledge.

Kant does, quite famously, provide a definition of what he deems to be a proper science in *Metaphysische Anfangsgründe der Naturwissenschaft* (1786). At its most basic, Kant's "actual science" (eigentliche Wissenschaft) is systematic, is characterized by the relationships of grounds and consequences, is apodictically certain, and allows for the use of mathematics (Kant, 1787, IV-V). Experimentation then is conceivably scientific when its underlying premises are rooted in a body of cognition which conforms with Kant's four stringencies. However banal this definition first appears, fraught as it is with altogether modern concerns with calculability and systematicity, this definition would have a tremendous impact on the course of German science. Kant himself recognizes that this meant subjects such as chemistry would not be considered sciences at all; chemistry might instead be called a "systematic art" (systematische Kunst), as its laws were only empirically substantitated. By the same measure, Kant further excludes the classification of natural kinds, natural history, and psychology (Kant, 1787, VIII). Medicine is not even worth a mention. For those fields from whom the aegis of scientificity was suddenly retracted, Kant's definition of an "eigentliche Wissenschaft" was a call to action.

Vitalism in medicine: German Science at the beginning of the 19th century

With most of Kant's most influential works being published from 1780-1790, Kant's definition of an *eigentliche Wissenschaft* came on to the scene in the midst of a transformative moment in the German world. Though in many respects Germany was late to the Enlightenment, the frenetic Newtonianism that had already swept France and England had firmly taken root in

Germany by the mid-18th century, even reaching the less educated masses (Rogers, 2003). By the time of Kant's *Metaphysische Anfangsgründe der Naturwissenschaft,* there was a widespread, popular sentiment that the world could be understood through properly scientific inquiry. Hence, the impact of Kant's definition of science was not merely theoretical. It struck a chord with those from all walks of life, at a time when scientificity was becoming one of the highest aspirations. The impact Kant's scientific epistemology was such that his criteria merited at the very least sincere response. This was particularly true of fields of study for whom Kant's criteria for scientificity called into question their basic validity. Of the latter group, there is no example more prominent than medicine.

During the eighteenth century, the successes of figures like Newton and Boyle breathed life into the notion that the truth of the matter, the secrets of eternity, lay in pursuit of an exact science. The world, accurately measured and quantified, was governed by unbending laws, the names of which could be learned through an empirical study of the universe. In medicine, Enlightenment evaluations of human reason's capacity to apprehend the laws of nature only underscored the imprecision of medical theory and therapeutics (Risse, 2003, 165). The 18th century Italian physician, Felice Fontana, once remarked that one would "be astonished to find that in the eighteenth century there have been Philosophers, Naturalists and *Physicians* who, even in the most important matters have ventured to substitute conjecture for experience" (Earles, 1961, 74). Kant's definition of an actual science turned an apparent shortcoming into a definitional failure. In theory, the underlying functions of the body would be discerned and the guesswork at play in the budding life sciences could be put to rest, as long as the system's components and their relations were appropriately measured and understood. The most daunting obstacle was the disconnect between the craft of practical therapeutics and physiology, whose own scientificity was up for debate.The difficulty was bringing medicine into a state of logical systematicity, with the caveat that systematicity was only properly scientific if it was grounded by apodictic certainty (Van den Berg, 2009, 7). This definitional stringency both undermined the legitimacy of recently established sciences such as chemistry and posed further difficulties for those who desired to see medicine realized as a science.

German Brunonianism

Nevertheless, a Bavarian physician by the name of Andreas Röschlaub (1768-1835) presented a solution. Promotion of his approach and the scientific footing it promised would briefly place Röschlaub at the centre of the German medical world, attracting a following that counted some of Germany's most famous minds.[15] The answer, Röschlaub suggested, was the medical system of John Brown. Critically, Röschlaub argued that Brunonian excitability was an object of apodictic certainty, an *a priori* principle, and thus could provide a foundation for a properly Kantian medical science (Tsouyopoulus, 1988, 65). Further still, Brown's emphasis on the quantifiability of both vital force and stimuli arguably fulfilled Kant's stipulation that a proper science make use of mathematics. However unconvincing this argument might appear to modern readers, it would appeal to many of Röschlaub's contemporaries.

Röschlaub first encountered the works of John Brown as a medical student, and while Röschlaub was ostensibly a Brunonian he gradually reinterpreted many of Brown's ideas into his own framework with the assistance of Adalbert Marcus, director of the Bamberg Hospital. In 1798, Röschlaub published *Untersuchungen über die Pathogenie oder Einleitung in die*

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[15] Nelly Tsouyopoulos has made an extremely convincing case for the centrality of Andreas Röschlaub in German Romantic Medicine. I will rely on her work here.

medicinische Theorie, his “big book” and the central argument for what he dubbed *Erregbarkeitstheorie* (Excitability theory) (Röschlaub, 1798; Tsouyopoulus, 1988, 65). Nelly Tsouyopoulus argues that Röschlaub was able to engender interest in Brown’s ideas by reframing the dialectical back and forth of excitability and stimuli as a true dialectical synthesis (Tsouyopoulos, 1988, 67). This appears to be in contrast with Brown, whose work described life as a forced state in the sense that living beings are perpetually in need of external stimuli to maintain life. In its initial articulation by Brown, this idea did not achieve broad popularity. Writing on Brunonian theory in *Von der Weltseele,* Friedrich W. Schelling (1775-1854) discounted Brown’s system on the basis of precisely this idea, which Schelling understood as relegating living creatures to a strictly passive relationship with the outside world (Schelling, 1798, 506; Tsouyopoulos, 1988, 67). Röschlaub repackaged this relationship as one in which external stimuli is still required to maintain life, with excitability remaining the basic energy that makes potential action possible; however, the effect of external stimuli is mediated by the existing excitability (Röschlaub, 1798, 244). In this way, an organism is neither a sponge, passively absorbing the output of the external world, nor an atomized self-contained entity, but rather a dynamic synthesis of an ongoing dialectic between self and the world.

The new and improved *Erregbarkeitstheorie* evidently left a significant impact on Schelling, as, though he denounced Brunonianism in *Von der Weltseele,* Schelling spoke highly of the Brunonian approach in *Erster Entwurf eines Systems der Naturphilosophie* (Schelling, 1799). That same year, Schelling contributed a defense of the *Erregbarkeitstheorie* to the first edition of Röschlaub’s 1799 journal (*Magazin zur Vervollkommnung der theoretischen und praktischen Heilkunde* (Schelling, 1799b, 255). There was even something of a brief friendship between the two of them, with Schelling making the effort to visit Röschlaub in Bamberg.

However, Schelling's efforts to further adapt the *Erregbarkeitstheorie* to *Naturphilosophie* quickly materialized into irreconcilable differences in how Schelling and Röschlaub understood the Brunonian system, a situation that led to a total split by 1805 (Tsouyopoulos, 1988, 65). The act that consummated this split was Schelling and Adalbert Marcus's decision to establish their own journal in order to distance themselves from Röschlaub, which they titled *Die Jahrbücher der Medizin als Wissenschaft* (Tsouyopoulos, 1988, 65). This severance of the movement swiftly led to the formation of two distinct camps, with Schelling leading the physiologists (such as Lorenz Oken, Franz von Walther, Ignaz Döllinger) and Röschlaub heading the pathologists (Schönlein, Grossi, Ringseis) (Tsouyopoulos, 1988, 65).

Evidently, Röschlaub's facelift on Brown's medical system was extremely effective at transplanting the model into a new context, so much so that it was naturalized in the form of its own "heresy". Which is to say, Schelling's split with Röschlaub over doctrinal minutiae, and the separation of their followers into different camps, more than anything demonstrated the depth of the German acceptance of Brunonianism's foundational concepts, in this period. How different really was Röschlaub's *Erregbarkeitstheorie* from its source material? Röschlaub altered the form in which the theoretical components of Brown's system were understood and thus fundamentally altered the mechanisms by which excitability was modulated. However, in regards to the application of Brunonian theory Röschlaub staunchly upheld Brown's therapeutic methodology. Röschlaub, in particular, reaffirmed the Brunonian emphasis on the use of stimulant therapy in curing diseases. In this sense, Röschlaub's interpretation preserved the vital substance concept developed by Brown's system, arguably even elevating this notion by guarding it through the crucible of translation and modification. Although the partitioning of the movement threatened the legitimacy of Brunonianism, it would receive sporadic support over the next five or so years, culminating in a revival that reached into the 1820s. Some examples of this are quite noteworthy.

Brunonian ideas further formed the basis of Baltic German physician C. von Brühl-Cramer's (???-1821) neurological theory of alcohol addiction, using a model of physiological dependence derived from Brown's system. Published in 1819, "Ueber die Trunksucht und eine rationelle Heilmethode derselben" arguably represented the first systematic study of alcohol addiction in a clinical setting. In addition to being one of the earliest scientific commentaries on fetal alcohol syndrome, von Brühl-Cramer's work outlined alcoholism's pathogenesis, aetiology, and different sequelae, such as dementia (Kielhorn, 1996). Notably, the introduction was written by celebrity doctor Cristoph Hufeland (1762-1836), who, though one of Brunonianism's earliest critics, "himself wrote several articles about John Brown, in 1819, 1822, and 1829, and compared Brown with Galen" (Tsouyopoulos, 1988, 66).

Eduard Loebenstein-Lobel, of the University of Jena, relied on Röschlaub-Brown in the creation of what is likely the first formal medical manual on wine therapy in 1816 (Paul, 2001, 28). Loebenstein-Lobel came out against his fellow-Brunonians in elevating wine above opium as the pinnacle of all vivifying therapeutics, going as far as identifying different wines for the treatment of different illnesses (Paul, 2001, 30). This is an excellent exemplification of the relative diversity of Brunonian thinking from the moment it gained popularity in the German world.

The scope of the Brunonian influence on German Romantic philosophy and science was so great that it even took root among the more German-facing of the English Romantics. Remarkably, though Brown was of course Scottish, English interest in Brown was almost exclusively through Schelling and Röschlaub. Among the most famous examples of this conceptual reverse migration was the Brunonianism of England's most famous opium-eaters: Coleridge and De Quincey, both of whom engaged with Brown's ideas through German publications.

For Coleridge, his initial exposure to Brown came through his good friend Thomas Beddoes, a physician and Germanist who had been the first to publish translated passages of Kant's *Kritik der reinen Vernunft* and *Kritik der Urteilskraft* into English (Vickers, 1997, 47, 59). Beddoes had become aware of the German Romantic Brunonianism of Röschlaub-Schelling in the 1790s, and it was through him that Coleridge ultimately became a disciple of Schellingian medical *Naturphilosophie* (Vickers, 1997, 59). By 1819, Coleridge counted Brown alongside the likes of Luther, Milton, Cicero, and Wordsworth as history's great geniuses (Vickers, 1997, 48).

A similar pattern is seen in the case of Thomas De Quincey. His *Confessions of an English Opium-Eater* awoke the unaware to the phenomena of English opium-eating in the 1820s (Gao, 2020, 6). There, De Quincey used Brunonian language to describe the effects of opium intoxication, musing about opium's excitatory capacity as a bodily stimulant (Gao, 2020, 9). Not only was De Quincey a Germanist himself, he met Coleridge in 1807, at which time they struck up a friendship and it was likely through him that De Quincey became a staunch Brunonian (Morrison, 1997, 27-28). Coleridge, like De Quincey, was enthusiastically taken with opium-eating, a personal practice that could be supported by the Brunonian system. Both parties certainly understood Brown's philosophy as the secret to not only personal, professional, and national health, but excellence (Morrison, 1997; Cooke, 1974).

The Brunonian system even figured into Humphry Davy's (1778-1829) discovery of nitrous oxide. Davy had been a pupil and collaborator of Beddoes (Golinski, 2011, 17). Beddoes, who had been a direct participant in the discovery of nitrous oxide and even invited Coleridge over to experience its effects, proposed that the Brunonian system as an interpretative framework through which to understand the new discovery, suggesting nitrous oxide was a vital stimulant (Golinksi, 2011, 19). It is clear that Davy himself ascribed to the Brunonianism of his mentor, albeit with less enthusiasm, and went on to describe nitrous oxide as a stimulant affecting the balance of excitability (Bergman, 1991, 535-536, 538). Notably, Davy himself described the effects of nitrous oxide by comparing it with those of opium, perhaps *the* vital stimulant (Golinski, 2011, 19).

German physiologists and advocates for scientific medicine, as well as their followers elsewhere in Europe, took to Brunonian ideas, using them to classify and categorize novel substances, techniques, and illnesses. In each and every case, the juncture of their encounters with Brunonian ideas was mediated by intoxication. This was not only because Brown's notion of excitability flowed directly from the experience of intoxication. From Davy to Brühl-Cramer, Brown's initial development of the "excitability" concept out of opium intoxication was repeatedly reconsummated with the establishment of new therapeutic intoxicants, and methods. The state of intoxication served as way of knowing that formed the basis for meaningful perceptional understanding of health and what it means to be in a body. As they were taken up by different physicians, the finer points of Brown's system even shifted in light of individual experiences with intoxication. Loebenstein-Lobel's elevation of wine over opium was, in this way, profoundly Brunonian, because Loebenstein-Lobel relied on his ownmost experience of wine intoxication as empirical verification of its superior therapeutic value.

Regarding the more general openness to Brown's ideas, Tsouyopoulus argues that the sudden receptiveness to Brunonianism amongst German thinkers after 1795 was not coincidental, suggesting that the phenomena can be traced back to the publication of Johann G.

Fichte's (1762-1814) *Wissenschaftslehre* (Tsouyopoulos, 1988, 70). The issue at stake here is how excitability could convincingly be presented as an *a priori* principle if *Lebenskraft* could not (Tsouyopoulos, 1988, 67). Published in 1794, *Wissenschaftslehre* dominated the academic discussion of the following year, with the poet Novalis (1772-1801) being the first to propose that there were significant conceptual similarities between Brown's and Fichte's systems (Tsouyopoulus, 1988, 70). Though compelling, it would seem the inverse is the case. Taking into account Schelling's and Hufeland's initial perceptions of Brunonianism as a mechanical philosophy, it would seem that Röschalub's success might largely be a product of his efforts to bring Brunonian excitability into agreement with the *Wissenschaftslehre.* This is evidenced by the inclusion of a public appeal to "Dr. Johann Gottlieb Fichte" to give consideration to, and ultimately pass judgement on, *Erregbarkeitstheorie* in the introduction to the second volume of the first edition of Röschlaub's *Magazin* (Röschlaub, 1799, xi).

Even if this is taken to be nothing more than a publicity stunt, it demonstrates Röschlaub's intention to bring intoxicated excitability, and the Brunonian model in general, into alignment with Fichte's authority. Röschlaub brought the Brunonian system into agreement with what he would eventually come to understand as Fichte's anti-formalist, dynamist ontology, focused as it was on relationality rather than substance or categories (Grant, 2008, 103). Additionally, the Brunonian system operated on the notion that excitability was a quantifiable force, at least *in principle*, and in this way it was able to formally satisfy the Kantian definition where simple *Lebenskraft* would not suffice. Once again, this is owing to the binding and enmeshing of excitability to vital therapeutics, without which there was no practical means of quantifying excitability. Thus, Brunonianism's candidacy for consideration as a rationally valid medical model hinged on its conformity with Kant's emphasis on quantifiability, though its enthusiastic adoption was ultimately a product of Röschlaub's efforts to realign Brown with Romantic thought. At the centre of process of mobilization and realignment remain vital substances, a concept which was now able to reach a broader audience than Brown could have feasibly done alone.

Chapter 3—Brunonian *Naturphilosophie* and Intoxicated Knowing

The Intoxication of Philosophy: Kant

Given the ubiquity of Brunonianism in late 18th and early 18th century German medicine, it is worth assessing the extent to which Brunonian ideas truly affected the broader discussion of intoxicants in the thought of the period. In doing so, it is important to take a look at the idea's reception in the midst of two of the most influential Germans in the early-19th century: Kant and Schelling.

It is difficult to concretely assess the full scope of Kant's position on the Brunonian system. Kant was still alive and active in the days of Schelling and Röschlaub's collaboration and, while his death in 1804 barred him from the developments that followed, Kant had the opportunity to encounter Brunonianism before Röschlaub reconceptualized it. In fact, there is some evidence directly associating Kant with Brown's system. As is the case with luminaries who are appreciated in their time, many of Kant's former pupils scrambled to publish memoirs on the life of their great mentor upon hearing word of his death. One particularly noteworthy source of biographical information on Kant's final years is a memoir written by Ehregott Andreas Christoph Wasianski (1755-1831), Kant's secretary, caretaker, and confidant in his dying days. Wasianski's *Immanuel Kant in seinen letzten Lebensjahren* is also our most valuable source of insight into Kant's perception of the Brunonian system that was sweeping across Germany, even if it is only secondhand.

Wasianski reports that "Kant war sehr heterodox in der Medizin" (Wasianski, 1804, 189).[16] Kant's "Sorgfalt für die Erhaltung seiner Gesundheit war auch die Ursache, warum ihn

[16] "In medicine, Kant was very heterodox".

neue Systeme und Erfindungen in der Medizin so sehr interessirten" and "[e]r sah das Brownsche als eine Haupterfindung dieser Art an" (Wasianski, 1804, 42).[17] By Wasianski's account, Kant had been introduced to Brown via the writing of Melchior Adam Weikard (1742-1803), after which Kant developed a deep trust in the system (Wasianski, 1804, 42). Weikard was actually one of the first Germans to promote Brunonianism, arranging for a German print of Brown's *Elementa* in 1794, to be followed by the release of a German translation the next year alongside Weikard's outline of Brown's system (Broman, 2002, 144). Thus, Wasianski's retelling actually establishes Kant as one of the earlier, pre-Röschlaubian (non-Fichtian) adopters of the Brunonian system, potentially going as far back as 1794-1795.

Kant's adoption of Brown's ideas prior to the interpolation of Romantic influences is of especial interest. In light of Röschlaub's efforts to bring the Brunonian system into alignment with Fichtian philosophy, Fichte's rivalry with Kant may have soured Kant against the system, had he not previously been exposed to Brown's ideas by other means. Yet, for many, Brunonian theory became synonymous with Röschlaub's *Erregbarkeitstheorie* to such a degree that they appear to be conflated, and so any influence resulting from Kant's interest in the idea would not likely have detracted from the authority of Röschlaub's interpretation.

If Wasianski's account is to be believed, Brown's system was not merely an object of personal or even strictly medical interest to Kant. In fact, Kant may have understood the Brunonian system to be centrally involved with his notion of enlightenment:

> Er hielt es für einen bedeutenden Fortschritt, den nicht nur die Medizin, sondern auch mit ihr die Menschheit gemacht hätte, fand es mit dem gewöhnlichen Ganze der Menscheit: nach vielen Umwegen vom Zusammengesetzen endlich zum Einfachen zurückzukehren, sehr über einstimmend, und versprach sich von ihm noch vieles andere Gute, unter

[17] Kant's "care for maintaining his health was also the reason why he was so interested in new systems and inventions in medicine" and "he saw Brunonian as a major invention of this sort".

> andern auch in ökonomischer Hinsicht für den Patienten, den Armut hindert, die kostbaren und zusammengesetzeten Hilfmittel zu gebrauchen. Sehnlich wünschte er daher, dass dieses System bald mehr Anhänger erhalten und allgemein in Umlauf gebracht werden möchte (Wasianki, 1804, 42-43).[18]

Just as the primary concern of Kant's *Was ist Aufklärung* is the emancipation of the subject from the social condition of *Unmündigkeit* into rational, individuated *Freiheit,* the dissemination of the Brunonian system manifests in the unfettering of the body from a state of socio-economic desolation (Kant, [1784]1999). Here, Kant is alleged to realize the Brunonian system as not merely a revolution in the science of medicine, as it was for Weikard and Röschlaub, but a missing component of the social and philosophical revolution represented by his own work.

With the great importance that Wasianski would have us believe Kant saw in the Brunonian system, it becomes far easier to make sense of Kant's famously rigid schedule. Wasianksi makes no secret of framing Kant's regimented consumption of drams of wine or rum as being "à la Brown" and it would seem many of Kant's daily habits were informed by Brunonian ideas as well (Wasianki, 1804, 189). Kant's day began at five o'clock in the morning when Kant took one cup of frequently refilled tea, shortly followed by a pipe of rapidly smoked tobacco (Wasianski, 1804, 40). Kant spent the rest of his morning lecturing or writing, until exactly a quarter to one. This was when the cook brought Kant a jug of wine or spirits to be set aside for later (Wasianski, 1804, 40). Kant would then eat lunch, go on a walk, and spend his

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[18] [Kant] considered it an significant advancement, not only for medicine but for all, finding it in agreement with the entirety of humankind: [for humanity] to return, after many detours, from the compound to the simple meant, among other things, a good thing for the patient in the economic regard, who is prevented by poverty from using expensive compound remedies. [Kant] therefore ardently wished that this system would soon receive more followers and be brought into general circulation.

afternoon in light work. After a lively dinner with friends, Kant would take a brisk walk without companionship, before finally reading in his library until dusk. By 10 o'clock, Kant was in bed, wrapped in blankets "gleichsam wie ein Cocon" to guard against the cold (Wasianski, 1804, 29-31).[19] If Wasianki's account is taken as wholly credible, each of these activities, though quite unremarkable in themselves, can be understood as components of a broader, totalizing application of the Brunonian system in Kant's daily life. Rising early; moderating one's consumption of food, coffee, and tea; and regularly exercising are all decisions which a tenacious Brunonian would make to quell any excessive excitability. Correspondingly, Kant's scheduled use of tobacco and alcohol, perhaps even the "cocoon" in which he was accustomed to sleeping, would either provide a necessary vital stimulus or slow the diminishment of excitability. The strongest piece of evidence that Kant's lifestyle was a concerted application of Brunonian theory, and not merely the product of an uncommonly idiosyncratic man in his final years, is how measured each activity seems to be. Food, drink, and above all intoxicants are consumed with deliberate regularity and are never taken in excess, nor does Kant ever abstain. Rather, Kant's lifestyle is a treatment plan—wine or spirits his prescription.

But Immanual Kant's commitment to Brunonian medicine, no matter how devote, is of little consequence if it does not figure into his more widely read work. How do intoxicants figure into his post-1795 work, of which the most famous pieces include *Die Metaphysik der Sitten in zwei Teilen* and *Anthropologie in pragmatischer Hinsicht abgefaßt*? And, lastly, how do these texts diverge from Kant's earlier work? Kant's earlier understanding of the body relied on a sophisticated understanding of vitalism that served as a pre-harmonizing principle (Shell, 2013). Thus, Brunonianism was a further elaboration, or specification, of the terms and nature of his

[19] […]"just like a cocoon."

pre-established conception of the place of vital force in the body. The ultimate question here is: did Kant come to perceive intoxicants as "vital substances" and, if so, how was this significant for his philosophy?

Many of Kant's starkest remarks about intoxicants and their effects are found in *Die Metaphysik der Sitten in zwei Teilen.* Kant devotes a meaningful section of text to the stupefaction of oneself through food and drink, which further includes a passing discussion of opium. Here, Kant identifies both intoxication and gluttony as examples of vices derived from "der Mißbrauch der Genießmittel, wodurch das Vermögen des intellectuellen Gebrauchs derselben gehemmt oder erschöpft wird" (Kant, 1797, 427).[20] Though at first Kant appears to approach states of drunkenness and gluttony with equal disgust, a distinct conception of intoxication as a mental state quickly emerges. Excessive eating "blos den Sinn als passive Beschaffenheit [...] beschäftigt," while intoxication stimulates the imagination into a "thätiges Spiel der Vorstellungen" (Kant, 1797, 427).[21, 22] Although both stupefy the mind, Kant evidently attributes significance to the different means by which they afflict the mind's capacity to think rationally. While both in turn reduce a person to the merest representation of a human being in Kant's eyes, intoxication does so by (over)stimulating the imagination. This sentiment is echoed in Kant posthumously published *Anthropologie,* where he once again credits intoxication with stimulating the power of imagination (Kant, 1798, 165).

[20] […] "misuse of the means of nourishment which limits and exhausts our ability to use them intelligently."

[21] […] "brings the senses into a passive state."

[22] […] "active play of representations."

This leads Kant to ask if intoxication is entirely a failure of one's moral duty to oneself or if it can be justifiable. On this subject, Kant is characteristically tempered. Excessive intoxication is, as Kant understands it, a debasement of oneself, that dulls the senses and weakens one's resolve to conduct their affairs. In this sense, Kant argues that excessive intoxication falls short of one's duty to oneself, as it violates our recognition of our own humanity as rational subjects (Kant, 1798, 427; Timmermann, 2006, 517). Simultaneously, Kant appears to suggest that intoxication is not entirely reducible to enjoyment of the senses, instead understanding it to stimulate the active play of the imagination. The terminology Kant uses to describe the activity of the intoxicated mind is that of *play,* language Kant also associates with the imagination in aesthetic judgments as discussed in *Kritik der Urteilskraft.* In spite of this, a discussion of intoxication or intoxicants does not figure in any of the Kant's critical works—save a passing remark that one's taste in wine does not amount to an aesthetic judgment (Kant, 1790). It appeared at first glance that the moral problem of intoxication was simply a matter of excess, and that it is on these grounds that Kant advocated for wine over opium and spirits (Kant, 1797, 428). Though, it appears there's more to Kant's reasoning here.

However few details are provided in the text, Kant's assessment of the morality of drinking and drugging can tell us a great deal about how he understood the relationship between intoxicants and the body. To this point, there are two basic questions that are worth exploring: (1) why is it that Kant identifies intoxication with the imagination at all, rather than with the senses? (2) why does Kant determine that intoxication is a violation of one's duty to oneself, given the possible benefits he outlines? Before the latter question can be answered, the first question needs to be properly considered and it is the answer shared by these two lines of questioning that will shed light on the place of intoxication in Kant's thought.

As discussed, the language Kant uses to describe the state of intoxication bears a striking resemblance to his discussion of aesthetic judgement. In *Kritik der Urteilskraft,* Kant explains that "[d]ie Erkenntnißkräfte, die durch diese Vorstellung ins Spiel gesetzt werden, sind hiebei in einem freien Spiele, weil kein bestimmter Begriff sie auf eine besondere Erkenntnißregel einschränkt" (Kant, 1790, 217).[23] Imagination generally conforms to the concepts applicable to a given situation as presented through the senses. However, the aesthetic brings the imagination and the understanding into *freien Spiele* (free play) by introducing sensory material without constraining imagination to any particular rule of cognition. As a result, the imagination and the understanding participate in a certain conceptual freedom, which follows the laws of cognition without conforming to a specific law. The importance of this for Kant's critical system is that aesthetic judgement appears to be a demonstration of the noumenal self's capacity for free will, thus engendering the possibility of moral duty. Importantly, this experience is accompanied with a pleasant sensation for the subject (Kant, 1790, 217-218).

Intoxication, comparatively, is described as eliciting "ein thätiges Spiel der Vorstellungen" through the imagination (Kant, 1797, 427).[24] Although he contemptuously refers to intoxication as an animalistic enjoyment of the senses, the Vigilantius notes on Kant's *Metaphysik der Sitten* confirm the impression given by the text that Kant does not regard the primary object of intoxication to be sensory enjoyment but rather the play of the imagination (Vigilantius, 1997, AA XXVII, S. 527, Z. 27). The result of this is not only pleasure, but a livening of spirits and even (in the case of opium) a dreamy inwardness (Kant, 1797, 427-428).

[23] […] "The cognitive powers brought into play by this presentation are in free play because no particular concept limits them to a specific rule of cognition."

[24] […] "active play of the representations."

A possible explanation for how Kant understands this is given in *Anthropologie,* where Kant states “die Trunkenheit ist der widernatürliche Zustand des Unvermögens seine Sinnenvorstellungen nach Erfahrungsgesetzen zu ordnen” (Kant, 1798, 165).[25] This is because of the excessive activity, or play, of the imagination. A secondary effect is an anesthetization and disorientation of the sensory faculties, which Kant likens to when one has quickly woken up from a deep sleep (Kant, 1798, 166). Drugs and alcohol seem to have this effect, as Kant tells us, by stimulating the vital force (Kant, 1798, 169-170)!

On the finer points of Kant’s understanding, the absence of a direct, systematic account leaves some aspects of this explanation ambiguous. It remains unclear from the text alone whether *active play* occurs because intoxicants stimulate the vital force, artificially overwhelming the senses and thereby freeing up the imagination, or because artificially stimulating the vital force holistically invigorates both the powers of imagination and the sensory faculties.

Just as the aesthetic experience brings the understanding and the imagination into free play, intoxication brings the imagination, and with it the understanding, into active play. Parallels can even be found in Kant’s explanation of sensory enjoyment, for—if intoxication were merely about enjoyment of the senses—it would only be comparable to judgements of agreeability (“the wine tastes good”), which are always subjective. Instead, both the aesthetic judgement and the state of intoxication are associated with a distinct activity of the imagination. This might appear to be an unlikely comparison. But aesthetic judgement and intoxication remain the only instances in Kant where the imagination is brought into a distinct state of play,

[25] […] “Drunkenness is the unnatural state of inability to classify your senses according to the laws of experience.”

unbound from compliance with a specific rule of cognition. To this point, it is worth considering if Kant is suggesting that intoxication can be morally significant in a fashion similar to aesthetic judgement. If this is the case, Kant leaves little trace of it and Kant does make repeated reference to the dangers of intoxication, in particular the possibility of developing a need for the pleasurable experience it imparts.

This brings us to the second question. Kant makes a point of asserting that the moral difficulties of intoxication "wird hier nicht aus dem Schaden, oder den körperlichen Schmerzen, (solchen Krankheiten), die der Mensch sich dadurch zuzieht, beurtheilt" (Kant, 1797, 427).[26] Disease caused by excessive intoxication can only ever establish a rule of prudence, rather than a moral duty (Kant, 1797, 427). Kant himself freely admits that intoxication, perhaps even minor excess, is not without its benefits, including a virtuous candidness, social limberness, elevation of the spirits, and even reprieve from the worries of day to day existence (Kant, 1798, 170-171). This is all in addition to, or a result of, the effects on the imagination just discussed. What then is the basis for the moral duty of restraint in all matters of intoxication? The answer it appears is not reducible to the debasement of befuddlement, but rather the risk of dependence.

To understand this argument, it is important to recall Wasianki's testimony that Kant was a committed Brunonian, by the time he wrote *Metaphysik der Sitten.* With that in mind, Kant's initial connection of intoxication with illness can be taken quite literally, as excessive stimulation is an "actual" cause of disease. Brown's model, which establishes the bodily need for outside stimulus and in turn opens the door to behavioural feedback loops between vital force and stimuli, has elsewhere been attached to the concept of drug and alcohol dependence. In fact,

[26] […] "is not judged here by the damage, or the physical damage (such as illnesses) that man gets through [excessive intoxication]".

Brühl-Cramer's 1819 text "Ueber die Trunksucht und eine rationelle Heilmethode derselben" used Brown's system to establish a clinical theory of alcoholism (Kielhorn, 1996, 121). Kant himself recognized that a real possibility of using intoxicants was the need to use them again in the future, and at higher doses (Kant, 1798, 427). In such a case, excessive consumption entails a real risk to one's bodily autonomy, subordinating rational freedom to an irrational cycle of bodily dependence. Far from the contextual and subjective question of debasement, the state of dependence on alcohol and drugs fits the very definition of a failure in one's duty to oneself (Timmermann, 2006, 517).

Relying on Wasianski's account, it becomes overwhelmingly clear that Kant perceives intoxicants as what I have earlier described as "vital substances". There is little reason to doubt Wasianki's reckoning of events, though when the subject is a figure as historically consequential as Immanuel Kant there is an understandable hesitance to rely on second hand accounts. Kant's approach to the matter of intoxicants to the concept of intoxication, as presented here, nevertheless supports the argument that Kant treats intoxicants as vital substances. Even if Wasianki's memoir is taken out of consideration, Kant still conceives of drugs and alcohol as having a privileged relationship with the vital force, far removed from other medicines, and uniquely significant to the human body and mind. But Kant was not the only foundational figure of the early 19th century for whom vital substances played a crucial role. As previously discussed, Schelling, too, was deeply invested in the notion of vital stimulus, to such an extent that it figured centrally into his *Naturphilosophie.*

The Intoxication of Philosophy 2: Schelling

Before rising to great fame (and, at times, infamy), financial need found Schelling in Leipzig, where he tutored a pair of young barons between the years 1796 and 1798. His job was to attend lectures on scientific subjects and reconvey their content to his pupils (Risse, 1976, 322). Of course, this was simultaneously an opportunity for the young Schelling to interact with experts on an assortment of subjects concerning the study of the natural world. Understanding the influence of this period of Schelling's life on the development of the system of *Naturphilosophie* is fundamental to any appreciation of Schelling's departure from Fichte. This is not to detract from Schelling's originality; however, it is difficult to conceive of the existence of Schelling's *Naturphilosophie* without the support of the extant tradition of Romantic science in the German sphere. Wolfgang von Goethe (1749-1832), Georg Stahl, Alexander von Humboldt (1769-1859), Karl Kielmeyer (1763-1844), Johann Blumenbach (1752-1840), Johann Reil (1759-1813), and Johann Ritter (1776-1810), alongside countless others, helped pave the way for the later acceptance of Schelling's idea by first uncovering "Romantic" nature as a dynamically unfolding holism.

Timothy Lenoir and Peter Watson credit earlier Romantic biologists Johann Blumenbach, Johann Reil, and Karl Kielmeyer with shaking up hegemonic classification schemes and paving the way for Darwinism (Watson, 2010; Lenoir, 1982).[27] Blumenbach, as well as his acquaintance Johann Reil, saw an immanent purposiveness in the organization of life (Lenoir, 1982; Watson, 2010). Organisms and their constituent parts were evidently subordinate to a lawful teleology which they understood in terms of a formative vitality. Blumenbach's pupil from 1786-88, Karl Kielmeyer further developed on these ideas in his attempt to derive the fundamental laws of

[27] Lenoir, in particular, identifies Kielmeyer and Blumenbach with the development of a novel biological science, centred around morphology. Lenoir's argument is, in part, in contention with Robert Richard's. My own thesis extends Lenoir's, while also drastically limiting its farthest reaching conclusions.

organic form by way of comparative anatomy (Lenoir, 1982; Watson, 2010). Kielmeyer's eventual argument for morphological affinity across animal forms led him to ultimately suggest that many, if not all, living species had in fact come from other species (Watson, 2010). There was also Goethe's famous 1790 scientific poem "Die Metamorphose der Pflanzen" which analyzed plant morphology as a dynamical unfolding of an archetypal concept or species (Goethe, 1811/1989). Watson's argument follows that the underlying principles of universal holism and dynamic relationality that pervade Romantic thought de-essentialized the classification systems that Linnaeus and others had erected throughout the natural world. Elements of all of these ideas make up the foundations of Schelling's thought. In some cases, the influence of early Romantic life science was more direct, such as in Schelling's references to Kielmeyer and Blumenbach in *Von der Weltseele*. Whether by generating a community that would eventually be receptive to Schelling's thinking or through Schelling's own efforts to mirror these ideas, the conceptual tendencies that pervaded these earlier Romantic thinkers made Schelling's fusion of natural science and post-Fichtean speculative philosophy a viable model within strong sectors of the German scientific discourse.

During this period, Schelling was also greatly inspired by chemist Johann Ritter's work on galvanism and Ritter, in turn, would come to see Schelling's *Naturphilosophie* as a viable basis for a systematic grounding of the physical sciences. A gifted but lavishly indulgent young experimentalist, Ritter's lodging were reportedly ridden with spent bottles of alcohol and he was known to consume large doses of opium, possibly to help treat pain caused by extreme electro-physical self-experimentation (Strickland, 1998, 456).[28] With the help of an introduction by his

[28] Ritter tested the effects of the electric pile on all manner of bodily function, from sneezing to defecation, and even ejaculation (Strickland, 1998, 457). The latter experiment involved connecting

friend Novalis (whose own fondness for wine and narcotics crops up in "Hymen an die Nacht"), Ritter rose to a position of prominence and authority within Romantic circles (Snelders, 1970, 201; Novalis, 1800/1988, 14). Schelling was introduced to Johann Ritter in 1798 when he saw Ritter lecture on his book *Beweis, daß ein beständiger Galvanismus den Lebensprozeß in dem Thierreich begleite* (Ritter, 1798; Weatherby, 2016, 186). Here, Ritter expanded upon Alessandro Volta's observation that heterogeneity between conductors (polarity) was a precondition for both inorganic and organic (galvanic) conduction (Ritter, 1798; Weatherby, 2016, 186). Ritter argued, and attempted to experimentally demonstrate, that galvanic conductivity arose not only out of the heterogeneity of muscle and nerve but from within nerves themselves (Ritter, 1798; Weatherby, 2016, 186). For Ritter, this raised the question of whether a satisfactory degree of heterogeneity itself was all that was required to generate an electric impulse, both within and without the organism (Ritter, 1798; Weatherby, 2016, 187). With the discovery of the electric pile in 1800, Ritter went on to conduct a number of extravagant experiments on the effect of electricity on sensory nerves. Unfortunately, his promising, if peculiar, career was cut short. Johann Ritter died in 1810 at the age of 33, leaving behind questions about the impact of his risky experimental practices on his overall health.

Unfortunately, further information on what Schelling studied during the period between 1796-1800 is sorely lacking, due to a temporary lapse in the correspondence between Hegel and Schelling (Fuhrmans, 1962, 73). Whatever it was that he studied, its impact on Schelling was tremendous. In the ensuing years, Schelling published *Ideen zu einer Philosophie der Natur als Einleitung in das Studium dieser Wissenschaft*, *Von der Weltseele*, and *Erster Entwurf eines*

the electrical pile to his reproductive organs (Strickland, 1998, 457). Ritter wrote to his publisher about his desire to marry his electrical pile (Strickland, 1998, 455).

Systems der Naturphilosophie, all before 1800. Röschlaub's interpretation of Brown provided Schelling with a conception of vital phenomena which upheld the individual organism as an independent entity while affirming the fundamental unity of self and world. In addition to impressing a preoccupation with the role of galvanic activity, Ritter further helped Schelling develop his *Naturphilosophie* by introducing differential polarity as a possible ground for the occasion of not merely electrical impulse but all manner of force and activity.

In regards to the philosophical legacy of *Naturphilosophie,* Schelling understood his system of *Naturphilosophie* as both furthering and clarifying Fichte's philosophy by overcoming some of its limitations and taking it into the next phase of its development. Kant had sought to limit philosophy to what we, as subjects, could know. Fichte meanwhile was concerned with the practical, ethical dimension of the transcendental subject. Nature could only be recognized as an idea emergent of the synthesis of necessarily limited perceptions. Both the philosophies of Kant and Fichte had, in this sense, reduced nature to mental projection. This greatly narrowed the nature of the interface between self and world. Schelling instead hoped to develop a real, concrete connection between nature and the mind. To accomplish this lofty ambition, Schelling's *Naturphilosophie* shifted focus away from the limitations Kant imposed upon the subject and instead asked how nature must be in order for the human mind to be as it is. This was an exercise in turning Kant on his head: where Kant quested after the categories necessary for consciousness to be possible, Schelling sought to uncover the "categories" in nature that made the human mind possible. Beginning with factical nature as an *a priori*, Schelling characterized primordial nature as a struggle between an infinite force of expansion and an infinite force of contraction. This dialectical struggle between opposed and yet infinite forces gradually gives rise to quantities such as space and time before generating natural forces and finally the physical world as we understand it. Thus, the entirety of nature is imagined as a self-organizing totality fundamentally determined by conflicting forces. The human mind is nothing less than the culmination of teleological evolution of spirit, first unconscious in nature and realized as conscious in the human mind. In this way, Schelling overcame the problems inherent to Kant's subject-object distinction by discovering nature as the dynamic ground of human consciousness, which is distinct from and yet essentially an unfolding of nature itself. In this sense, each entity was both a reflection of the dynamic forces behind all that is and a self-contained whole: the many-in-one and the one-in-many.

At the heart of Schelling's optimistic project were the scientific ideas he had been exposed to by teachers, friends, and colleagues. Chief among them were the Brunonian concepts of Röschlaub and the galvanic principles of Ritter, both of which would shape Schelling's conception of living beings. Rather than immediately seek answers in Schelling's published work, it is worth considering one of the greatest scandals of Schelling's life: the death of Auguste Böhmer. Here, Schelling's support for the Brunonian system, and vital intoxicant therapy it promoted, figured into a very real life and death struggle with holistic, Galenic medicine. Most of all, it demonstrated the concrete, practical nature of Schelling's belief in the curative potential of intoxicating stimulants.

Only 15-years old at the time of her death on the 12th of July, 1800, Auguste Böhmer was the step daughter of August Wilhelm Schlegel and the daughter of Caroline Schlegel (Wiesing, 1989, 275). Likely due to the scope of the scandal that surrounded her early death, many aspects of Auguste's life have been obfuscated by centuries of academic debate, at times bordering on scholarly gossip. Officially, she was the daughter of Caroline and Johann Franz Wilhelm Böhmer, although biographer Walter Ehrhardt has made a case for the suggestion that Auguste's biological father may have been Johann Wolfgang von Goethe (Ehrhardt, 2006, 277). As for her connection to Schelling, Auguste has been represented as Schelling's fiancée or, at the very least, the daughter of Schelling's lover (Caroline divorced Schlegel and married Schelling later in 1803) (Steinkamp, 2002, 478). Whatever the nature of their relationship, the burning question then, as today, was if Friedrich Wilhelm Schelling was responsible for the death of Auguste Böhmer (Wiesing, 1989, 275).

In May 1800, Caroline, Auguste, and Schelling traveled together to Bamberg, with the stated intentions that Schelling would attend some of Röschlaub's lectures on Brunonianism while Caroline would retreat to the Franconian bathes (Wiesing, 1989, 277). It was hardly a secret that the secondary purpose of the trip was to allow Caroline (then still Schlegel's wife) and Schelling to spend time together (Wiesing, 1989, 277). By July, Caroline had fallen ill with dysentery and recovered, which prompted Schelling to return from a visit with his parents, only for Auguste to fall ill shortly thereafter. Auguste's treatment was initially only being overseen by a surgeon from Bad Kinninger named Büchler, but Schelling, having just attended medical lectures from Röschlaub, soon interceded. Though there was tension between the two concerning the proper course of treatment, Schelling officially conceded to the positive prognosis given by Büchler until as late as two days before Auguste's death. This can be seen in Schelling's letters to August Schlegel, although it is worth asking if Schelling has masking a deeper concern for the sake of Auguste's stepfather (Fuhrmanns, 1962, 196). When Auguste ultimately did not recover, Büchler rushed to place the blame for her death on Schelling's interference, publicly and privately spreading this opinion amongst the officials and dignitaries of Würzburg (Wiesing, 1989, 278).

The primary dispute over the proper course of medical treatment centred around opium. Büchler and Schelling not only disagreed about the appropriate dosage but also the particular preparation that ought to be used (Wiesing, 1989, 280). Büchler, consistent with the Galenic compound medicines of the period, prescribed the young Auguste opium mixed with rhubarb tincture and Arabic gum (Wiesing, 1989, 280). In a letter to Schlegel, Schelling remarked that the rhubarb tincture and gum Arabica was—at best—diluting the opium and—at worst—a laxative which actively undermined Auguste's recovery, instead supporting a prescription of smaller doses of pure opium (Fuhrmans, 1962). After all, Brown identified colonic illnesses such as dysentery as primarily diseases of asthenia, and pure opium was the remedy most capable of rectifying any radical disequilibrium in vital force (Brown, 1795, 182).[29] It is impossible for modern researchers to determine the extent to which Schelling's involvement helped or hampered Auguste's progress. It is clear, however, that perceptions of culpability were inevitably impacted by Schelling's role as an ambassador for the Brunonian system, as well as Büchler's symbolic stance against it. Büchler and Schelling were caught in a real life-or-death struggle over the very concept of disease and the body, with Auguste tragicly paying with her life.

Two years after the death of Auguste, an anonymous reviewer in *Allgemeine Literatur-Zeitung* criticized the new philosophy of a medical student named Joseph Reubein, writing ominously about their hope that Reubein not befall the same fate as Schelling by killing those he hopes to heal (Anonymous, 1802, 329). This anonymous reviewer, generally identified as either Franz Berg or Christian Schütz, passes off Schelling's guilt as an open secret. Yet, Brown's work would continue to exert periodic influence for roughly another twenty years. As for Schelling,

[29] Ironically, pure opium may well have worked, as the effect of opiates as a cure for diarrhea is now well known.

this tragic story makes one thing overwhelmingly clear: Schelling's alignment with the Brunonian system was not an association of convenience for the purposes of fleshing out his *Naturphilosophie* nor a passing interest. Schelling vehemently believed that the Brunonian system would save Auguste's life. In her final days, it was Andreas Röschlaub, then the greatest German Brunonian, that Schelling called upon (Wiesing, 1989).

So, it comes as no surprise that Schelling raised opium as the prime example of his principle of causation in the *Erster Entwurf*. Schelling upholds the Brunonian notion that "[d]ie Lebensthätigkeit erlischt [würde erlöschen] ohne Objekt, sie kann nur durch äußeren Einfluß erregt werden" (Schelling, 1799a, 82).[30] But the organism, Schelling contends, is not merely "une machine qui monte elle-même ses ressorts" (La Mettrie, 1748, 34).[31] Because it is a self-organizing body and not merely a dependent, an organism itself should have some role in constituting itself in relation to external influences or, as Schellings puts it, "dieser äußere Einfluß [auf das Produkt] ist selbst wieder bestimmt durch die organische Thätigkeit" (Schelling, 1799a, 82).[32] The organism is the primary agent in its own construction—but it can only do so by interacting with the outside world. The external world constantly constructs the organism as object and the organism, in turn, develops itself as subject. For, Schelling reasons, if the organism as subject was determined solely by the input of the external world then the organism would possess no capacity for independent activity (Schelling, 1799a, 82), Because of the organism's *being a subject* it constantly constructs itself through its engagement with the

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[30] […] "[t]he vital activity goes out [would go out] without object, it can only be excited by external influence."

[31] […] "a machine that winds its own springs."

[32] "[…]this external influence [on the product] is itself determined by organic activity."

external world, which takes the organism as object. The dynamic unity of activity and reactivity, this duplicity of organism as subject and object, is what Schelling understands Brown to have intended by the concept of excitability. This accounts for the lived experience of the subject as well as, in the spirit of Schelling's doctrine of the all-in-one and one-in-all, the microbiological activities of organic life itself. Thus, the body is not something *acted upon*, as though it were dead material; it is both a body and *productivity*, with the potential for an external influence (such as opium) being made possible only in and through the living body (Schelling, 1799, 144). Schelling frames this as an agonal dialectic where, just as the body warms itself in cold weather, the body constantly strives for equilibrium in the face of constant external influences. In this sense, Schelling firmly establishes that external influences cannot affect the entire organism chemically, as the effects of the external influences are only ever mediated by the organism (Schelling, 1799a, 82-83).

This is where opium enters Schelling's argument as an exemplification of his theory of causation in the body:

> Daß das Opium erregend wirkt, ist erklärt aus seiner chemischen, oder, was dasselbe ist, seiner elektrischen Beschaffenheit (darum wirkt es auch im Galvanismus) - aber seine mittelbare, d.h. durch die Thätigkeit des Organismus selbst vermittelte Wirkung ist narkotisch, und diese Wirkung ist freilich chemisch unerklärt: denn sie ist indirekt. So zeigt sich im Ganzen, daß eben dieselben Materien, welche die heftigste Erregbarkeit verursachen (was aus ihrer chemischen und elektrischen Beschaffenheit erklärt werden muß), indirekt die Erregbarkeit erschöpfen (was nun freilich nicht mehr aus ihrer chemischen Beschaffenheit erklärbar ist). Es ist kein Wunder, daß es mit den chemischen Erklärungen nicht fort will. Die letzte Wirkung der äußeren Ursachen auf den Organismus kann nicht mehr chemisch erklärt werden (Schelling, 1799a, 83).[33]

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[33] "That opium has an exciting effect is explained by its chemical, or, what is the same, its electrical nature (that is why it also works in galvanism), but its indirect effect, that is, the effect mediated by the activity of the organism itself, is narcotic, and this effect is of course chemically unexplained: because it is indirect. Thus, on the whole it is shown that the very same materials which cause the most violent

Ultimately, the discussion of a possible chemical activity that underlies the bodily effects of opium are, for Schelling, a moot point. For Schelling, "[d]as ganze Geheimnis beruht auf jenem Gegensatz zwischen Innerem und Aeußerem, den man zugeben muß, wenn man in der Natur überhaupt etwas Individuelles zugibt" (Schelling, 1799a, 84).[34] By merit of the reflexive structure of Schelling's *Naturphilosophie,* the case could be made that Schelling's use of opium here could be replaced with nearly anything and is of little importance. Yet, from the wording alone, it is evident that the reference to opium specifically is fundamental to Schelling's argument. Here, opium is not merely opium, but (as has just been stated above) "Materien, welche die heftigste Erregbarkeit verursachen" (Schelling, 1799a, 83).[35] Opium represents the pinnacle of what I have termed "vital substances," holding a unique purchase on the organic, and thus becomes the crucial exemplification of Schelling's concept of the body. Citing anything weaker than opium in the hierarchy of vital excitation leaves space for counter-examples and creates a need for further explanation. The soporific effects of opium intoxication, framed by Schelling's conception of reciprocal activity, themselves become a reflection of the dynamic

excitability (which must be explained from their chemical and electrical properties) indirectly exhaust excitability (which, of course, can no longer be explained from their chemical properties). It's no wonder chemical explanations go no further than this. The ultimate effect of external causes on the organism can no longer be explained chemically."

[34] […] "the entire secret rests upon the on the opposition between inner and outer, which must be conceded if one concedes to anything individual in nature overall."

[35] […] "materials which cause the most violent excitability."

forces that underlie the foundational conception of the body expressed in Schelling's *Naturphilosophie,* and thereby the world.[36]

Naturphilosophie, Lorenz Oken, and Romantic Physiology

Though Schelling himself was not directly involved in the direction of any study of the natural world, his efforts to unite and systematize the disparate Romantic tendencies of many late-18th century scientists through transcendental philosophy would shape much of German scientific thinking for the next 30 years. In the life sciences, the impact of the Brunonian unification of physiology, aetiology, and therapeutics was difficult to ignore and the swelling ranks of the *Naturphilosophen* demonstrated that this was trend was only further propelled by the philosophical supporters of Schelling's system.

Of all of the students of the natural world who built upon Schelling's *Naturphilosophie*, none is more deserving of the moniker of arch-*Naturphilosoph* than Lorenz Oken. Born Lorenz Okenfuß (1779-1851), Oken was one of the earliest scientific adopters of Schelling's *Naturphilosophie*, demonstrating his commitment from the outset with the 1802 publication of *Grundriss der Naturphilosophie, der Theorie der Sinne, mit der darauf gegründeten Classification der Thiere.* Before considering how Oken further developed the notion of vitally stimulating intoxicants, it is important to first establish Oken's relationship with vitalism, morphology, and Brunonian excitability.

As the title suggests, *Grundriss der Naturphilosophie* was nothing less than a radical attempt to simultaneously deconstruct the Linnaean system of animal classification and establish a novel classificatory scheme which was consistent with the fundamental understanding of nature

[36] Brunonian physicians would term this reciprocal activity "contra-stimulus."

reflected in Schelling's *Naturphilosophie.* Oken proposed that the number of animal classes could be reduced to no more than five, with each class being associated with the historical development of a particular sense organ (Oken et al., [1802] 2007). The most rudimentary of the classes was that of the invertebrates; followed by fish; reptiles; then birds; and, finally, mammals. Invertebrates were counted as being one of the oldest and least sophisticated life forms. Fish distinguished themselves through the development of not only a spine but a tongue (Oken, 2007). Reptiles brought with them a true nose, giving animal life the capacity to draw breath from the open air, and birds were associated with the appearance of proper ear openings. Mammals were the last and most developed of the classes, holding all known sense organs in addition to movable eyes with lids. The boldness of this position embodied the revolutionary spirit of *Naturphilosophie.* A system which identified human self-actualization with the teleological fulfilment of nature's grand historical development, *Naturphilosophie* was more than a total theory of the natural world; it was a call to action. Thought of in this way, all life could be understood as progressive elaborations upon an originary theme, which Schelling understood as a mirroring of nature's own developmental process from a primordial interplay of forces. It is to be expected that the social fabric of human society also reflected this pattern, as both Fichte and Hegel (& co.) would eventually argue.

By 1805, Oken's *Die Zeugung* argued that all organic structures consisted of vesicles and cells, which were themselves derived from the collection of *Urschleim* (protoplasma) (Oken, 1805). This meant that special differentiation was a matter of disparities in the procedural organization of the protoplasmic masses. The next major development in Oken's work was the publication of the first edition of the *Lehrbuch der Naturphilosophie* in 1810, which would see further editions in the years that followed. While *Grundriss der Naturphilosophie* and *Die*

Zeugung focused exclusively on the life sciences, the *Lehrbuch* extended Oken's system into the inorganic world, identifying in the entirety of nature an *a priori* mode of classification founded on the principles of evolution from more basic forms. As discussed, the blurring of classificatory distinctions was already present in the works of earlier Romantic physiologists like Kielmeyer, Riel, and Blumenbach. But it was the addition of Schelling's *Naturphilosophie* that provided a framework through which Oken could subsume the entirety of the natural world into a central developmental model.

Schelling understood excitability to be the first property of the organism (Schelling, 1799a, 144). This conception of the underlying duplicity of the organic body as both a holistic account of the experience of organic bodies and a description of the underlying biological activities that make life possible had a profound effect on Oken's thinking. Unsurprisingly, Schelling himself brought the concept of excitability as an underlying tension or duplicity—life itself—into his understanding of morphology and the historical development of a species. It's clear that Schelling perceived the comparative anatomy of Blumenbach and the associated preoccupation with morphological characteristics as an obfuscation of the possibility of a more essential project of comparative physiology (Schelling, 1799a). This is because, for Schelling, the outwardly visible components of organic bodies are actually expressions of the proportional intensities of organic forces inherent to the organism (Schelling, 1799a, 171). The oppositional interactions of these organic forces give rise to complex sequences of biological activities which determine the physical form of a given organism as well as its specific nature. Thus, any classification scheme established on the basis of anatomical features can only ever sign toward a more natural system of speciation on the ground of the variable intensities of organic activities. Schelling identifies the organic forces in question are sensibility, reproductive force, and irritability (Schelling, 1799a, 171-172). All of these organic forces, whose entangled conflicts are expressed in the organism as a discrete entity, are ultimately derived from Brunonian excitability, as the first property of all living things (Schelling, 1799a, 144).

It is in this way that Schelling succeeds at excising any materialist notions from the Brunonian concept of excitability in order to find in it a reflection of the essential interplay of forces that lay at the feet of Being as a whole. Speciation, thus, is analogous to the originary differentiation of the universe from a primordial unity, with excitability being the overarching principle of organic existence. Species, properly identified, would then be defined by the relationships of underlying, organic forces that are derived from the organizational principle of excitability. This is on the basis of the implicit identification of excitability as both that which makes life possible and that which, through the relative fixedness of its derivations in a given organic body, engenders the appearance of species forms.

This becomes a crucial point when considering Lorenz Oken's reception of Schelling, his position as a vitalist, and the influence of vital substances. While Schelling was profoundly inspired by the scientific research he encountered in establishing post-Fichtian foundations of *Naturphilosophie*, Schelling was not much of a scientific researcher himself. Oken brought *Naturphilosophie* into the study of the natural world, into the lab, and realized *Naturphilosophie* as a full-fledged research program rather than merely an *ad hoc* framework. Further still, Oken understood *Naturphilosophie* as the avenue through which the speculative work of natural history might become truly scientific, ultimately combining with chemistry, anatomy, and physiology into what Oken called *Biologie* (Gambarotto, 2018b, 62). Oken's *Grundriss*, *Zeugung,* and *Lehrbuch* were all examples of physiological research conducted with

Naturphilosophie forming its initial premise, guiding its methods, hypotheses, and interpretations of the evidence.

At first glance, Oken appears to differ from Schelling on a crucial point by assuming an anti-vitalist position. This is, at the very least, how Hans Driesch generally interprets Oken in his *Geschichte des Vitalismus* (Driesch, 1922, 92). Oken does lend his voice to the opposition against the more rudimentary concept of *Lebenskraft* found in Riel and elsewhere, unambiguously asserting that "[d]er Galvanismus ist das Princip des Lebens [and] es gibt keine andre Lebenskraft, als die galvanische Polarität" (Oken, 1810, 10).[37] Yet, both Oken and Schelling prepare distinct modes for understanding the nature of organic activity, not a vital force but nevertheless a distinct vital activity. For one, although galvanic energy is closely associated with electricity, Oken does assert the uniquely organic nature of galvanic force. For, "der Magnetismus hat eine Basis: sie ist das Metall[,] der Chemismus hat eine Basis: sie ist das Salz[,] so hat der Galvanismus eine Basis: sie ist die organische Masse" (Oken, 1810, 11).[38] Oken takes this notion as far as the assertion that "Organismus **ist** Galvanismus in einer durchaus gleichartigen Masse" (Oken, 1810, 10).[39]

At such a point it becomes exceedingly difficult to uphold the suggestion that the particular association that Oken establishes between galvanism and organic life can be understood as anything other than vital in nature. Although Oken, like most *Naturphilosophen* and many Romantics, argued for the essential unity of the organic and inorganic world, the

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[37] "Galvanism is the principle of life [and] there is no other life force than galvanic polarity."

[38] "Magnetism has a basis: it is metal [,] chemistry has a basis: it is salt [,] so galvanism has a basis: it is organic matter."

[39] […] "Organism is galvanism in a homogenous mass."

objective of Oken's project was explicitly not to reduce life to a byproduct of physio-chemical processes. Rather, all of Being is likened to an organism, borne out of a primordial oneness. Organisms, in this way, are elevated to a position of distinction as the culmination of a teleological unfolding of the universe's own self-awareness. Galvanic force may not be a form of energy or force which is incommensurate with the inorganic world, though it does have a distinguishing relationship with organic bodies and in this sense galvanic force might be considered "vital".

Perhaps most important in making the case for Oken as vitalist is Oken's position concerning Schelling's conception of excitability. As Oken himself states, "die Erregbarkeit ist das allgemeinste Phänomen der organischen Masse, und kommt Pflanzen und Tieren zu" (Oken, 1810, 134).[40] Oken's 'excitability' [Erregbarkeit] is "das Vermögen die Natur zu assimiliren" (Oken, 1810, 134).[41] On this topic Oken's thinking seems to be at its absolute closest to Schelling, as he states in the *Lehrbuch:*

> In dem Gefühl geht das Thier immer über sich hinaus. Es ist so nur Erregbarkeit. In der Bewegung bleibt das Thier in sich. Nur aus beiden Zuständen[, as both nature and differentiated being,] geht das Selbstgefühl hervor (Oken, 1810, 135).[42]

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[40] […] "Excitability is the most general phenomenon of organic matter, and it belongs to plants and animals."

[41] […] "the ability to assimilate nature."

[42] "In feeling the animal always goes beyond itself. So it's just excitability. In motion the animal remains in itself. The sense of self emerges only from both states [as both nature and differentiated being]".

Galvanism is likened to the vital force in the sense of *Lebenskraft*; it is the animating force. This is not to say that galvanism is the first principle of vital activity, for, as Oken clarifies, the *first truth* of the organic is excitability, as the very possibility of animation.

All of these aspects of Oken's thinking, the continuity between Schelling's and Oken's formulation of vital activity, the underlying principles governing morphology, and the nature of excitability, make up the foundation of any analysis of the place of substances of intoxication in Oken's system. Though Oken does make sparing remarks in his earlier publications which demonstrate that Oken understood opium to be a highly effective and relatively safe therapeutic, there is an unfortunate scarcity of direct remarks on the function and nature of opium, or any other therapeutics, in Oken's corpus (Oken et al., 2007, 48). This is not on account of a judgement of insignificance on Oken's part, but a result of the broad and systematic approach embodied in the understanding of *Naturphilosphie* and *Biologie* that guided all of his endeavours. That said, it is possible to assess the extent to which the legacy of "vital substances" was perpetuated through his work.

As a starting point, it is worth recalling that Schelling himself understood the excitatory potential of opium and alcohol as pertaining to their apparent galvanic properties. In fact, Schelling was so certain of opium's galvanic properties that he goes as far as, quite confusingly, suggesting that opium, which is a plant principle, depresses a plant's potential for stimulation (because Schelling holds the irritability of plants to be a negative form of galvanism) (Schelling, 1799, 82). This is also the case for Oken, as will soon become clear.

All galvanic processes rely on polarization, the unresolved differential of which is the basis for further movement, development, and activity. Substances of intoxication would, thus, be expected to affect the body through some process of polarization. With respect to opium or other plant based intoxicants, Oken's *Lehrbuch* does make a singular mention of medicinally active plant principles, where he classifies them alongside plant poisons and elsewhere refers to alkali plant bodies as "betäubend" (stupefying/narcotic) (Oken, 1843, 209). This classification on its own is quite telling, particularly in light of how Oken makes use of the term 'poison'. Oken argues that poisoning of the organic body is thus destruction by some means of excessive polar equalization. Put another way, poisoning is the disruption of the galvanic process (Oken, 1843, 350). Saliva, for example, is a poison which breaks down organic matter (food) into the most basic organic components so that they can be taken up (Oken, 1843, 350-351). Localized and controlled 'poisoning' is thus part of the essential maintenance of any organic being.

It is worth questioning whether Oken's categorization of medicinally active plant principles alongside vegetable poisons was not merely coincidental and suggest that Oken's medicinally active plant principles function via his neutral concept of poison. Oken suggests that the difference between chemical or animal poisons and vegetable poisons is that plant derived poisons are nervous poisons—they affect the nervous system (Oken, 1843, 350). This sheds light on Oken's use of the word (*betäubend*) to describe alkali plant products: Oken's stupefying plant alkalis, medicinally active principles, and (some) vegetable poisons are one and the same, referring to substances of intoxication such as opium. This is to suggest that to be intoxicated is to be poisoned. More specifically, Oken says that "Pflanzengifte führen das Thier auf die Pflanze zurück" (Oken, 1810, 340).[43] The animal nervous system is overtaken by a disruption in the galvanic polarity before finally the animal's senses are pulled into a stupor.

It is clear that Oken's concept of poisoning serves as a model for intoxication. Consistent with the Paracelsian adage that "allein die Dosis macht dass ein Ding kein Gift ist," the

[43] […] "vegetable poisons return the animal to the vegetative state."

separation between medicinally active plant principles, alkalis, and dangerous poisons are largely matters of degree for Oken (Paracelsus, 1538/1965, 510).[44] After all, vegetable and animal poisons harm or help the subject by intervening in the balance of galvanic polarity, thereby undermining the vital process itself. On this point, Oken offers two possible modes of action "Neutralieren oder übermäßiges Polarisieren" (Oken, 1810, 340).[45]

Oken provides very little by way of direct explanation of the activity that gives rise to the either poisoning through neutralization or excessive polarization. He does, however, identify animal poisons, and particularly saliva, as blood-poisons. Saliva, which Oken likens to all animal poisons, poisons organic substances through equalization of their galvanic polarity and thus neutralization of the organic substance into mere infusorial mass (Oken, 1843, 350-351). This leaves vegetable poisons, including substances of intoxication, to account for poisoning by means of excessive polarization.

As for Schelling, the potent effects of substances of intoxication are a direct reflection of their latent galvanic potential. Opium and other stupefying plant alkalis affect the nervous system not by neutralizing bi-poles but by pushing the state of polarity to an extreme, by overcharging its galvanic potential and pushing the vital process to the point of annihilation. However concealed substances of intoxication may be by Oken's prose, the significance they held for Schelling appears to have remained largely intact. Oken functions in this story as one of many bridges between physiology and medicine, a bridge built at least initially upon the concept of vital substances and their determinative role in the body. Although Oken and the other *Naturphilosophen* were fixated on the notion of the holistic unity of the organic and inorganic

[44] […] "the dose alone makes the poison."

[45] […] "Neutralization or excessive polarization."

worlds, bodies themselves remained vital in their teleological wholeness. The Romantics did not 'discover' the biological subject. On the contrary, the "golden[e] Flut der Trauben - [der] Mandelbaums Wunderöl, und der braun[e] Saft des Mohns," as Novalis put it, themselves vitalize the living-body—the body remained a vital entity (Novalis, 1800/1988, 14).[46] Oken also fostered a number of particularly influential pupils, among them Johannes Müller, who would exercise a profound influence on course of late-nineteenth century physiology. But these details are yet to come. First, we must cover the breaking point of a quietly earth-shaking revolution from within the domains of Romantic science, one which continues to shape our world: the discovery of morphine and the dawn of the age of alkaloids.

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[46] The "golden flood of grapes-the almond trees miracle oil, and the brown juice of the poppy."

Chapter 4—A (Brief) Historical Ontology of an Alkaloid

In Search of the Principium Somniferum

Nestled around the source of the spring-fed Pader river, Paderborn was a quiet bishopric in 1804. The *Stadt* was then still a Prussian subject, passing into the hand of the French vassal *Königreich Westphalen* in the years between 1807 and 1813. This provincial locale would seem an unlikely setting for one of the 19th centuries most transformative developments, and yet it was. It was here that a young pharmaceutical chemist by the name of Friedrich Wilhelm Adam Sertürner would isolate morphine from opium, simultaneously giving rise to a novel classificatory scheme that included "the most important of all the substances of the *materia medica"*: the alkaloids (Hay, 1882/1883, 719).

The novelty of morphine's debut on the world stage was not merely a matter of historical precedence. It was the very consummation of the concept of the "active principle", the theory that the effects of medicinal plant and animals stem from the ingestion of a particular substance, or principle, which can be distinguished from its source material. For one, this was the genesis of an uncountably large category of heretofore unrealized scientific objects, engendering the mobilization of pharmacy into a field of sincere experimental inquiry. In addition to this, the discovery of morphine, and with it the active principle concept, fundamentally altered the viability of the extant classificatory systems of medicines and their corresponding theories of action. Classification of plant therapeutics on the basis of external characteristics becomes more complicated when the "active" plant principle can be extracted, distilled, and purified, thereby erasing the origin plant itself within the equation.

In 1805, the German pharmacist Friedrich Wilhelm Sertürner (1783-1841) isolated a precipitate from opium for the first time, identifying it as the primary active ingredient (Patil, 2012, 165). In a later 1816 paper, Sertürner would not only claim to have extracted the active constituent of opium in the form of what he called "morphium," but he would also imply that the substance likely belonged to a new class of organic matter. "By the use of ammonia," Sertürner rendered a precipitate "from an aqueous opium extract," which he could then "crystallize from ethanol" (Phillipson, 2012, 4). He published an article on his peculiar findings titled "Darstellung der reinen Mohnsäure," with little impact (Sertürner, 1806). After the findings were repeated and published again in 1816-1817, the subsequent recreation of Sertürner's method led Pelletier and Caventou to the isolation of "quinine, strychnine, veratrine, caffeine, and emetine" (Patil, 165). This was the beginning of something immense.

Yet, "before 1817 the existence of alkaloids was scarcely suspected" (Lesch, 1981, 305). Prior to the emergence of the alkaloid as a kind of thing to be, the rise of experimentalism had normalized the expectation that traditional therapeutics achieved medicinal validity through a process of empirical investigation. Concurrently, new discoveries in plant chemistry defined the conditions under which the investigation of plant principles would take place. There is likely no single field of scientific inquiry which contributed more to the appearance of alkaloids at the beginning of the nineteenth century than plant principle analysis, a topic of significant import at the end of the 18th century, mostly in France and England. In the decades preceding Sertürner's publication, the work of diligent experimentalists in plant analysis produced a wide array of methodologies for classifying various plant components. Despite productive efforts, Antoine Fourcroy (1755-1809) and his contemporaries complained about the lack of understanding concerning plant chemistry and dreamed of narrowing the gap between natural history and chemistry (Klein and Lefèvre, 2007, 240). One of the principal difficulties faced by researchers in the field, which likely impeded the sense of steady progress, was a complete lack of agreement regarding how plant principles were to be separated.

At the most basic level of classification, it was generally agreed upon that organic substances were distinguished from inorganic substances. From there, Fourcroy systematized plant principles according to those components which were separable without chemical alteration (Foye et al., 2002, 2). Keeping with his mentor Bucquet, "Fourcroy distinguished between the common 'humours' or 'sap' of plants, and particular juices" (Klein and Lefèvre, 2007, 240). He then "further distinguished juices separated from plants 'by mechanical means' without any alteration, which he defined as their 'proximate principles'" (Klein and Lefèvre, 2007, 240). Thomas Thomson, for comparison, created four classes of plant principles, of which three "were defined primarily on the basis of the relative solubility of their members in water, alcohol and ether" and the fourth according to the presence of trace minerals (Lesch, 1981, 307).

Evidently, the nature of plant bodies and their components was available as object of consideration and negotiation, but "whatever the basis of classification, plant alkalis did not appear as a category" (Lesch, 1981, 307). Of course, the existence of plant acids was already accepted: Scheele identified citric and malic acid in the wake of Lavoisier development of the oxygen theory of acids back in the 18^{th} century, finding that natural plant acids played a significant role in plant chemistry (Kremers & Sonnedecker, 1986, 359). The question faced by those studying plant acids was no longer if they existed, but their nature, their function, and their role in the rightful categorization of basic plant principles. In spite of this, there is little to no speculation on the possible existence of plant alkalis in the period before 1817. For all of the energy churned into the careful consideration of the nature of plant bodies and their structure, for all of the variety in the approaches taken to the separation of plant principles, there was no space for the idea of plant alkalis. Thomson "discussed alkalis as a group only under mineral chemistry," suggesting that an alkali presence in plant materials stemmed from mineral contents absorbed from the environment rather than a distinct plant principle (Lesch, 1981, 307). Potash seemed a probable explanation, as it was believed that plants could readily take up potash found in the soil.

Thomson's assumption makes one thing overwhelmingly clear: within the thinking at the turn of the 19th century there is no apparent rational necessity that organic and inorganic chemistry have an analogous relationship. Even if the relationship between acids and bases in mineral chemistry could be given analogous consideration, this relationship did not translate into the reality of plant bodies. Such notions rubbed against the Romantic principle, just discussed, of the originary unity of all matter, and the corresponding symmetry of Being. However, this assumption went well beyond a skeptical empirical attitude, which refused to recognize plant alkalis on grounds of insufficient proof. There was, after all, evidence of an alkaline presence in plants. In the face of such contradictory results, scientific chemists could point "to the work of Thenard and Chevreul that had shown that many plant bodies could enter into close combinations with acids" (Lesch, 1981, 319). The widespread consensus on the importance of Thenard's and Chevreul's work, and the subsequent black boxing of the "fact" it supported, provided further grounds on which to disregard the consideration of a substance's taxonomic identity on the basis of its perceived acidity or alkalinity. It was not that this was the work of poor scientists, or sloppy chemistry. The question of whether plant alkalis could or could not exist was simply not eligible for consideration within the ruling domain of the rational. The conceptual foundations necessary to make possible the question of whether or not plant alkalis existed were simply not there. Pivotal to understanding this claim is a consideration of the basic *a priori* structures that supported the epistemological validity of competing systems of plant principle classification.

18th century research into plant principles operated within its own distinct series of historically structured classification schemes. The specifics of these classification systems were still fairly fluid at the beginning of the nineteenth century. Plant principles could be differentiated according to what was observable in a plant's "natural" state (meaning unmanipulated), as Fourcroy attempted, or according to their solubility in water and other solutions, as Thomson argued. Debates would rage over the proper means of classifying proximate plant principles (Klein and Lefèvre, 2007, 241). While these details were more or less up for negotiation, the most foundational assumptions which underlay these classification systems enjoyed almost unanimous agreement amongst those involved with plant principle analysis in France and England. In Germany, Friedrich Gleb, Rudolph Vogel, and Johann Wiegleb raised their voices in support of the French model, decrying the Romantics and *Naturphilosophen,* and agreed upon the essential difference between organic bodies, meaning plants and animals, and inorganic bodies, meaning minerals (Klein and Lefèvre, 2007, 241). In England, this sentiment was shared by William Lewis and in France by Macquer, Demachy, Bucquet, Derosne, and Fourcroy (Klein and Lefèvre, 2007, 241). Whatever the system of plant analysis, every approach was foundationally structured according to the assumption of a fundamental, natural distinction between organic and inorganic matter. It would have seemed irrational, even absurd, to overturn this basic notion and consider an equivalence between the chemistry of life and the chemistry of matter.

Although the rise of Romanticism in Germany had done a great deal to advance the argument that there was in fact a fundamental similarity that underlies the structures of the organic and inorganic worlds, by the time of Sertürner's surprise appearance conventions in the chemical classification of organic substances, and the theories that supported these classifications, were still primed to resist Sertürner's suggestion that the substance he would call "morphium" was a salifiable base. Prior to the isolation of morphine from common opium, there is evidence of an unconscious resistance to the possibility of plant alkalis. Why might this be and how is it overcome? It is very clear that the approach taken to the analysis of plant principles and their chemistry was conditioned by the assumption of a fundamental difference between organic and inorganic materials. The theoretical assumption of this difference determined the range of rationally permissible classification schemes in such a way that the chemical relationships seen everywhere in mineral chemistry would not easily be assumed in the case of plant principles. This can be seen in the work of some of Sertürner's predecessors and contemporaries.

Sertürner was not the first to isolate a substance from opium and declare it the *principium somniferum*: Derosne, Seguin (École Polytechnique), and Vacquelin (Fourcroy's assistant from 1783-91) each yielded white, odorless powders from opium, but failed to give it a proper name or identify it as a salifiable base (Hay, 1882/1883, 719). Derosne, as discussed, might have isolated morphine before Sertürner and even published a paper on the subject in 1803 ("Mémoire sur l'opium") which was translated to German in 1804 ("Über das Opium"), but failed to name or classify his substance (Hay, 1882/1883, 719). Derosne recognized the alkaline quality of his precipitate, but overlooked this anomalous consideration because his primary concern was the isolation of the *principium somniferum* (the soporific principle) (Derosne, 1804). Seguin had also isolated a similar type of substance from cinchona bark, while Vacquelin, among the first to suggest singular substances might account for the effects of medicinal plants, produced an isolate of daphne alpina. Derosne and Sertürner both isolated their substances in the form of a precipitate from an aqueous opium solution and both turned litmus paper blue-green, suggesting the presence of an alkali (Phillipson, 2012, 4). Derosne simply could not, or would not, concede to the existence of a plant alkali; it had to be potash contaminant. Contrasting Derosne, Sertürner "drew the conclusion that the basic nature [of the precipitate] was an integral property of the substance," despite his editor's protestations (Phillipson, 2012, 5).

The scope of Sertürner's work and research expands considerably in the transition from his relatively novice 1806 publication to his 1817 paper. In his 1817 publication, Sertürner does a comparison of the crystalline structure of his precipitate and that yielded by Derosne. In differentiating between the structure of the crystals formed by both substances, Sertürner puts forward the suggestion that Derosne's isolate was contaminated by meconic acid, thereby accounting for his failure to identify morphine as a plant alkali (Sertürner, 1817). Years later, it was decided that Sertürner was wrong about Derosne: Derosne's *Sel Nacrotigue de Derosne* was found to be a different alkaloidal constituent of opium, having no recognizable narcotic effects (Kapoor, 1995, 14). Then so confident in his findings that the protestations of his editor and friends could not dissuade him, Sertürner's 1817 publication moves away from the agnosticism of his 1806 claim to firmly make a case for the discovery of "das neue Pflanzen-alkali" (Phillipson, 2012, 5).

This hesitance on the part of chemists, pharmacists, and medical experimentalists baffles the modern mind. Derosne's shortcoming was not the result of any methodological misapplication. On the contrary, it becomes clear that Derosne's method was more than sufficient to achieve Sertürner's results. This pattern of encountering strange substances in the process of plant principle analysis and uneasily forcing them to fit within the pre-existing categories pervaded the pre-1817 isolations of active plant principles. In this case, it appears that chemical "language was insufficient to describe the new reality" (Lesch, 1981, 12). Here the language of the scientist succeeded in describing their world: the existence of plant alkalis already excluded on a theoretical level, language rightly described the world to correspond with the basic conditions of rational viability.

If there was a discontinuity to be found here, everything seemed to point toward an error on the methodological level. The assumption of a potash contaminant affecting Derosne's results was not a product of any demonstration of the presence of potash, nor was it a negative heuristic erected in defence of a primary hypothesis. The detection of alkalinity being attributed to potash in the plant body was an *a priori* judgement derived from established theory, part of the background noise of theory and methodology that makes any claim about the natural world possible. In this sense, the anomalous alkaline white precipitate only becomes anomalous retroactively.

Sertürner the Romantiker

How is it then that Sertürner came to a different conclusion? One explanation championed by Reinhard Löw, and very much in line with a familiar notion from Thomas Kuhn's *The Structure of Scientific Revolutions*, suggests that Sertürner's youth made him more susceptible to the influence of the idea of universal polarity (Löw, 1977, 310; Kuhn, 2012).[47] Löw's argument suggests that the dynamic polarity that the *Naturphilosophen* understood to lay at the very foundations of nature, that was cause for such inspiration on the part of Ritter, Schelling, Oken, and many others, led Sertürner to consider that plant alkalis ought to exist if

[47] This is in reference to Thomas Kuhn's suggestion that it is often younger scientists, less integrated into the community of the current paradigm, whose research engenders paradigm shifts.

plant acids did as well (Löw, 1977, 310). John Lesch, however, finds the evidence behind this claim to be lacking. Perhaps rightly so, as there is no mention of polarity in any of Sertürner's writing. If he was familiar with the concept, it does not appear to be foundational to the arguments presented in any of his publications. Instead, Lesch proposes that, as a practical pharmacist, "[Sertürner] was not committed to an exclusive distinction between organic and inorganic," freeing him up to consider the results from a different perspective (Lesch, 1981, 319).

Of course, this narrative has its own problems. Sertürner's 1806 publication "Darstellung der reinen Mohnsäure" does not concede to the existence of a new class of alkali plant principles, but rather remains true to the findings of his French contemporaries in remarking that his isolate was simply alkali-like (Sertürner, 1806). There is, furthermore, evidence to suggest that, despite having a less formal education in chemical theory, he was acquainted with the work of some of his contemporaries (Sertürner, 1806; Schmitz, 1985, 65). Interestingly, Sertürner was initially unaware of Derosne's work and it is only after encountering Derosne's paper that he makes the claim that morphine is an alkaline base in his 1817 publication "Über das Morphium" (Sertürner, 1817). Though, the case can be made that Sertürner believes his precipitate to be a true alkali in 1806, as among his concluding remarks is the suggestion that similar substances might be at play behind other poisonous or medicinal plants as well as alkali plant principles (Sertürner, 1806). While Lesch's argument is compelling, it ultimately performs one of the historian's cardinal sins in seeing too much of the future in the past. It is all too easy from a modern perspective to perceive the discovery of alkaloids as a rebuke of vitalism, even a crucial one, but this was not the context in which Sertürner conducted his experiments. On the contrary, Sertürner work testified to the reality of vitalism. Even though the classificatory distinction between organic and inorganic matter arose on the basis of vitalistic concepts, it was fully possible in early 19th century Germans to uphold vitalism and advocate for an allegorical similitude between all physical substances. For, Sertürner was not only a chemist but a Romantic chemist.

Friedrich Sertürner's knowledge of chemistry was largely self-taught, entering the profession of pharmacy as both a means to hone his craft as well as a way to make ends meet. In the German world of the 18th and early 19th centuries, "the only possibility young chemists and pharmacists had of doing experimental work in a laboratory was in a pharmacy" and this seems to be the rationale behind Sertürner's own choice of profession (Schmitz, 1985, 63). Early on, it is clear Sertürner was inspired by the work of Erfurt pharmaceutical chemist Christian Friedrich Buchholz, who had been educated by his uncle Wilhelm Buchholz, a Romantic philosopher and science advisor to Goethe (Partington, 1962, 581). Being no older than 22 at the writing of his first publication on opium's somniferic principle, it comes as no surprise that Sertürner's initial experimental approach and writing style is heavily indebted, even derivative, of C. Buchholz's own publications (including his work on opium). Sertürner's first publication on opium was actually a cursory text in Trommsdorffs *Journal der Pharmacie für Aerzte, Apotheker und Chemisten*, titled "Säure im Opium" (1805). "Säure im Opium" is both stylistically and methodologically similar to Buchholz's 1800 "Versuche die Zerlegung des Opiums," Buchholz's own experiment in identifying the volatile narcotic principle in opium (Sertürner, 1805; Buchholz, 1800; Maehle, 1999, 190). Sertürner's now famous "Darstellung der reinen Mohnsäure" also appears to proceed from Buchholz's instructions in "Versuche die Zerlegung des Opiums" regarding the separation of plant principles.

The place of Christian Buchholz in the mind of the young Sertürner provides key context concerning Sertürner's own scientific perspective, especially as a self-taught chemist. Between

1820 and 1826, Sertürner would identify himself with the project of *Naturphilosophie* in "System der chem. Physik" (1820/22) and "Annalen für das Universalsystem der Elemente" (1826). Who is to say how early Sertürner began to personally identify with *Naturphilosophie,* but many of the foundations of this position can be traced back to Sertürner's earlier interest in Buchholz. Though Buchholz never openly donned the moniker of *Naturphilosoph* himself*,* his work, his perspective, was unquestionably intertwined with those of the Romantics and the *Naturphilosophen.* Buchholz's influence is not the only evidence of Romantic and naturphilosophic influences on Sertürner's early years. Around the time his earliest work on opium was published in 1805/6, Sertürner had also participated in a competition held by the Institut de France on the phenomena of galvanism, a topic of extreme importance to *Naturphilosophie* as well as their varied conceptions of "vital substances" (Schmitz, 1985, 62). This is all to say Sertürner was not merely some brilliant young mind who, left to his own devices in a backwater bishopric, happened upon one of modern medicine's greatest discoveries. He was evidently aware of, if not directly involved in, the discussions surrounding *Naturphilosophie,* even from the earliest stages of his career.

At this point, it becomes possible to understand why morphine's genesis as a scientific object occurred in Germany between 1805-1816 rather than in France with Derosne. The entity in question, the substance now known as "morphine", was likely present in the opium Derosne used in his experiments. The experimental apparatus and its associated methodology was almost identical in the cases of Sertürner and Derosne; the underlying chemical principles involved in the extraction method were not up for debate. The possibility of a narcotic plant principle being the cause of opium's physiological effects was up for candidacy for truth or falsity, so the existence of the entity in question was theoretically permissible. In this respect, Derosne was enormously successful: he extracted a white crystalline substance from opium which he believed to be the essential "salt" of opium. The case can thus be made that it was ultimately Derosne who first identified an "active principle" in the field of plant principle analysis, even if he fell short of identifying his precipitate as a distinct substance, a new *kind* of thing in the world. But when it came to the alkalinity of this "essential salt of opium" it was the particular theoretical foundations of Deronse's scientific encounter with opium which precluded him from understanding it as a base.

Derosne's barrier was a matrix of historical *a prioris*, things which, though not strictly physical beings, are inextricably rooted in a time and a place, through institutions, books, social ties, etc. The categorical exclusion of plant alkalis as a possible thing to be, or even be taken up as a candidate for consideration as true or false, is inextricable from the profoundly institutional influence of Parisian chemistry on a gentleman pharmacist like Derosne, who had been brought up in the field by his famous cousin Louis-Claude Cadet de Gassicourt. Meanwhile, Sertürner's pharmaceutical practice in Paderborn could not have been further from Paris's community of lively chemical research. The scientific atmosphere of Germany was more accommodating of a free-flowing, Romantic blurring of categorical distinctions. Hence, when entities, experimental apparatuses, and theoretical frameworks gathered in Paderborn, Sertürner was open to the possibility of plant alkalis, while also acknowledging the novelty of this claim.

It was Sertürner's famous 1816 publication, "Ueber das Morphium als Hauptbestandteil des Opiums," that actually saw the genesis of alkaloidal chemistry. In many ways this paper was a rehash of the paper published 10 years earlier, the primary difference being Sertürner's willingness to see through conclusions which he had earlier only alluded to, aided by the inclusion of further experimental methods. It appears that, outside of the apparent rhetorical necessity of mustering the most current methods, many of the changes to the overall methodology of Sertürner's earlier paper serve to directly usurp, as well as sublate, Derosne's work into his own. One such method is Sertürner description and illustration of the crystalline structure of his precipitate. Sertürner eagerly points out that Derosne included a description of the crystalline structure of his own precipitate, which "schiest dagegen in prismnatischer Form unter einem Winkel von 30 bis 40 Grad an" (Sertürner, 1817, 64).[48] This is markedly different from Sertürner's description of pure morphine crystals as "ganz farbenlos und in ganz regelmäßigen, horizontalliegenden Parallelepipdenden mit schrägen Seitenflächen," turning litmus paper blue (Sertürner, 1817, 64).[49] After reviewing the appearance of morphine's appearance in the form of various acid salts, Sertürner concludes that Derosne had not succeeded in extracting a pure extract of morphine at all (Sertürner, 1817, 67).

An interesting aspect of Sertürner's painstaking analyses of the varied structure of morphine and its salts is how little it brings to bear on the search for the *principium somniferum.* It demonstrated that there was a difference between what he and Derosne described, as well as allowed later researchers to know if they had reproduced Sertürner's results. Here too, in determining the physiological effects of his precipitate, Sertürner discerned himself from Deronse. Where Derosne tested his product on animals (with no narcotic effects of note), Sertürner disparaged the value of animal tests in ascertaining the effects of a pharmaceutical product (Derosne, 1804; Sertürner, 1817, 68). Instead, Sertürner enlisted the help of three teenage boys to partake with him in a bout of dangerous self-experimentation. Sertürner and the

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[48] […] "shoots to one side in a prismatic shape at an angle of 30 to 40 degrees."

[49] "…completely colorless and in completely regular, horizontally lying parallelepipeds with sloping sides"

three teenage boys consumed a dose of half a grain of morphine in diluted alcohol, followed 30 minutes later by an additional half-grain and a further half-grain 15 minutes thereafter. The effects were so pronounced that by Sertürner's own account he soon worried for the mortal well-being of both himself and the young men, forcing them all to quickly down strong vinegar to induce vomiting (Sertürner, 1817, 69).

Harrowing and ethically dubious as Sertürner's account may be, it is in this process of self-experimentation that the vital substance concept rears its head. Just as the influence of intoxication as a form of tacit knowledge was allegedly the inspiration behind the Brunonian system and evidently influenced its reception amongst the Romantics, Sertürner's experience with *Morphium*, the god of dreams, takes centre stage during the most crucial step of Sertürner's analysis. Here, this tacit "feeling" of intoxication is so much more than mere feeling; it is a form of knowing, of understanding—it imparts information about the world and the body in that world. The very first words Sertürner musters in recounting morphine intoxication, beyond a flushing of the eyes and cheeks, is that "die Lebensthätigkeit schien im Allgemeinen gesteigert" (Sertürner, 1817, 69).[50]

No more than a few words. Yet, in the context of the 1810s in Germany (coupled with Sertürner's Romantic and naturphilosophic background), it becomes perfectly clear that Sertürner understood the newly realized morphine to at least have vital "effects". This is further demonstrated by the battery of experiments Sertürner conducted on the galvanic properties of his novel precipitate, a major point of discussion in the naturphilosophic understanding of opium's vital properties (recall, for a moment, Schelling's discussion of opium's galvanic properties). It is unclear the extent to which Sertürner was exposed to a "learned" conception of vital substances,

[50] "The vital activity seemed to increase overall."

as most of his work on naturphilosophic theories of science were written later in Sertürner's career. Nevertheless, the first time in history that a morphine isolate was consumed by a human being it was associated with a vital response in the body: morphine was a vital substance.

When Sertürner, opium, reagents, solvents, and pharmacy glassware gathered in the setting of Romantic Germany in the mid-1810s, a novel kind of thing in the world edged its way onto the horizon of scientific study. Sertürner testified on the nature of this new scientific object with his own body. Contrary to the arguments made by Lesch, this gathering of entities saw the emergence of morphine not in spite of vitalism but because of a rudimentary belief in the fundamental unity of worldly phenomena on the basis of an organic unity, of which the concept of vital substances was an active component. Morphine, the first time it was identified and named for the god of dreams, passed onto the horizon of scientific consideration as a "vital substance".

What does it matter how Sertürner understood morphine, or the emergent class of alkaloids, as alkaloids are entities about which our knowledge is constantly evolving? There are what we will provisionally call upper tier theories. They're only upper tier because of their historical prominence in the work of identifying a given theory's candidacy for truth or falsehood. There are also what might be called second, third, etc. tier theories which are also attached to a scientific object, but are less prominent. All of these designations are in a state of constant flux and are specific to each encounter with a given scientific object. Now, many of these lower tier theories are in every sense subject to ruptures in the prevailing domain of reason, passing out of candidacy for truth or falsehood. There are also far more amorphous, almost unrecognized, associations. All of these theories and associations persist in our relationship with scientific objects because scientific objects are entities themselves, characterized by a nigh irrevocable "thingness" that presses up against our theoretical encounters with them. For the most part, these associations can be traced to the emotive and perspectival reality of embodiment and have a funny way of clinging to our perceptions of the world. They persist in light of their discreet emotional significance to our experience as individual subjects. This is also why these associations rarely apply to theories in a historically significant way. But this is the peculiar thing about intoxicants: they intercede in our experience of embodiment; they make us feel something.

Just as the tacit knowing involved with intoxication helped establish the vital substance concept, this emotional aspect of intoxicants, which is bound up in human perception, allows the lower tier theories concerning intoxicants to persist in the form of associations. This is owed to the unique relationship between the embodied and intoxicants, which complicates the expected nature of one's encounter with intoxicants as scientific objects. To return to the case of Sertürner, the simultaneous generation of the novel class of plant alkalis and emergence of morphine represents a turning point in modern history. If later scientific encounters with intoxicants carried an association derived from the vital substance concept, morphine's emergence is a prominent bottleneck. This is particularly because the emergence of morphine was not a singular development in the history of pharmaceutical chemistry, but the spark behind an explosion in the number of known alkaloidal medications and intoxicants that would send shockwaves throughout the nineteenth century.

Once presented for consideration in the form of Sertürner's 1817 paper, Guy-Lussac, the French chemist, had little choice but to consider not merely his immense discovery of the *principium somniferum* but all of Sertürner's findings, leading many chemists, such as Vauquelin, to retroactively claim the discovery for themselves (Jurna, 2003, 281; Bache, 1980, 490). The first effort to formally define alkaloids as a class of things was put forward by Carl

Friedrich Wilhelm Meißner in 1819. In the aftermath of Sertürner's 1817 publication, Meissner isolated sabadillen and coined the term "Alkaloide" to distinguish plant alkalis from mineral alkalis (Phillipson, 2012, 5; Fattorusso & Taglialatela-Scafati, 2008, 74). Many competing definitions soon entered the fray. Chemists like König reserved "the name alkaloid for plant bases related to pyridine," while others, in keeping with one of Sertürner's observations, saw nitrogen as a necessary component (Fattorusso & Taglialatela-Scafati, 2008, 74). Interest in defining plant alkalis as a novel category of thing directly corresponds with the exponential increase in the number of newly identified alkaloids. Robiquet identified narcotine (1817) and codeine (1832), Caventou and Pelletier isolated strychnine (1818) as well as caffeine and quinine (1820), Meissner and Caventou found ceratrine (1818) and colchicine (1819), nicotine by Posselt and Reimann (1828), Woskresensky recognized theobromine in 1842, and Niemann isolated cocaine in 1860, just to list a few (Huxtable & Schwarz, 2001). The collective impact of these discoveries is almost inconceivable. Quinine alone may be responsible for turning the tide in European efforts to colonize India and Africa (Porter, 1999, 465). New alkaloids burst onto the world, making fortunes and conquering continents, bodies, and minds. But, of course, this was barely the beginning. The vital substance concept would soon meet its match in a revolutionary shift in the study of the body and, with it, a change in the meaning of embodiment.

Chapter 5—Great Expectations: the Humboldts, Johannes Müller, and the Rise of Neo-Mechanism

Berlin and the Modern University

While leaps and bounds were taking place in the discovery of novel alkaloidal plant principles and new intoxicating medicines stepped on to the world stage, hints of a comparable shift could be seen in the budding sciences of the body. Throughout the first half of the 19th century, the world of German physiology was still a messy place. More so than medicine, physiology was empirical; however, it still relied on theoretical and philosophical frameworks in inferring the clinical significance of its findings. Though it arguably stood on drier ground, it lacked the systematicity required to identify itself as a proper science, and was thus mired in the very same morass as the other aspiring German sciences of the 19th century. The ensuing revolution would make names for some of the 19th century's most widely lauded figures: Helmholtz, Du Bois-Reymond, Meynert—all of whom rose on the shoulders of their teacher and mentor: Johannes Müller. It would be their legacy that set the stage for the pioneering efforts in the study of the body and mind that took hold in the middle of the 19th century, fundamentally transforming not only how the body was studied, but understood. But such dramatic developments can hardly be attributed to the sweat of any one person's brow. The neo-mechanist revolution could, arguably, just as readily be identified as the product of organizational changes in the German university, political pressure, or the influence of outside benefactors. Of all the individuals who contributed to the circumstances that made possible this shift in the physiological sciences, few are as significant as the Humboldt brothers.

The hands of Alexander and Wilhelm von Humboldt both lay heavily upon the 19th century development of the science of the body, although the nature of the influence each brother exerted outwardly could not have been more different. Alexander von Humboldt canoed the Amazon and scaled Mount Chimborazo. Wilhelm von Humboldt was a sober-hearted linguist, educator, and government functionary. Unsurprisingly, Wilhelm's contribution to history of the body and its study was institutional in nature. In 1801, the Prussian cabinet began planning the development of a new university in Berlin, which at the time was almost devoid of scientific learning (Rüegg, 2004, 16). The task fell on Wilhelm von Humboldt who, with the assistance of Ferdinand Schleiermacher and Johann Gottlieb Fichte, established the University of Berlin in 1809 (Rüegg, 2004, 16). Located in an eighteenth-century palace, the newly founded university was to embody the *Einheit von Forschung und Lehre* (unity of research and education), a true centre of cutting-edge inquiry into the hidden wonders of the natural world (Rüegg, 2004, 16). Research and education in the sciences were the focus of the new university from its very inception. These high ideals were well suited to the Prussian academic reforms initiated back in 1799, which restricted academic rights to graduates whose Master's or doctoral thesis was recognized by the scientific community (Klinge, 2004, 137). This emphasized independent research as a core value of a university educator and promotion within the academic hierarchy increasingly depended on the publication of new research. The University of Berlin also instituted the *Habilitation*, a piece of independent, post-doctoral scholarship that was a required to attain the status of a full professor (Klinge, 2004, 137).

With the University of Berlin as its proven model, "the German university system allowed scientific research to be a professional, bureaucratically regulated activity" (Rüegg, 2004, 17). The German university system also gave rise to the first professional scientists. By the middle of the nineteenth century, "practically all researchers in the natural sciences and medicine in Germany were active either as heads or collaborators of institutes or university laboratories" (Rüegg, 2004, 17). This development was largely a product of the emergence of scientific research as a new type of profession embedded in the structure of the university. Another entailment of professionalization, this was also the period in which the inter-university academic conference and the academic journal both gained popularity as a means of sharing and consolidating new knowledges (Klinge, 2004, 130. To draw a comparison which illustrates how transformative this was for German science, French physiologist Claude Bernard made many of his discoveries in a cellar, while Louis Pasteur did experiments in a pair of attics (Rüegg, 2004, 18). This is not to suggest that one should expect a greater share of scientific insights to emerge out of institutional, academic science. What the emergence of the modern research university produced was a space wherein local encounters were collected and “legitimized”, having passed through the crucible of professional science. Wilhelm von Humboldt was instrumental in the development of the university as the privileged, if not exclusive, site of knowledge creation and translation.

If Wilhelm von Humboldt is to be credited with paving the way for organized academic science, Alexander von Humboldt is rightly recognized for doing much to lead intrepid young minds on to such a path. Alexander von Humboldt had been a naturalist, botanist, and explorer in his own right. He had voyaged around the globe, describing all manner of phenomena, and pioneered fields of science such as biogeography, meteorology, volcanism, and geomagnetism, to name but a few (Jenkins, 2007; Echenberg, 2017). From his home base in the Prussian capital of Berlin, Alexander von Humboldt also enjoyed profound influence on the Prussian court, occasionally serving as a foreign emissary representing the Prussian crown. Rather than marshal his favour is service of personal enrichment or political intrigue, Humboldt exercised his influence to promote Prussian influence in scientific discovery. This could be as direct as

Humboldt personally reading a draft written by a given protégé to the Prussian King (Finger et al., 2013, 28). In the case of Johannes Müller, it was the enthusiastic support of Alexander von Humboldt that encouraged him in his work. This mentorship spanned Müller's early days in Berlin to his latter days as a widely lauded, seasoned professor, with a cohort of protégés all his own. When Müller died in 1858, it was Alexander von Humboldt that rallied to raise funds for his widowed spouse. Supported by the Humboldt brothers, Berlin would become one of the world's greatest centres of physiological study of the 19^{th} century, largely due to Müller's great commitment as an educator and his tremendous productivity as a synthesizer of knowledge.

But Müller, as will soon be discussed, was more of an end than a beginning—a transitional figure between the dynamic vitalism of the *Naturphilosophen* and the neo-mechanists, who sought to reduce the body to physio-chemical processes. For the purposes of this story, Müller will simultaneously be a primary instigator in the shift towards an approach to the science of physiology which singularly privileges anatomical and experimental investigations, as well as a forceful advocate for the experimental validity of a post-Okenian understanding of the vital substance concept. And it will be precisely this dual significance of Müller's that will make his ideas the object of alterity at the hands of his physio-chemically-minded students. For, they, intent on overcoming the perceived shortcomings of their mentor, will seek to undermine every remaining vestige of the theory of vitalism, and, with it, the vital substance concept. As far as the history of intoxicants and the body is concerned, the effort on the part of the neo-mechanists to divest scientific physiology of the vital substance concept will result in a broader hesitance to study the effects of intoxication at all. By the time that their students are entering into the scientific community, among them the "brain-psychiatrists" Eduard Hitzig, Theodor Meynert, and Karl Wernicke, research on the effects on the mind would only be conceivable as an investigation in neurophysiology. The anatomical body would become the sole locus of the real concerning a meaningful understanding of the body and mind. This is crucial, for it will be their failure to reduce mental processes to brain matter that gives credulity to a novel approach to understanding the nature of the mind.

Johannes Müller

Prior to Johannes Müller's appearance on the world stage, German physiology was in a state of disarray, despite persistent Romantic efforts at systematization. Oken had brought a fresh empirical spirit, but the theories underlying physiological relationships between anatomical features remained reliant on speculative principles. Medicine and pharmacy had seen a flurry of development, and, yet, many of these changes had only raised more questions. But this was all fertile soil for Müller's calls for an approach to physiology as a science that was concerned primarily with empirical phenomena, rather than grand cosmological questions. Yet, the legacy of Müller as reformer is as much a reflection of the revolutionary aspirations of his brightest students as his actual efforts to change the course of German physiological science.

Müller was the great reformer, the famous father of scientific physiology. He was also a vitalist with Okenian impulses and a toe or two in the 18^{th} century. It was through this heritage that Müller carried forward the vital substance concept. Despite openly disregarding Brunonian ideas, a vitalistic conception of intoxication, and intoxicants, was clear for all to see in Müller's bodily concept. Further still, Müller's role as a reformer in the field of physiology meant bringing the vital substance concept into the anatomical lab, and seeing it unfold before his eyes under the microscope. The very nature of his concept of vitalism was made clear through his encounters with intoxicants.

It would be these vitalistic, and intoxicatingly Brunonian, influences which would become a point of contention for several of his key students, among them Emil du Bois-Reymond, Hermann Helmholtz, and Ernst Brücke. For them, Müller's methodological reformation of physiology had not gone far enough. The nature of the body—indeed of the entire world—could be understood exclusively through mechanistic relationship between empirically identifiable physio-chemical processes. These "neo-mechanists" were in all-out revolt against vitalism and everything it stood upon, including the vital substance concept. They were also fabulously successful, for a time. But before discussing the theoretical shift represented in the anti-vitalistic critique levelled by the neo-mechanists and the implications for the study of the body and mind, it is worth addressing some of the institutional and societal shifts that made a so-called 'neo-mechanistic revolution' possible, before discussing the foundation set by Johannes Müller.

Johannes Petrus Müller was born in 1801 in French-occupied Koblenz. After a year of military service, Müller began studying medicine at the University of Bonn in 1819 (Zimmer, 2006; Otis, 2007). These studies concluded in 1822 upon the acceptance of his thesis on the motion of several animals (*De Phoronomia Animalium*) (Zimmer, 2006; Otis, 2007). Müller continued his education in 1823 when he received a scholarship to study comparative anatomy with Carl Asmund Rudolphi in Berlin, who had also been a mentor to Moritz Romberg (Otis, 2007; Zimmer, 2006). His early research, while centred around topics within the scope of physiology and anatomy, were more speculative and philosophical in style, given the enduring impact of Lorenz Oken. It was not until Müller began researching comparative anatomy with Rudolphi that Müller committed himself to relying solely on observable phenomena in the discernment of physiological principles (Zimmer, 2006; Lohff, 1978, 247; Otis, 2007).

Müller was swift to make a number of essential discoveries in the study of bodily function. With the publication of *De glandularum secernetium* Müller established the role of glands in regulating bodily functions. In 1831, he experimentally demonstrated Charles Bell's and François Magendie's unproven theory that dorsal roots carried sensory nerves and ventral roots carried motor fibres on frog spines (Steudel 1963, p. 570). In total, he would publish 267 titles over the course of his life. From 1833 to 1844, he gradually consolidated his own research and the available work in physiology into his *Handbuch der Physiologie* (Handbook of Physiology) (Lohff, 1978, 247). During his time, Müller came to be considered the foremost expert on physiology and his *Handbuch* its primary treatise (Rachlin, 2005, 41)

The *Handbuch* was one of the most circuitous textbooks of its time, incorporating both the products of Müller's own experiments as well as bringing together disparate publications in French, English, and German (Zimmer, 2006). Far-reaching in the scope of its detail, Müller provided extensive macro- and microscopic description of various tissues, nerves, and fluids in different organisms, all of which was organized into 9 books and a lengthy prolegomenon (Müller, 1834, III). During the process of writing the *Handbuch*, Müller also founded a journal in 1834 with the intention of increasing communication within the field of physiology. Titled the *Archiv für Anatomie, Physiologie und wissenschaftliche Medicin* (Archive for Anatomy, Physology, and Scientific Medicine), his journal quickly became highly respected amongst researchers in the natural sciences (Otis, 2007). In contrast to journals started later in the 19th century which trend toward increased specialization, Müller's periodical was relatively broad in focus. It instead embodied the principles of a new approach that affirmed the importance of cooperation between anatomy, physiology, and "scientific" medicine.

Müller as Reformer/Müller and Vital Substances

For these reasons, Johannes Müller is sometimes "considered the main reformer of physiology in the late 1830s, because he was instrumental in liberating the science from the romantic spirit that had prevailed for several decades," or so has been argued (Zimmer, 2006). Such assessments focus on how Müller's "emphasized the comparative and developmental aspects in the study of physiology and stressed the importance of meticulous observation" (Zimmer, 2006). But on a theoretical level, Müller was in many ways a product of his Romantic education. Müller asserted as early as 1826 that the methodologies of physiology needed to approach the body physio-chemically, although, in actuality, physiology could never be reduced to physio-chemical processes (Müller, 1826, 19). For one, Müller's commitment to limit his approach to observable phenomena did not preclude him from remaining a vitalist, affirming the essential difference between organic and inorganic matter and lending his voice as an opponent of rigid empiricism. In 1824 Müller gave a lecture titled "Ueber das Bedürfnis der Physiologie nach einer philosophischen Naturbetrachtung" (On the Need of Physiology for a Philosophical Contemplation of Nature) (Rothschuh, 1973, 197). Here, he expressed not only that philosophical contemplation of nature was meaningless without scientific observation, but also that empirical rigour was blind without philosophy.

Beyond merely advocating of the importance of philosophical analysis, it is apparent that Müller's physiological work was deeply indebted to the likes of Leibniz, Kant, Schelling, and Oken. The clearest example of this can be found in one of Müller's most impactful insights: the law of specific sense energies. An idea that had come to guide much of Müller's work was the notion that "sensation is an attribute not of some real object out in the world but of the sensory nerves" (Fullinwider, 1991, 22). Müller's "law" was that sensory nerves were not neutral conductors: some sensory nerves represent energy from the sun as heat and some as light, but not both at the same site (Fullinwider, 1991, 22). The law of specific sense energies is first explicitly introduced in Müller's 1826 *Zur vergleichenden Physiologie des Gesichtssinnes des Menschen und der Thiere* (On the Comparative Physiology of Vision in Men and Animals). This deceptively simple idea held tremendous significance for the way the body and mind were to be understood. The law of specific sense energies not only seemed to confirm that our knowledge of an external reality was actually a reflection of the nervous system itself but it suggested that the stuff of perception, like sound or colour, were innate, rather than passively received (Fullinwider, 1991, 22).

Many of Müller's conclusions about what the rule of specific nerve energies tells us about perception mirror ideas central to Kant's philosophy of empirical psychology. Müller understood his law of specific sense energies as a physiological parallel to Kant's categories of thought (Hergenhahn and Henley, 2014, 222). Just as Kant's categories of thought are necessary for the construction of tangible, conscious experiences, Müller's specific sense energies are physiological *a prioris* which construct and shape our perceptions of an external world (Kant, B-161). While Müller provided a physiological explanation, he was one with Kant in the idea that we have no awareness of external objects as things-in-themselves (Müller, 1834). True knowledge of objects "outside" ourselves is impossible because our perception of them will always be comprised of limited inputs from the nervous system. In this respect, Kant's psychological framework and Müller physiological model were functional identical: our awareness is only of our nervous system.

Although Kant made a great impression on Müller's work, Müller was not without criticisms of Kant's conclusions. In drawing a physiological parallel between the law of specific nerve energies and Kant's categories, Müller did away with Kant's transcendental forms. At the same time, Müller acknowledged the existence of something phenomenologically akin to the unity of apperception. Ultimately, Müller felt the solution was similar to what Leibniz called 'pre-established harmony,' an idea Kant considered for himself in *Kritik der reinen Vernunft* (Critque of Pure Reason) (Fullinwider, 1991, 22). Johannes Müller suggested that each biological species had an unconscious 'ruling idea' or 'organizing principle' whose action regulated the physical and mental structure of an organism (Fullinwider, 1991, 22). Here, at the core of his physiological Kantianism Müller presents like a good *Naturphilosoph* (Finkelstein 1996, 78). Like Schelling, Müller saw the apparent harmony in the organization of extremely complex organic systems as evidence of an underlying idea that specific to each species, of which the individual was a particular unfolding. The unconscious action of this organizing idea was what Müller understood to be a 'vital principle' (Fullinwider, 1991, 23).

Müller shared in the vitalistic thinking of his day. But in Müller's case, vitalism was not merely an explanation of the difference between organic and inorganic bodies. It was a solution to fundamental questions concerning the possibilities for knowledge as embodied subjects. The vital force itself is a fact of organic life and, while speculation about its origins belongs in the domain of philosophy, experimental physiology should strive to understand the properties of the vital force (Müller, 1834, 18).

The primary distinction between organic and inorganic matter, Müller suggests, is best understood as the difference between beings which are *organized* and those which are not. This notion is explained by the existence of a purposive ruling idea at work in organic life, as Müller writes in the *Handbuch*:

> Die organischen Körper unterschieden sich nicht bloss von den unorganischen durch die Art ihrer Zusammensetzung aus Elementen, sondern die beständige Thätigkeit, welche in der lebenden organischen Materie wirkt, schafft auch in Gesetzen eines vernünftigen

> Plans mit Zweckmässigkeit, indem die Theile zum Zwecke eines Ganzen angeordnet werden, und diess ist gerade, was den Organismus auszeichnet (Müller, 1834, 18).[51]

Organisms are comprised of many separate components working harmoniously towards a common goal. Müller understood this in terms of purposiveness. Separate components, each with their own purposes, serve to perpetuate the existence of the entire organism. Furthermore, this purpose, or ruling idea, appears to guide the development of the organisms throughout its entire existence. The germ, however different in appearance, is always equitable with its more developed form *in potentia* before it realizes its completed form in *actuality* through development (Müller, 1834, 24). In this sense, "Stahl's Seele [*anima*] ist die nach vernünftigem Gesetz sich äussernde Kraft der Organisation selbst" (Müller, 1834, 24).[52]

It is the organizing principle which ultimately distinguished living and "dead" matter. Pondering the nature of this vital principle, Müller conceded that it was still impossible to describe it materially as a force or an energy, but remarked the same could be said of important phenomena is physics (Müller, 1834, 27). Still, Müller clearly understood the vital force on a physical level, describing at length the reanimation of tissues deprived of vital stimuli (Müller, 1834, 27). In this, Müller appears to diverge from his primary description of vital phenomena. Where even Oken rejected *Lebenskraft* as an animating force in favour of galvanic energies potentiated by the underlying principle of excitability, Müller appears to be making a case for a

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[51] "The organic bodies are not only distinguished from the inorganic by the nature of their composition of elements, but by the constant activity which is working in living organic matter. [This active force] works with purposefulness according to a rational plan, such that the parts are arranged for the purpose of a whole. This is what characterizes the organism."

[52] "Stahl's [idea of the soul as the animating force] is the organizational force itself which manifests in accordance with a rational law."

vital force which is at once animating and organizing. The explanation is found in Müller's reception of the concept of vital force in relation to substances of intoxication.

Müller does make reference to John Brown in his *Handbuch*, a brief mention occupying scarcely more than a couple pages (Müller, 1834, 60). He recognized Brown as an intrepid, but crude, discoverer of a number of the laws of excitability and a bold, but dangerous, progenitor of the earliest forms of scientific medicine (Müller, 1834, 59-61). However, Müller's commentary here is not solely of a historical nature, and he is quick to make a series of pointed criticisms of Brown and his followers. For one, Müller describes Brown's notion of sthenic diseases as contradictory (Müller, 1834, 60-61). All diseases, Müller asserted, are defects in the bodily composition of tissues comprising the "Organsystemen," characterized by a diminishing of vital powers as opposed to over-stimulation (Müller, 1834, 66). This, in itself, is not a particularly harsh criticism; insofar as sthenic conditions were exceedingly rare, the practical application of Brunonian theory almost always treated diseases as asthenic, with bodily exhaustion being described as a product of overstimulation. But Müller's firmest critique of Brown was a hit squarely on the vital substance concept that guided Brunonian therapeutics. Brown, Müller asserts, erred in the conflation of apparent vital stimulation with *actual* vital stimulation (Müller, 1834, 61-62). This is because substances of intoxication produce an effect on organic function, the symptoms of which create only the temporary appearance of vital stimulation (Müller, 1834, 61-62).

Müller's anti-Brunonian critiques appear to establish a conclusive break from the vital substance concept as received from Brown. Yet, Müller's extensive and diverse remarks on the influence of opium and other intoxicants on the body complicate this position. Müller's *Handbuch* frequently relays the conclusions of physiological experiments with opium, both conducted by Müller himself and found in other's work, asserting that opium stops the motion of the heart (Müller, 1834, 181 and 184), suppresses feelings of hunger (Müller, 1834, 466), and locally abolishes nervous activity in the limbs of frogs (Müller, 1834, 233). Müller even attempts to reproduce Humboldt's findings that choloric acid, opium, and alcohol (amongst other substances) have an excitatory effect at the nerve, though Müller singles out alcohol, choloric acid, and opium as halting all excitatory activity in the nervous tissue (Müller, 1834, 596). The poisonous effects of narcotics are actually a recurrent theme in Müller's *Handbuch*—above all, this seems to be a reflection of the unique effects that Müller understands narcotics to have on the body. Unlike other stimuli, the group of substances known as narcotics "alteriren die materielle Zusammensetzung der Nerven" (Müller, 1834, 607); or, as he states elsewhere, "[es] folgt, dass das Opium die Nervensubstanz selbst verändert" (Müller, 1834, 233).[53] [54]

How does this alteration of nervous material occur? Müller is very clear in stating that it is not owing to any discernible chemical or mechanical destruction of the nervous system but through the actual alteration of nervous material (Müller, 1834, 607). In regard to how narcotics enter the body in order to engender an effect, Müller remarks that they are generally taken up in the blood (Müller, 1834, 609). Under experimental conditions, narcotics can affect nerve material locally which Müller suggests is a result of the narcotic's destruction of the excitability of the nervous material (Müller, 1834). Müller mostly emphasizes the capacity of narcotics to abolish nervous activity, although it is important to note that this is not the full extent of the picture. As has just been established, Müller asserts that all narcotic substances "in grossen

[53] Narcotics "alter the material composition of the nerves."

[54] "it follows that the opium itself alters the nervous substance."

Gaben sogleich deprimirend durch Alteration" (Müller, 1834, 607-608).[55] However, "einige sind in kleinen Gabem reizend und weniger deprimirend, wie Opium" (Müller, 1834, 607-608).[56] So what is Müller really saying here? The answer casts doubt on the extent of Müller critique of Brown, or at the very least clarifies it.

By Müller's own account, opium produces excitation—it stimulates, at least up until the point that it brings about an alteration in nervous tissue. It is hard to imagine a more vivid echo of Oken's own description of the action of vegetable poison, which, as discussed earlier, bring about the destruction of the nervous system by supercharging its polarity. Müller is more agnostic with respect to understanding the technical process by which this change in nervous tissue occurs, though the effect is the same. Both Müller and Oken describe narcotics, and alcohol in Müller's case, to be initially excitatory before bringing an end to vital activity in the tissue, almost as if the nervous tissue is being over-stimulated to the point of neutralization. Müller goes as far as relating the corresponding loss of muscle receptivity at the sites where narcotics have been locally applied to the destruction of the excitability of nervous filaments (Müller, 1837, 15). Like his mentor, Müller evidently does in fact understand substances of intoxication, in particular narcotics, to negate the excitability of nervous tissue through excessive vital excitation.

Returning to Müller's remarks on the Brunonian system, Müller's position appears to be more of critique of what might be called "Classical Brunonianism," as opposed to the Romantic and naturphilosophical reception of *Erregbarkeitstheorie.* This is made clear in through Müller's Okenian understanding of narcotic poisoning, though it can also be seen in Müller's own

[55] "all when given in large doses deaden [excitability] through alteration [of nervous matter]."

[56] "some, such as opium, are stimulants and less depressive at low doses"

conception of excitability. Müller understands excitability to be the distinct property of organized bodies (Müller, 1834, 592-593). This "property" refers to the constant process by which organized bodies take up external vital stimuli to bring about changes in their bodily form in making life possible (Müller, 1834, 37). In this sense, organisms are, at once, dependent on environmental stimuli and yet irreducible to a passive mechanism. This is of course clearly derived from Oken-Schelling's understanding of excitability as interpreted through Röschlaub's *Erregbarkeitstheorie,* that excitability is not a substance but a process, a dynamic relationality that forms the basis of all vital activity.

The turn, however, was Müller's efforts to bring the vital substance concept into the anatomist theatre and validate the idea by experimental means. By taking the Schellingian-Okenian concepts of excitability and reading them through the methods of scientific physiology, the principle of intoxication as the over-stimulation of galvanic polarity was translated into the over-excitation of nerve fibrilles. Even when Müller struck out against the likes of Brown, the foundational notion of a vital stimulant had remained, albeit in a new way. That Müller received the vital substance concept from Romantic sources is clear. But intoxicants also interceded in Müller's bodily existence as well.

Müller had suffered from severe bouts of depression and infirmity throughout his life, notably in 1827, 1840, 1847, and later in 1857 (Otis, 2007, 34, 228). During his 1857 episode of illness, Müller began taking massive doses of opium, allegedly to treat insomnia and a pain in his abdomen (Otis, 2007, 228). Somewhat inescapably, the opium also perhaps mitigated Müller depression and melancholia. And it would be opium, again, that led to Müller's death by overdose on April 28th, 1858, at the age of 56. But this can hardly be counted as a singular instance of opium use in Müller's life. Sometime in the early 1850s, laudanum had already become something that Müller would recommend to those around him as a curative for a vast assortment of ailments. To this point, Du Bois-Reymond sent a letter to Helmholtz in 1853 in order to communicate Müller's recommendation that he treat his colic with laudanum (Otis, 2007, 228). While evidence of earlier use of opium products is lacking, Müller's recurrent illness, experimental familiarity with opium, and worsening use toward the end of his life suggests that Müller became well-acquainted with the intoxicant at an earlier point than the 1850s. It is very possible that Müller's own experiences with opium, and alcohol, intoxication figured into recognition of these intoxicants as excitatory, rather than merely as poisons. What is clear is that, even as he decried Brunonian ideas, Müller upheld and even advanced the notion of intoxicating vital stimulants, using the methods of physiology.

In his published works and the surviving notes on his given lectures, Johannes Müller remained committed to vitalism, and the vital substance concept, until his death in 1858. Müller mentored many young scientists over the course of his career, including Emil Du Bois-Reymond, Helmholtz, Friedrich Henle, Carl Ludwig, Rudolf Virchow, and Theodor Schwann. Emil Du Bois-Reymond and Hermann Helmholtz, in particular, would later take it upon themselves to dedicate their life's work to refuting their teacher's assertion of the fact of the vital principle. Of primary importance to their refutation was a concerted effort to erase any enduring argument for not only the vital substance concept but the empirical methodology that had permitted intoxication as a valid testimony concerning the ultimate nature of the body and mind. Even as they laboured to wipe out belief in their mentor's central idea, they remained indebted to Müller's tremendous work as a reformer, whose work directly paved the way for the research of the physiologists who followed.

The Electricity of the Body

Of all of Müller's students, it would be Helmholtz, Brücke, and du Bois-Reymond who would be most crucial in preparing the next generation to oppose Müller's vitalistic ideas, and electrophysiology one of their earliest tools. Deemed the 1847 group, they conceived of a new approach to physiology which was empirically grounded in physical and chemical principles and divested of vitalism in all its forms. Electrophysiology was still a budding concept when Helmholtz and Du Bois-Reymond began studying medicine. The phenomena of galvanism had been known since the late 18th century; the *Naturphilosophen* and the Romantics, among them Alexander von Humboldt who had written on electric animals encountered in his travels, had privileged galvanism as an important area of study. But it was Emil du Bois-Reymond and Hermann Helmholtz who would establish what might be likened to an early vestige of the modern concept of electrophysiology. An important component of their work, one which fundamentally differentiated their approach from those promoted by Müller, was the notion that the electric currents found in animals were no different than those derived from inorganic, external sources.

They would take issue with their mentor's approach on the topic of vitalism, but they acknowledged the significant philosophical implications of Müller's law of specific nerve energies. Perhaps even more so than Müller, Helmholtz and Du Bois-Reymond relied on the empirical psychology of Immanuel Kant to provide a theoretical framework for the new physiology. In Helmholtz's case, his Kantianism led him to study the physiology of perception and the human senses. Deeply inspired by Ernst Weber's declaration that vital forces were in fact physical forces, Helmholtz and Du Bois-Reymond shared a vision of developing physiology into an organic physics (Cahan, 25, 1993; Königsberger 1906, 25). This meant reducing physiology to the most basic laws of physics and chemistry in order to reconceive of the body on the basis of strict empirical observation. A founding tenant of their new approach was the complete rejection of vitalism. This would present a number of theoretical hurdles. For one, Johannes Müller identified *organization* as the primary difference between organic and inorganic matter. Searching for an empirical explanation for the appearance of organization in life, Helmholtz and Du Bois-Reymond's became enthusiastic, early adopters of Darwinism. Their efforts to develop physiology into a fully realized physical science would leave a profound impact on their students, continuing the transition toward greater centralization and institutionalization in the field of physiology.

Yet, in elevating physiological neo-mechanism, or organic physics, as an ontological reality, the neo-mechanists undermined the epistemological validity of anything but the experimental sciences. Not only that, this approach would come to manifest in an identification of bodily substance as the singular locus of the real with respect to the nature of the body and mind. By the time that Meynert and Wernicke were promoting their theories of cortical localization, this doctrinal principle of the body as the real would lead to an equation of mental states with neural states, the failure of which would pave the way for a radical new science of experiential intoxication. In order to make sense of this, however, it is important to first explore early neo-mechanism's efforts to repudiate the vitalistic, and vitally intoxicated, ideas that came before them.

Du Bois-Reymond and Helmholtz

It was under Müller's supervision, which started around 1838, that Du Bois-Reymond was introduced to Carlo Matteucci's latest work on animal electricity in 1843 and 1844 (Finger et al., 2013, 23). Müller, himself, had become aware of this work thanks to Alexander von Humboldt, whose observations on electric eels encountered during his travels in the Americas from 1799-1804 had maintained an enduring interest in the subject (Finger et al., 2013, 23). Humboldt took Du Bois-Reymond into his mentorship when he was still a student. Encouraging his work and at times even participating in Du Bois-Reymond's experiments, Humboldt's support validated the young pupil's fascination in neuromuscular electrophysiology. Most importantly, Humboldt's immense influence, as much in court as in the academic community, all but assured financial and institutional recognition of the importance of Du Bois-Reymond's research (Finger et al., 2013, 26). Under the supervision of Müller and with the support of Humboldt, Du Bois-Reymond selected electric fishes as the subject of his graduation project and by 1848 published the first section of a multi-volume work on animal electricity, a publication which Humboldt is alleged to have read to the king himself (Finger et al., 2013, 28).

From the outset, Emil Du Bois-Reymond appears to have been confident in his vision. As early as 1845, he wrote to Humboldt that "[p]hysicists and physiologists will see this dream realized of an electricity operating the movements and perhaps transmitting sensations in animal bodies" (Finger et al., 2013, 27). One factor which was especially formative of the course of Du Bois-Reymond's work was his rivalry with Carlo Matteucci. The Italian scientist had developed what he termed a "rheoscopic frog": an apparatus for detecting current which consisted of the cut nerve of a frog's leg and the attached muscle (Finger et al., 2013, 68). Functioning under the premise that excitable organic tissues generated direct current when damaged, Matteucci also developed a crude battery of frog thigh muscles which he called a "frog pile." Seeking to further understand the phenomena, Matteucci charged current from his frog pile through individual muscles and nerves, but was unable to produce a positive result. In the face of this result, Matteucci concluded that there may be a fundamental difference between currents produced by laboratory preparations and those produced by living animals (Finger et al., 2013, 68). Recognizing the great intellectual debt he owed to his older competitor, Emil Du Bois-Reymond was confident that Matteucci had erred in making this assumption and he set out to prove it.

At the most basic level, Du Bois-Reymond discredited the veracity of this conclusion because he was already convinced of the homogeneity of currents found both within and without organic bodies. Identifying faulty experiment design as the culprit, Du Bois-Reymond was able to identify the frog's charge as consisting of component charges found throughout the nerve and muscle tissue. This insight led to the discovery of the law of muscle currents (Finger et al., 2013, 69). This rivalry with Matteucci also explains Du Bois-Reymond's insistence on the use of a galvanometer to measure animal electricity. Whereas Matteucci's rheoscopic frog always left open the possibility of a type of current specific to life, the use of a mechanical device served as testimony that the electricity of organic and inorganic matter was one and the same. After studying the various galvanometers and acquiring suitable expertise, Du Bois-Reymond produced the most sensitive galvanometer of its day, the first true nerve galvanometer (Finger et al., 2013, 66).

Like Du Bois-Reymond, Hermann Helmholtz was elevated early on by the patronage of Alexander von Humboldt. While Helmholtz was serving his military duty at Potsdam, Humboldt took notice of his great intellect. Utilizing his influence as Royal Chamberlain and science advisor in the court of König Friedrich Wilhelm IV, Humboldt was able to shorten the length of Helmholtz's mandatory military service, which he had earlier taken upon himself in exchange for a free medical education (Finger et al., 2013, 24).

Helmholtz's own first independent foray into physiology also centred on electrophysiology. In 1841, Helmholtz bought a microscope with the little money he had and decided to write a dissertation under the great Johannes Müller. He undertook a detailed microscopic study of the structure of the nervous system in invertebrates. Nerve fibres, he observed, appeared to originate in the ganglionic cells, a discovery that Müller then had him confirm in other organisms (Cahan, 1993, 24). After studying under Müller, Helmholtz moved to Königsberg in 1849 and began to work at ascertaining the speed at which signals travelled along nerve fibres (Finger and Wade, 2002).

The speed of a signal along a nerve fibre was initially thought to be almost instantaneous, perhaps even immeasurably fast (Pantalony, 2009, 21). Helmholtz wanted to test this for himself, but the technology of the day posed a significant barrier. In addition to being insensitive, the galvanometers available to him were sluggish and fickle. Rather than rely on the galvanometer to measure or detect charges in the muscle or nerve, Helmholtz utilized Pouillet's technique which used the galvanometer to measure short intervals of time. This was accomplished by placing the galvanometer in a circuit along with a battery and two switches (Glynn, 2013). The "first switch would be closed at precisely the moment at which a very brief but powerful shock [...] was given to the sciatic nerve of a recently killed frog," while "the second switch, which was closed before the experiment began, would be opened as soon as the muscle began to shorten" (Glynn, 2013). The idea was "that a current [...] would flow through the galvanometer from the moment the sciatic nerve was stimulated to the moment the muscle began to contract" (Glynn, 2013). The constant voltage meant that the "maximum deflection of the needle was determined by the duration of the current" (Glynn, 2013). Using a light source and a small mirror on the needle, measurements could be made on a scale from the light reflection. Through repeated experiments, by 1850 he was able to ascertain that the speed of the nerve impulse was 24.6-38.4 m/s (Helmholtz, 1850; Glynn, 2013). It was not until 1868 that Julius Bernstein, working in

Helmholtz's laboratory in Heidelberg, was able to expand on the insights of Helmholtz and Du Bois-Reymond and accurately measure the speed at which action potential moved along the sciatic nerve, clocking it at 28.7 m/s (Helmholtz, 1850; Glynn, 2013).

For both du Bois-Reymond and Helmholtz, the establishment of organic activity as reliant on electrical, rather than galvanic or vital forces, was crucial as an experimental demonstration against vitalism. Du Bois-Reymond, himself, was convinced that the nature of life could be explained through physical laws, yet "he was a hesitant mechanist at best" (Fullinwider, 1991, 27). For one, he ascribed to the argument that 'force' and 'matter' were abstractions. Scientists never have access to force or matter in-themselves. Rather, objects in the world are always already encountered in a reactive state (there is no pure state of force or matter that can be seen in the lab) (Fullinwider, 1991, 27). In the 1848 foreword to *Untersuchungen über Thierische Elektricität,* Du Bois-Reymond makes this position overwhelmingly clear: "Mit einem Worte, die sogenannte Lebenskraft in der Art, wie sie gewöhnlich auf allen Punkten des belebten Körpers gegenwärtig gedacht wird, ist ein Unding" (Du Bois-Reymond, 1848, 13).[57]

Du Bois-Reymond's position was emphatically shared with Brücke and Helmholtz, who would write to one another about their certainty that life could be explained on a physical-chemical level (Jones, 1953, 45). For Helmholtz, causality and the law of the conservation of energy additionally provide the basis of Helmholtz's rebuke of vitalism. Du Bois-Reymond saw Helmholtz's *Erhaltung* as the killing blow to the idea of a 'vital principle' (Fullinwider, 1991, 23). At least since the spread of Brunonianism through Germany, the vital force has been conceived of as a measurable energy or force. As the law of the conservation of energy

[57] "In a word, the so-called life force, in the sense in which it is usually thought in all cases of the living body, is an absurdity."

establishes that energy cannot be created or destroyed, the vital principle could not simply appear at birth and disappear at death. This criticism of vitalism would shortly be problematized by cell theory, insofar as it could be said that all living creatures already existed as living cells of their progenitor and that their energy was only lost in death. In any case, tracking the causal chain with the knowledge that energy could not be created or destroyed, as Helmholtz demonstrated in the *Erhaltung*, meant concluding either that at some point life-energy was created out of nothing or that life had to be explainable according to physio-chemical laws. The former being impossible, Helmholtz and Du Bois-Reymond concluded with the latter.

Intersubjectivity, a secondary aspect of the question of pre-established harmony, could be supported through measurement (Fullinwider, 1991, 26). Helmholtz accepted the conclusions of Müller and Kant that we have no knowledge of the world outside of that which is provided by our nervous systems. For Helmholtz, that "real world" outside of our direct perception is comprised of energy transferences. Many of these changes occur without our awareness until something happens which is available for sensory experience. At this stage, intersubjective awareness of the lawlikeness of perceptions can be explained through unconscious inferences, which depend on *a priori* judgements. That is to say, our awareness that others perceive the external world according to the same rules that our powers of perception do is rooted in the unconscious application of necessary truths. But they can also be corroborated through measurement. Reminiscent of Humboldt's work, Helmholtz saw measurement as a tool for asserting the veracity of intersubjective perceptions of the "real world" of energy transferences. Causality and the law of the conservation of energy additionally provide the basis of Helmholtz's rebuke of vitalism. Du Bois-Reymond saw Helmholtz's *Erhaltung* as the killing blow to the idea of a 'vital principle' (Fullinwider, 1991, 23). At least since the spread of Brunonianism through

Germany, the vital force has been conceived of as a measurable energy or force. As the law of the conservation of energy establishes that energy cannot be created or destroyed, the vital principle could not simply appear at birth and disappear at death. This criticism of vitalism would shortly be problematized by cell theory, insofar as it could be said that all living creatures already existed as living cells of their progenitor and that their energy was only lost in death. In any case, tracking the causal chain with the knowledge that energy could not be created or destroyed, as Helmholtz demonstrated in the *Erhaltung*, meant concluding either that at some point life-energy was created out of nothing or that life had to be explainable according to physio-chemical laws. The former being impossible, Helmholtz and Du Bois-Reymond concluded with the latter. The purview of the scientist, as they now understood it, was to understand the physical-chemical relationships that underlay the world—not to speculate about the sources of life or the metaphysical roots of organization in nature.

An illustrative point of emphasis here is found in Du Bois-Reymond's comments on the legacy La Mettrie. La Mettrie was something of a hero, to Du Bois-Reymond. So much so that in January 1875 Du Bois-Reymond dedicated an entire talk to the legacy of the peculiar Frenchmen, wreathing his name in honours and esteem. In particular, Du Bois-Reymond recognizes La Mettrie as he who "beobachtet den Einfluss von Fasten und Fleischkost, von Wein, Caffee und Opium auf die Vorstellungen[,] er zergliedert die denkbaren mechanischen Bedingungen des Gedächtnisses," amongst other things (Du Bois-Reymond, 1875, 193).[58] This excerpt is illustrative of the image of La Mettrie that Du Bois-Reymond wanted to impart, namely that of La Mettrie as an undaunted scout in the vanguard of the coming revolution of

[58] […] "observes the influence of fasting and meat diet, of wine, coffee and opium on the ideas[,] he dissects the conceivable mechanical conditions of memory."

physio-chemical mechanism. Little more needs to be said in asserting that Du Bois-Reymond and Helmholtz were mechanists. What this excerpt also stresses is the kind of role that substances of intoxication had in Helmholtz and Du Bois-Reymond's mechanism and how his views departed from those held by his teacher, Müller.

Neo-Mechanism contra Vital Stimulants

In Du Bois-Reymond's *Rede* and *Untersuchung über thierische Elekticität,* substances of intoxication are addressed infrequently, but when they are discussed it is generally in an effort to discredit the findings of other researchers. This takes the form of an explicit pattern of critique levelled against the vital substance concept in all its manifestations. In one section of his *Untersuchungen* titled "Einfluss verschiedener Todesarten des Thieres auf den Strom," Du Bois-Reymond brings forward the result of his own experiments and compares them with Matteucci's. The question at hand concerned differences in the electrical activity of a frog pile comprised of frogs poisoned by opium (Du Bois-Reymond, 1849, 171). Where Matteucci had discerned that the opiated frog pile performed more excitedly, du Bois-Reymond was steadfast in his findings that there was no measurable difference (Du Bois-Reymond, 1849, 171). Elsewhere, Du Bois-Reymond discussed an experiment involving "drei Frösche, welche bezüglich mit essigsaurer Strychninlösung, einem Brei von Opium purum und Tinctura Opii simplex, und dem Acidum hydrocyanicum Pharm. Bor. vergiftet worden waren, zeigten an ihrem Ischiadicus den Strom in gesetzmäßiger Richtung und nicht auffallend geschwächt" (Du Bois-Reymond, 1849, 287).[59]

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[59] [...] "three frogs which had been poisoned with respect to acetic acid strychnine solution, a mash of Opium purum and Tinctura Opii simplex, and the Acidum hydrocyanicum Pharm. Bor. had been poisoned, showed at their ischiadicus the current in lawful direction and not conspicuously weakened."

Later in that very section, Du Bois-Reymond added a further qualification that when nerves were locally bathed in *narkotische Gifte,* specifically hydrocyanic acid, morphine, opium tincture, and strychnine, nervous electrical activity was only eventually abolished (Du Bois-Reymond, 1849, 182).

Each of these examples are remarkable departures from Müller's *Handbuch.* Where Müller found intoxicants such as alcohol and opium to have both stimulating and destructive effects on nervous material, Du Bois-Reymond suggested these effects were either weaker than otherwise reported or questioned the existence of such a relationship altogether. Any questions about Du Bois-Reymond's motivations here are soon explained by way of Du Bois-Reymond's comments concerning some of his 18th century forebears in the field of animal electricity, in particular the Romantic Johann Ritter. Du Bois-Reymond's *Untersuchungen* makes frequent references to Ritter, far exceeding the number of references to Müller and barely falling behind the number of references to Matteucci or Volta (Du Bois-Reymond, 1848). While Du Bois-Reymond is quick to praise Ritter's role as a pioneer in the subject, he is swifter still to disparage his findings. More specifically, Du Bois-Reymond attacks Ritter's reckoning on the findings of Ritter's own experiments on the grounds that Ritter was profoundly influenced by Brunonian ideas (Du Bois-Reymond, 1848, 367 and 317-318). Du Bois-Reymond questioned the credulity of other Brunonians too, outright attacking Giovanni Aldini's experimental use of Brunonian stimulus (Du Bois-Reymond, 1848, 98).

The scope and persistent nature of Du Bois-Reymond's criticisms of Ritter and, with him, Brunonian ideas explains the marked departure in Du Bois-Reymond's conception of the effects of intoxicating substances on nervous material from those espoused by Müller. The vital substance concept, associated as it was with vitalism, needed to be exorcised from the thinking of the new mechanism. More than merely disagreeing with the concept's theoretical underpinnings, Du Bois-Reymond attempts to readdress all of the physiological findings which appear to be even secondarily associated with any notion of "vital substances". This in itself speaks to either the significance that Du Bois-Reymond saw in the influence of the vital substance concept or the intensity of its association with vitalism itself. Rather than modify the theoretical interpretation to account for the trove of findings in support of some form of excitation as result of intoxicating substances, Du Bois-Reymond did away with them.

The case could be made for Helmholtz as well, though Helmholtz employed an entirely different strategy of critique. Famously, "Helmholtz was very reluctant to participate in any scientific debate if he thought it fruitless" (Elkana, 1970, 290). This is not merely an observation of Helmholtz's temperament, but an overt "rule" spoken on by Helmholtz himself. Helmholtz deemed it "necessary to reply to criticisms of scientific propositions and principles only when new facts were to be brought forward" (Elkana, 1970, 291). The expectation was that, "when all data have been given, those familiar with the science will ultimately see how to form judgement even without the discursive pleadings and sophistical arts of the contrary part" (Elkana, 1970, 291). As a polemic strategy, the effects are twofold. Firstly, there is the immediate result of displacing or writing over the object of the would-be debate—where Helmholtz did not regard the opposing position as bringing forward new facts. Secondly, and more impactfully, Helmholtz's "rule" entailed a judgement concerning the scientific validity of the position in question. Those ideas which did not merit rebuttal had already been spoken to by the science, so to speak—a rhetorical structure which undermined the rational permissibility of opposing claims on the basis of a shifting notion of scientificity. Thus, Helmholtz's silence concerning both Brunonianism and the nature of intoxicating vital substances not only displaced them through his electrophysiological research, it excluded these concepts from consideration on the basis of the epistemic validity.

This segues into what was arguably the most significant element of the neo-mechanical conflict with the vital substance concept. The most potent movement against the last remnants of the vital stimulants was the exclusion of the methodological principles that had made possible their inclusion from the outset. For Brown and Sertürner, the state of intoxication entailed a tacit attunement to the state of embodiment. This intoxicated way of knowing imparted information about the world, empirically even, and the body's place in it; gathered in those places at those times, their experiences of intoxicated embodiment gave testimony. The neo-mechanical emphasis on an experimentally derived physio-chemical study of the body implicitly excluded these forms of testimony, to the extent to which they could. They excluded the study *with* the body from the study *of* the body.

Intoxicants could always of course intercede at the level of personal experience; however, it appears that, relative to those who came before them, the neo-mechanists sought to distance themselves as much as possible from vital substances. In their case, this meant moving away from intoxicants as central objects of scientific research, instead privileging the study of finer physio-chemical structures. It would be this shift to privilege certain modes of framing the study of the body, and their corresponding criteria of epistemic validity, that would guide what I have called the 'young' neo-mechanists to an impasse concerning the correspondence between neural states and mental states. It would be unto this very breach that substances of intoxication would, once more, intercede to testify concerning the nature of the relationship between the body and the mind.

Chapter 6—The 'Young' Neo-Mechanists and the Brain

Localization of Brain Function

By the beginning of the latter half of the 19th century, it appears there was a clear sense that physiology was well on its way to understanding the function of much of the body. Yet, prior to the emergence of the theory of brain localization in the latter 19th century, brain and nerve tissue had been studied, but little was known about how the brain functioned. Nevertheless, the concept of brain localization was not new. Franz Gall (1758-1828) developed a doctrine of brain localization at the end of the 18th century that led to the creation of phrenology (Van Whye, 2002, 17). His system, which he called the *Schädellehre* (Doctrine of the Skull), divided brain function into different sections, called "organs", which could be discerned through the analysis of the contours of the skull (Van Whye, 2002, 22). After six years of lecturing, Gall was unexpectedly banned from lecturing in Vienna by decree of Austrian Emperor Franz II in 1801 (Van Whye, 2002, 25). This ban failed to quash interest in Gall's doctrine and the attention roused by the ban quickly made Gall a topic of pan-European interest (Van Whye, 2002, 25). Gaspar Spurzheim (1776-1832), who worked under Gall as an assistant in Vienna, elaborated on Gall's work, further spreading phrenology to wider audiences in Europe and America (Bilal et al., 2017). The work done by Gall and Spurzheim briefly popularized an early model of brain localization, but by the 1840s their theories had largely fallen into disfavour owing to a lack of clinical substantiation (Simpson, 2005).

The 19th century model of cerebral localization would primarily be developed by a new generation of physiologists educated in the school of the organic physicists, Du Bois-Reymond and Helmholtz. Learning during a period of greater institutional centralization, the researchers behind the modern concept of brain localization also represent the first generation of professionalized physiologists, many of whom would go on to further develop different specializations. The discovery of the motor cortex in the 1870s made by Eduard Hitzig and Gustav Fritsch, both of whom studied physiology in Berlin, helped concretize the connection between physiology and psychiatry, which helped usher in the brain-psychiatry of Theodor Meynert and Karl Wernicke. It would be Karl Wernicke and Theodor Meynert who would expand upon the theory of brain localization to establish models of neurophysiology founded on scientific principles. In this way, Meynert and Wernicke's desires to see the development of a scientific psychology on the foundations of neurophysiology reflected the values of German physiology toward the latter half of the 19th century. This would ultimately be their undoing, as their *a priori* identification of brain matter as the site of valid experimental knowledge concerning the mind would lead to accusations of blurring the lines between psychology and physiology. Most importantly, it precluded them from considering the experimental value that intoxicants might provide, despite regularly interacting with various intoxicants in their clinical practices. It would be those who eventually introduced substances of intoxication into experimental psychological research who would also prove to be the most steadfast critics of Meynert and Wernicke's approach, which might be dubbed "brain-psychiatry". Arguably, it would be the relative failure of brain-psychiatry that would help give rise to experimental psychology in the first place. But, before exploring that possibility, it is worth briefly tracing the development of neo-mechanical neurophysiology, beginning with Hitzig and Fritsch's identification of the motor cortex.

The Motor Cortex as an Experimental Demonstration of a New Theory

Eduard Hitzig and Gustav Fritsch created a remarkable disturbance in the physiological community in 1870 when they demonstrated that they were able to cause movement when certain areas of the brain were electrically stimulated (Fritsch and Hitzig, 1870; Hagner, 2012). While Albrecht Kölliker had previously succeeded in establishing an *anatomical* relationship between motor nerves and the brain, Hitzig and Fritsch were suggesting that they could *physiologically* localize motor function in the human brain.

Gustav Fritsch (1838-1927) studied medicine in Berlin, Breslau, and Heidelberg, learning under none other than Helmholtz, Peters, Traube, Frerichs and Langenbeck (Anonymous [Nature], 1938). Eduard Hitzig (1838-1907) attended the University of Berlin and the University of Würzburg, where he learned under Virchow, Du Bois-Reymond, and Moritz Romberg, who had played a similar role as Müller for the field of neurology.[60] In this sense, Hitzig and Fritsch represented the next generation of physiologists—their views, and methodologies, reflecting those propagated by the "organic physicists" of Berlin. At the time, the pair were *Privatdozenten* at the University of Berlin. Barely more than Adjunct Professors, they were not allowed to conduct their experiments in the university laboratories (Millet, 1998, 284). Their conclusions overturned a principle of neurophysiology that had been foundational since Haller: cortical inexcitablity, the belief that the brain could not be artificially stimulated (Millet, 1998, 284).

Fritsch and Hitzig "reported that five punctate 'centres' could be distinguished in the anterior cortex of the dog, stimulation of which led to contractions of the contralateral muscles in the neck, legs and neck" (Fritsch and Hitzig, 1870, 311; Millet, 1998, 284). "Clonic movements and epileptic convulsions" were precipitated by the application of oscillating currents (Millet,

[60] Moritz Romberg was a Berlin neurologist who, as Müller had in physiology, contributed greatly to study of neurology through the collection, translation, and synthesis of neurological research from across Europe, primarily active between 1820-1850 (around the same time as Müller).

1998, 284). Of far greater interest were the localized muscular twitches caused by “constant galvanic stimulation” the moment a circuit was completed (Fritsch and Hitzig, 1870, 311; Millet, 1998, 284). Reviewing the outcome of their experiments, Hitzig and Fritsch conclude that the “anterior regions of the cerebral cortex is associated with motor function” (Millet, 1998, 284). While these conclusions appear simple given the outcome of their experiment, this was a crucial moment in the establishment of the modern understanding of brain localization. From the neo-mechanistic perspective, Hitzig and Fritsch’s work was cause for serious hope concerning an experimental understanding of the brain and mind in the near future. Their study of the motor cortex, in a sense, further demonstrated that the neo-mechanical research paradigm of organic physics was making good on its initial objectives.

The revolutionary impact of their joint discovery beautifully symbolized the beginning of a new era in German physiology and neurophysiology. The period from 1850-1870 had been dominated by the legacy of Johannes Müller. From Kölliker and Virchow to Helmholtz and Du Bois-Reymond, nearly all the great physiologists of the period had studied under Müller and, whether through modification or principled rejection of his theories, his ideas lived on in their work. Hitzig and Fritsch, as the students of Helmholtz, Virchow, and Du Bois-Reymond, represented the next generation of neurophysiologists. With the appearance of a new generation, however, came a transformation in the culture of physiology, a major part of which can be attributed to the formation of the first modern research universities in Germany.

By the 1870s, physiology and biology had retreated even further into the laboratory. Universities across Germany committed to massive expansion projects, investing heavily in laboratory space (Klinge, 2004, 128). Historical methods were increasingly displaced by experimentation (Weindling, 1993, 44). At this point, the life sciences were almost completely dominated by formal professionals, functioning within established institutional bodies and university research centres (Weindling, 1993, 44). Romantic thinkers like Haeckel were quickly becoming relics of a bygone era. This increased "professionalism [also] meant a retreat from the overt political activism of Virchow" and others, as researchers became increasingly focused on scientific work (Wiendling, 1993, 45).[61]

As seen in the case of Hitzig and Fritsch, these changes in the structure of the natural sciences had theoretical ramifications as well, as the new generation assumed the philosophical attitude taken by the "organic physicists." Professionalized physiology was well-suited to the stance taken by Helmholtz and Du Bois-Reymond, which favored a shared outlook amongst researchers, a common clinical nomenclature, and methodological agreement. Citing Darwin as the empirical foundation of biology, Hitzig and Fritsch were satisfied to approach the human body and mind as a physio-chemical process, rather than as a philosophical subject (Weikart, 2016, 115).

Another factor was the breakout of the Franco-Prussian war, also in 1870. Like many young physiologists, Hitzig served in the Franco-Prussian war as a physician. While very little information from his time in the war was reliably documented, Hitzig discussed his clinical

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[61] Virchow understood physiology, and particularly cell theory, to be political consequential. Politics, Virchow argued, was nothing less than the medicine of the *polis,* referring to both the environmental origins of many ailments and the analogous relationship between cells in a body and people in a society. Just as a small number of cancerous cells can undermine the entire body, strife in one sector of society compromises the well-being of society as a whole. Such conclusions led Virchow, and, to a lesser degree, other physiologists, to advocate for sweeping political and economic reforms (Wiendling, 1993).

treatment of Joseph Masseau, a 20-year old French soldier of the 30th Line Infantry regiment, in *Physiologische und klinische Untersuchungen über das Gehirn* (Hitzig, 1874, 115). Masseau "hatte am 10. bei Orleans einen Streifschuss an der rechten Seite des Kopfes erhalten," a wound which eventually say Masseau experience localized, involuntary convulsions (Hitzig, 1874, 114 and 116).[62] In the autopsy, Hitzig was able to connect Masseau's symptoms with damage to the motor cortex, reflecting Hitzig and Fritsch's recent findings in experimenting on a dog (Hitzig, 1874, 119).

The Masseau case was a turning point for Hitzig in more ways than one. The Masseau case was a demonstration of the clinical significance of Hitzig and Fritsch's work, specifically in the field of psychiatry. Further clinically significant experimentation on the brain was Hitzig's study on induced seizures, once again in dogs. In a rather brutal experiment, Hitzig succeeded in imparting several dogs with seizure disorders, a malady then associated strongly with mental illness, by damaging portions of the brain (Hitzig, 1874, 273). Spurred on by newfound supporting evidence, Hitzig's work helped guide neo-mechanical thinking in the developing field of psychiatry, as the domain of a yet unconquered realm in physiology: the human mind.

Hitzig and Fritsch's study of the motor cortex would change from a critique of the theory of cortical inexcitability to an experimental demonstration of a largely theoretical, conceptual model of neural activity. Already theorized in the work of Theodor Meynert (1833-1892), who suggested there were discernible variations in the histological structure of different regions of the brain in 1867, Hitzig and Fritsch's rebuke of the theory of cortical inexcitability would come to be framed as a crucial experimental demonstration of the developing theory of cortical localization (Meynert, 1867; Meynert, 1872). It would be this model, championed by, above all,

[62] […] "had received a graze shot to the right side of the head on the 10th near Orleans."

Theodor Meynert and Karl Wernicke, that would form the foundation of the most aggressive efforts to see even higher order mental states equated with neural states, in an effort to settle the final frontier of the neo-mechanical body.

Mapping an Inner World: Meynert and Cerebral Cytoarchitecture

Meynert had been a student of Carl von Rokitansky (1804-1878) in Vienna, under whom he studied neurological diseases of the brain and spinal cord (Guenther, 2015). In Vienna, Meynert was also exposed to Ernst Brücke who had taken on a professorship at the University of Vienna as early as 1849 (Seebacher, 2006). Brücke was recommended for the position in Vienna by anatomist Joseph Hyrtl who, although sceptical of this new neo-mechanist appproach in physiology, believed that Brücke's familiarity with microscopic analysis was sorely needed by Vienna's medical faculty (Seebacher, 2006). Eventually, Meynert's expertise in neuropathology brought him to psychiatry, assuming a position as head of Vienna's psychiatry clinic in 1870 (Guenther, 2015). Meynert had no previous psychiatric experience or training, leading some to question his eligibility for the post. Concerns over eligibility arose again in 1873 when Meynert was up for a promotion, but Meynert's former mentor Rokitansky interceded to see that Meynert got the position (Guenther, 2015). There is no reason to understand this as a simple case of nepotism; Rokitansky understood that Meynert had a vision of an interdisciplinary approach to brain research, one which would bring the anatomical study of nervous tissue out of the lab and into the clinic (and back again) (Seitelberger, 1998; Guenther, 2015). One of the dissenting votes had been cast by the alienist Ludwig Schlager (1828-1885), director at Niederösterreichische Irrenenstalt am Brünnlfeld. Schlager was one of the most celebrated alienists in the Empire (Shrady & Stedman, 1885, 211). Schlager was so incensed by Meynert's presence that they both eventually refused to work together at the psychiatry clinic (Guenther, 2015). Clearly, there was no love lost between the two men, but more than anything their collegial spat epitomized the struggle between the perspective of alienist psychology and Meynert's designs for a new psychiatry grounded in anatomical research.[63] This issue ultimately resolved in 1875 when the psychiatric clinic was split in two, with Meynert leading the Second Psychiatric clinic (Dalzell, 2011; Guenther, 2015).

Meynert was not alone in asserting that German psychiatry was at a crossroads. Wilhelm Griesinger (1817-1868), founder and editor of the *Archiv für Psychatrie und Nervenkrankheiten* (Journal for Psychiatry and Nervous Diseases), prefaced the journal's first 1867 edition with the observation that German psychiatrists were too preoccupied with philosophical attitudes, despite firm evidence of the connection between neuropathology and mental illness (Marx, 1970). The cause of seeing the mentally ill treated like any other patients was supported by the idea that mental illness was physiological in nature. The only difference was the difficulty of assessing neuropathological conditions (Marx, 1970). Meynert understood how this functioned in both directions. Neuropathology would remain incomplete as long as it refused to recognize mental illness as physiological in nature, but the study of a given mental illness's neuropathology required external support through clinical observation (Marx, 1970). The result is a totalizing project to map the diverse functions of the brain through neurophysiological research, in the process subsuming psychiatry and psychology into neurology.

While Meynert's position was that of a director of a psychiatry clinic, Meynert was first and foremost a neuroanatomist, with little interest in clinical practice (Hakosalo, 2005). Meynert's visionary approach understood the scope of brain research as an emerging field that

[63] Karl Jaspers likened the spat between the two men to a conflict between asylum psychiatry and university psychiatry, or bottom-up versus top-down psychiatry (Pichot, 2013).

required the cooperation of then conflicting disciplines (Seitelberger, 1998). Here, brain research was not merely a pursuit of anatomical interest; it meant coming to understand mental illness in scientific terms.

Meynert's theories of cortical localization were built upon his histological research on the brain stem, cerebrum, and fibre systems and were externally corroborated by clinical observations (Seitelberger, 1998). An essential component of Meynert's understanding of cortical localization was his discovery of the difference between projection and association fibres. Projection fibres extend from the spinal cord "to the hemispheres and descend from the cerebral cortex through the medulla and white substance to the spinal cord" (Hakosalo, 2005, 175). Where the projection fibres establish connections between the brain and the rest of the body, association fibres served the function of forming links between the cortices of the brain (Hakoalo, 2005). This distinction between projection and association fibres provides the structural framework for Meynert's map of cortical function. In 1865, Meynert traced the locations of the visual, auditory, and olfactory centres to the occipital and temporal lobes (Meynert, 1865). A year later, Meynert was able to study a patient suffering from aphasia, identifying the cause to be growths in the wall of the temporal lobe (Hakosalo, 2005).

Though Meynert had been fairly successful anatomically mapping cortices by tracing nerve fibres, Hitzig and Fritsch's 1870 experimental identification of the motor cortex in a dog was a major turning point for Meynert's work. Meynert had actually predicted Hitzig and Fritsch's results, including their conclusions of the excitability of brain tissue, but had not been able to experimentally demonstrate his findings. Hitzig and Fritsch were well aware of this, as they remarked in their 1870 paper:

> Der einzige, welcher auf Grund von anatomischen Untersuchungen, deren Möglichkeit freilich von Manchen angezweifelt wird, einen von der herrschenden Meinung durchaus abweichenden, aber ganz entscheidenen Standpunkt einnahm [(that brain tissue was not excitable)], war Meynert (Fristch and Hitzig, 1870, 307).[64]

Hitzig's and Fritsch's work provided experimental confirmation of the clinical significance of Meynert's anatomical research. Two years later, Meynert published "Vom Gehirne der Säugethiere" in the second volume of *Handbuch der Lehre von den Geweben des Menschen und der Thiere*, before undertaking a more serious effort in clinically applying his theoretical understanding of brain function (Meynert, 1872).

Meynert's idea that the sum of human experience could be explained on a physiological level was not unique or revolutionary, and in many ways had been an expectation within academic physiology since at least the 1860s. Further still, Wilhelm Griesinger had already established himself as an advocate for a shift toward a physiological model in psychiatry. What made Meynert a visionary was his eagerness to personally extend the very new concept of cortical localization, as a bodily description of the mind, into the clinic sphere, and begin to reconceive of clinical practice from the ground up.

On (the Other) Aphasia: Karl Wernicke's Meynertian Localization of Language

Despite Meynert's optimism and the clear significance of Hitzig and Fritsch's finding, the cytoarchitectural research program that was emerging out of the neo-mechanical approach in physiology had yet to provide any notable discoveries concerning what were perceived to be

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[64] "The only person who, on the basis of anatomical investigations, took a decisive standpoint that differed from the prevailing opinion [(that brain tissue was not excitable)], the possibility of which is, of course, doubted by some, was Meynert."

higher order brain functions, such as language. This would change in 1874 with the publication of "Der aphasische Symtomencomplex" (The Aphasic Symptom Complex) by Karl Wernicke, written while studying under Meynert in Vienna (Marx, 1970, 364).

The publication's full title reads: "Der aphasische Symtomencomplex, Eine psychologische Studie auf anatomischer Basis." The subtitle alone leaves no question as to how Wernicke understood the broader theoretical implications of his work. Though, if any question remained as to Wernicke's research objectives, the opening paragraphs of "Symtomencomplex" makes them perfectly clear:

> In der von Meynert geschaffenen Faserungslehre des Gehirnes sind die Anfänge einer exacten Gehirn-Physiologie enthalten, zwar nur in grossen allgemeinen Zügen, aber in Zügen von so genialer innerer Wahrheit, dass sie schon jetzt unbedenklich die Anwendung auf den einzelnen Fall gestatten.
>
> Die vorliegende Arbeit ist ein derartiger Versuch, die Meynert'sche Gehirn-Anatomie praktisch zu verwerthen, und zwar für ein Gebiet, in welchem derartige Grundlagen am meisten Befürfniss sein sollten, in der That aber bisher am wenigsten benützt worden sind (Wernicke, 1874, 3).[65]

Wernicke's aphasia research was explicitly intended to not only empirically validate Meynertian neurophysiology but further extend it into the practical domain of psychiatry. The combined effect of such an undertaking would, at least theoretically, further establish the potential for neurophysiology to provide physical explanation for a wide variety of mental states.

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[65] "In Meynert's theory of brain fibres are contained the beginnings of an exact brain physiology, indeed only in large general features, but in features of such ingenious inner truth that they already now unhesitatingly allow for their application to the individual case. The present work is such an attempt to make practical use of Meynert's brain anatomy for a field in which such fundamentals should be most needed, but in fact have been used least so far."

Thus, it made sense that Wernicke chose aphasia as a starting point. French physician and anatomist Paul Broca (1824-1880) had already provided a firm foundation through his own work on aphasia. In France, earlier arguments for cerebral organization had undergone a process of steady decline, in part due to the experimental failings of phrenology (Finger, 2000). During the 1860s, Broca forever changed the debate surrounding brain localization when he presented his findings on autopsies done on twelve patients suffering from aphasia (Finger and Wade, 2002). All of the autopsies found lesions in the frontal lobe, specifically encompassing a region which is now termed Broca's Area.

Reviewing Broca's findings, Wernicke suggested that Broca's Area was found adjacent to motor nerves that affected the lips, vocal cords, and parts of the face (Wernicke, 1874, 19). This led Wernicke to theorize that this region contained the "rules" for speech production (Wernicke, 1874; Geschwind, 1970, 941). He linked these findings with his own observations that receptive aphasia was specifically related to damage to a region found in the posterior section of the superior temporal gyrus (Wernicke, 1874; Geschwind, 1970, 940). Later termed Wernicke's area, Wernicke proposed that these two regions of the brain were linked and that this relationship could be used to explain the different ways in which Broca's aphasia and Wernicke's aphasia affected written and spoken language production and reception (Geschwind, 1970, 941). One explanation given for the effect of receptive aphasia on written language suggested that written language developed in relation to a spoken language, so damage to Wernicke's area would impair comprehension of both spoken and written language (Geschwind, 1970, 941).

Wernicke's Applied Neurophysiology

Following a brief stint of enthusiastic study under Theodor Meynert, Wernicke moved to Berlin in order to work under Karl Westphal at the Berlin's Charité, from whom Wernicke would learn about psychiatry. Westphal, in the tradition of Griesinger's advocacy for the recognition of mental illness as brain diseases, saw the future of psychiatry to be in neurological and physiological research (Marx, 1970, 361). In this sense, Wernicke came by his interest in synthesizing mechanical and physiological principles in explaining higher brain functions in both his neurophysiological and psychiatric training. Wernicke's sought to continue on the path laid out in both Meynert's anatomical theories and Broca's work on language, which set off by asking whether it was even possible to localize higher brain functions (Roth, 2002). Wernicke's observations concerning the localization of language function, combined with Hitzig and Fritsch's earlier work on the motor cortex, led Wernicke to theorize about the localization of further brain functions. Utilizing the framework established in his own work, Wernicke soon aspired to use physiology to explain all forms of complex brain function (Marx, 1970, 365). He hoped that this new paradigm in cortical localization would provide the foundation for a natural science of psychology in neurophysiology.

This new neurophysiological concept of cortical localization should be distinguished from that put forward by F. J. Gall, which ascribed fixed locations for all brain functions (Marx, 1970, 365). Although Wernicke conceived of the brain as a network of cortical centres, he theorized that memory and emotion were facets of the structure of the brain itself. Memory was conceived of as "lasting changes" in the structure and organization of nerve cells, whereas emotions were an undetermined "property of living cells" (Marx, 1970, 365).

Like many of his predecessors, there is strong evidence to support the inference that Wernicke's neurophysiology was to be understood as strictly physio-chemical. In his lectures, Wernicke made a point of qualifying terminology associated with vitalistic concepts in Müller's *Handbuch* with more concrete psychological concepts. For example, as opposed to the vitalistic meaning used by Müller, "Organ sensation" (*Organempfindung* or *Organgefühl*) is instead used by Wernicke to refer to sensory input associated with muscle or eye movement in addition to sensory input from the skin (Miller & Dennison, 2015, 450).

Publicly, Wernicke took the offensive against those who resisted the neo-mechanist research program in psychiatry, becoming an outspoken critic of alienist experiential psychology in the treatment of mental illness on the basis of what he perceived to be an absence of physiological support (Engstrom, 2003, 101). In spite of this, Wernicke took a holistic approach in the treatment of his own patients as a psychiatrist. This is reflected in Wernicke's lectures on psychiatry, collected in *Grundriss der Psychiatrie* (1894). Here, Wernicke's lectures illustrate his tendency to describe psychiatrically relevant symptoms in a neurophysiological way and yet, in the very same series of lectures, prescribe treatment principles which appear to have no clear physiological foundations.

Wernicke understood the psychiatric institution, or asylum, to be at the absolute centre of psychiatric treatment. This was not only practical; the very structure of asylum life was understood to have a curative function. Much of our behaviour, Wernicke argued, amounts to processing stimuli in accordance with a given cognitive order, creating a more general order between perceptions, as Wernicke explains:

> Wie Ihnen erinnerlich, nannten wir die Verknüpfung der Vorstellungen Association, die in den Vorstellungen herrschende Ordnung lässt uns also auf den Besitz ganz bestimmter Associationen, welche für alle Individuen annähernd gleich sind, schliessen. Unsere Frage kann daher auch so gestellt werden: Auf welche Weise bilden sich diese allgemein gültigen Associationen? Bei Besprechung des Bewusstseins der Aussenwelt habe ich schon darauf aufmerksam gemacht, dass sich die natürliche Ordnung und Aufeinanderfolge der Dinge in unserem Gehirn gewissermaassen widerspiegele, und dass dadurch eine gesetzmässsige Verknüpfung bestimmter Erscheinungen unter einander, wie

> sie die Aussenwelt uns liefert, auch in unserem Bewusstsein hergestellt werde (Wernicke, 1900, 69).[66]

Wernicke appeals to an operative theory of mind in which each individual's mind is comprised of an order of sequential associations which reflect a shared reality, and so are relatively similar. In those suffering from mental illnesses, this 'proper' order becomes disturbed—dislocated (Wernicke, 1900, 69-70). Here, the very structure of the asylum works toward the ordering of perceptions and mental associations. The theoretical foundation of this method is grounded in Wernicke's argument that the behaviour of individuals in a group bends to loosely conform with that of the whole (Wernicke, 1900, 70). This notion, with the support of a monitored and regulated lifestyle of asylum life, is intended to holistically order a patient's mental associations in accordance with 'reality.' For Wernicke, "die ganze Anstaltsbehandlung der Geisteskranken steht auf diesem Fundament" (Wernicke, 1900, 70).[67] Of course, in cases where the symptoms are acute, the institution also isolates the patient from broader society, where they can 'safely' be sedated with something such as opium or chloral (as required) (Wernicke, 1900, 280).

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[66] "As you remember, we called the connection of the ideas association, the order ruling in the ideas lets us therefore conclude on the possession of certain associations, which are approximately the same for all individuals. Our question may therefore be put thus: In what way are these generally valid associations formed? In discussing the consciousness of the external world, I have already pointed out that the natural order and sequence of things is reflected in our brain to a certain extent, and that thereby a lawful linking of certain phenomena among each other, as they are supplied to us by the external world, is also established in our consciousness."

[67] "The entire institutional treatment of the mentally ill rests on this foundation."

There is also what Wernicke termed the "Hauptmittel zur Erwerbung einer bestimmten Ordnung in den Vorstellungen," which is articulated speech (Wernicke, 1900, 70).[68] This is because, Wernicke reasoned, the internal structure of language trains the mind to form series of associations in a consistent way, including the most abstract of concepts (Wernicke, 1900, 70-71). To this point, it is apparent that Wernicke's focus on language is at least partially a byproduct of his own work on aphasia. Citing precisely this research, Wernicke argues that, insofar as those suffering from aphasia still understand concepts and emotions, language is the tool that trains the mind rather than the medium through which associations are maintained (Wernicke, 1900, 70-71).

In each of these treatment protocols, Wernicke's approach reflects a robust philosophy of mind, albeit one not explicitly derived from neurophysiological principles. The example of articulated speech reflects instances where Wernicke's approach to understanding mental phenomena appeared to be grounded in brain research; however, this is only in the form of inference from a largely unrelated point of study. There was an apparent disconnect between the perceived significance of recent discoveries surrounding cortical localization and clinical practice.

The careful reader is probably wondering what, if anything, does this have to do with a story that has been, for the most part, about intoxication. But, this is precisely the point. The extent to which neither Meynert nor Wernicke appear to have been particularly interested in studying the effects of intoxicants on the mind, despite extensive use of various *Nervina* in their clinical life, should be shocking. The psychiatric epoch of Meynert and Wernicke was the heyday of chloral hydrate. From the 1860s onward, chloral had quickly become a pillar of

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[68] […] "Main means of acquiring a certain order in the ideas."

asylum life, due to its profound hypnotic effects and the fact that it did not need to administered by injection (Shorter, 1997, 231). Use of chloral at home also became increasingly common, a means for the well-to-do to avoid a hasty and embarrassing visit to the asylum. This led to an explosion in the number of habitual chloral users, with particular societal emphasis on middle class women (Shorter, 1997, 231). The chloral phenomena began in the German world in the 1860s, yet by the 1880s was already wreaking havoc in America. Harold Kane discussed the growing chloral phenomena in America in his 1881 book *Drugs that Enslave: The Opium, Morphine, Chloral, and Hashisch Habits*, where he remarks at the sheer scope of overlooked chloral use (Kane, 1881, 149). Kane himself was convinced of the fact that chloral use was habituating, though he acknowledges this argument was hotly contested among European physicians (Kane, 1881, 149).[69]

Back in Germany, not only were Meynert and Wernicke on the front line of the chloral phenomena, they were direct participants in introducing patients to chloral hydrate. Wernicke discusses the use of chloral, morphine, and opium in the treatment of the more frenetic patients under rare circumstances in his *Grundriss* (Wernicke, 1900, 292). There is even a somewhat well-known case where Meynert referred a 42 year old woman to a private clinic on grounds of an initial diagnosis of a chloral habit, only for her to flip into psychosis upon cessation of chloral use (Eder, 1889, 267). It is astounding that the effects of intoxicants on the mind were practically never discussed by either Meynert or Wernicke, given the prominence of questions of intoxication as well as addiction in their professional lives. Meynert was even allegedly to be an alcoholic (Shorter, 1997). The answer to this question, as will soon be discussed, rests with the

[69] Notably, the text itself uses Brunonian concepts to explain the function of the various drugs on the nervous system (Kane, 1881).

way in which any questions surrounding the effects of intoxicants on the mind were theoretically framed and are bound up in the broader issues that ultimately undermined Wernicke and Meynert's shared research program.

Neo-Mechanistic Neurophysiology: Much Ado about Nothing?

By 1891, the young Sigmund Freud would remark in his own *Zur Auffassung der Aphasien* that Wernicke might have conflated psychological concepts with physiological constructs (Freud, 1891). Freud would even go as far as suggesting that Wernicke's neurophysiological foundations were in fact "the product of psychological assumptions […] disguised in anatomical and physiological terms" (Marx, 1970, 368). Meanwhile, Meynert's grand aspirations to see the development of an interdisciplinary brain science had been dubbed the 'brain mythology', an accolade which Karl Jaspers would bestow upon Meynert and Wernicke, jointly, in 1913 (Jaspers, 1913). But the prestige of both Meynert and Wernicke had begun to unravel far earlier still, arguably well underway by the 1880s and assuredly so by the 1890s. Meynert had been Rokitansky's star pupil; an esteemed friend and colleague to Ernst Brücke, one of the original organic physicists; and a respected peer in the eyes of Eduard Hitzig, among others. In sum, Meynert had descended from a place of standing as the presumptive progenitor of a definitive physio-chemical reckoning on the nature of the human mind to something of a has-been, bringing Wernicke with him. What is to be made of the sudden collapse in their joint research program?

The crucial issue was the difficulty of practically translating neurophysiological brain research into an empirical understanding of mental illness in particular, and mental states in general. Meynert's conception of brain function was strictly mechanistic. The foundational principles from which Meynert extrapolated psychological functions from cerebral anatomy were little more than the assumption of Bell's law for reflexes and the Müllerian dogma that the role of nerve cells is sensing, while sensations were formed in the forebrain (Meynert, 1884, 126; Marx, 1970, 362). This was useful for describing certain phenomena, such as blinking, but raised greater complications when marshalled to describe more complicated mental activities (Meynert, 1884, 146). As a result, it was all too simple for Meynert to conflate psychological concepts with physiological facts. Meynert's general disinterest in clinical practice may have been his saving grace, given that in Wernicke's case the apparent disconnect between his approach to mental illness and neurophysiology was plain for all to see.

There were other factors which ultimately undermined Meynert's research program. Hakosalo makes the case that Meynert's authoritative footing was eroded by methodological innovations in neuroanatomy, particularly the development of serial sectioning. Coming up in the 1860s and 1870s, Meynert's methodology consisted primarily of the meticulous hand tracing of nerve fibres. Meynert particularly described his own *Abfaserung* technique using the verb "einbrechen," to break in, and describes how the relationships between cortical structures were best observed by pulling apart structures of the brain to view fibre bundles *in situ* (Meynert, 1884, 39-40). The result was something of a roughly hewn severance which, to his credit, preserved the integrity of structures which might otherwise have been occluded by a cross-section, although Meynert also hand-sectioned with a razor (Hakosalo, 2006, 179). Meynert's skill at hand-sectioning was admired as a tremendously difficult in itself, though it made reproduction of his findings challenging.

While Meynert continued to make use of the difficult *Abfaserung* method, use of the microtome in neuroanatomical research was quickly becoming a standard practice. Though Bernhard Gudden had been the first to use the microtome on brain tissue no earlier than 1875, use of the microtome in neuroanatomy had become routine by the 1880s (Hakosalo, 2006; Monakow, 1970; Gudden, 1875, 229). This was partially on account of the difficulty of sufficiently hardening brain tissue in order to achieve a clean slice, without altering the appearance of the brain matter itself. This particular issue, that of ascertaining the most effective methods for staining and hardening samples, had plagued German physiology for much of the 19th century. The needs of the community had rapidly outstripped the available techniques. Rudolf Virchow had widely used a carmine stain, the use of which can be trace back to late 18th century botany (Alturkistani et al., 2015). Waldeyer developed a hematoxylin stain in 1863, which was flexible and acid-resistant but a relatively weak stain (Alturkistani et al., 2015). Although Italian biologist Camillo Golgi developed the first method for staining nervous tissue using potassium dichromate followed by silver nitrate in 1873, it was not until Kölliker's review of the method, published after visiting Golgi in 1887, that the silver stain technique would be properly introduced to German physiology (Shepherd, 1991, 170). Meynert himself used a variation of the carmine staining method, similar to that devised by Rudolf Berlin and used by Virchow, which utilized a fixing agent consisting of potassium dichromate, a carmine-based stain, and a turpentine clearing solution, though some samples were hardened using alcohol and acids (Shorter, 1997, 97; Meynert, 1867, 198).

Hakosalo argues that while Meynert maintained his fidelity to the older *Abfaserung* method and sectioning by hand, newer work favoured serial sectioning with the assistance of a microtome, which enabled consistent transparent brain sections which could be viewed under a microscope (Hakosalo, 2006, 185). This can be seen in the almost immediate effect that the advent of serial sectioning had on the anatomy of cerebral pathways, a conceptual focus of Meynert's research program. As Hakosalo points out, Auguste Forel recognized from the moment he first saw sections of the human brain that this spelled trouble for Meynert (Hakosalo, 2006, 188; Forel, 1935, 74). Things came to a head when Emmanuel Mendel gave a presentation on the structure of the "superior cerebellar peduncle at the Medico-Psychological Society of Berlin in January 1878," questioning "whether the auditory pathway was related to the superior cerebellar peduncle or not" (Hakosalo, 2006, 188). Mendel concluded "that the acoustic pathway was related to the superior cerebellar peduncle on the level of the dentate nucleus," a contradiction of Meynert's schematics (Mendel, 1878, 402; Hakosalo, 2006, 188). Notably, although Meynert was not present to defend himself, Wernicke was at the meeting and, on account of their personal and professional closeness, he was swift to attack the serial sectioning method on display (Mendel, 1878, 403). Wernicke's defence was, ultimately, unsuccessful and Meynert's anatomical descriptions would fall under continued assault in the coming years, particularly from Constantin von Monakow.

Even before his death in 1891, "malicious tongues claimed that Meynert's only connection with psychiatry was that he had gone through delirium tremens" (Shorter, 1997, 97). This was a pointed slight against Meynert's alleged alcoholism. There is a clear case to be made here that the shortfalls of both Meynert and Wernicke's neuropsychiatric research program, though visible, could have been optimistically overlooked in the face of their apparent success in neurophysiology. However, when Meynert's and Wernicke's neuroanatomical work was increasingly threatened by contradictory findings, the broader theoretical argument for late-nineteenth century brain-psychiatry was no longer tenable. Though methodological and representational changes in German brain anatomy undermined the Meynert-Wernicke research program of interdisciplinary brain science, brain anatomy evidently continued to flourish, even thrive. The failure of Meynert and Wernicke's approach made way for further developments in the study of brain anatomy, but their competitors, the likes of Mendel, Monakow, and Obersteiner, were deterred from making grand conflations between anatomy and mental life.

The neo-mechanistic ideal of seeing mental states reduced to neurophysiology had been all but decisively routed. This was, in a sense, the ultimate turning point in the broader project of organic physics. Neo-mechanism, as a scientific and philosophical that reflected the methodological approach of scientific physiology, had been at the heart of remarkable industriousness in nearly every branch of physiology and anatomy. Yet, Meynert's and Wernicke's aspirations, the once great enthusiasm for their approach, also reflected the extent to which, simultaneously, there was an understanding that a neo-mechanistic description of the phenomena of organic beings would always remain incomplete for as long as it could not also account for mental life. In this sense, the most fervent strains of neo-mechanism had failed.

The difficulty faced by neo-mechanistic attempt to account for mental states is arguably a by-product of the underlying assumptions of the approach itself. There is a consistent conceptual throughline that can be traced from the likes of Helmholtz's relegation of the world as it is perceived to quasi-illusion to Meynert's cytoarchitectural brain paradigm. In both cases, there is an *a priori* assumption that the locus of the "real" was not found in questions of perception or representation, but rather in a concealed series of physical interconnections. Helmholtz's world of energy exchanges was simply the ideal description of a neo-mechanistic, Platonic reality of which Meynert's machine brain was a part. These were underlying theoretical assumptions with methodological and hermeneutical implications: certain possible objects of scientific inquiry were elevated, while others were diminished, and more still all but disappear.

In the case of the neo-mechanistic encounter with the body, by merit of their approach's underlying theoretical principles, they privileged the anatomical body as the ultimate source of meaningful knowledge about life. But this focus on the body as scientific object was at the expense of a consideration of the body as something experienced, as *embodied.* This may itself have been a product of the Neo-Kantian interpretation of Müller's law of specific nerve energies, from which it followed that when we perceive the world we truly perceive our nervous system. As a result, any scientific inquiry into the experience of embodiment became occluded by the physiology of perception. The issue, of course, was that the summation of conscious experience was irreducible to simple sense perception. Thus, when Meynert and Wernicke made a pioneering effort to extend this approach into mental life, they hoped to describe the mind by way of the body. But, they could only see the body, and, in the end, their efforts fell tragically short. The study of mental states is, after all, the study of the experience of *embodiment*. The prioritization of the anatomical body as the locus of the "real" encounter with life was simultaneously an erasure of life as *embodiment.*

It is worth considering how the retraction from any serious discussion of a science of embodiment beyond the physiology of perception and neural states also served to legitimize the neo-mechanistic approach in its nascence. As has been discussed, a great deal of support for the vital substance concept stemmed from the embodied experience of intoxication, from Brown's initial opium insight to Sertürner's identification of the *principia somnifera.* These were empirical insights, albeit derived from the lived experience of intoxication, which were perceived as having real purchase on the ultimate nature of living beings. This association between not merely intoxicants but the experience of intoxication itself and vitalism may well have discouraged neo-mechanists from studying not only intoxicants but embodiment itself. A hard rejection of vitalism was the initial impetus behind Brücke, du Bois-Reymond, and Helmholtz's neo-mechanical shift. In this sense, an attitude of disinterest toward a scientific study of the experience of embodiment, which did not focus on the physiological foundation of said experience, served to further delegitimize the work of their vitally-minded forebears. The scientific priority of the body in the neo-mechanical doctrine erased the experience of embodiment as a meaningful object of study, not only on the basis of its underlying theoretical foundations but on a rhetorical basis. But another line of inquiry had risen in the time since the Icarus flight of Meynert's interdisciplinary brain project. Eventually positioning themselves in direct opposition to Meynert's kind of brain-psychiatry, Leipzig, first with Fechner and then through Wundt, would become the site of a novel research program dedicated to the development of a new science of embodiment. It would be this new science that would ultimately provide an alternate description of the relationship between bodies and minds, one which only became possible on a theoretical level by taking root in the space left behind by a routed brain-psychiatry.

Chapter 7—A Tale of Two Cities: Berlin, Leipzig, and Scientific Psychology

Gustav Fechner, from the Psycho-spiritual to the Psychological

As legend has it, the peripeteia in Gustav Fechner's life came in 1839 in the form of an experiment on vision gone awry, one which saw Fechner partially blinded and resigned to stepping down from his position as professor of physics (Boring, 1950). After stewing in pain and poverty for a decade, Fechner imagined, while lying in bed the morning of October 22, 1850, that there might be a connection between geometric relationships in the magnitude of bodily stimulus and logarithmic functions (Ludlow & Alvarez-Salvat, 2000, 6). It was in this sunken state that Fechner is purported to have turned from physics to thoughts on the relationship between the body and the mind. Fechner's would devote the greater part of his remaining life to the development of an experimental understanding of the relationship between consciousness and external stimuli: *Psychophysik.*

Drawing heavily on the legacy of his former teacher and colleague—Ernst Weber, Fechner's psychophysical research program set the foundation for Leipzig as the epicentre of a new science of psychology, sweeping aside Kant's dismissal. Notably, while Weber had been an influential proponent of physiological reductionism, Fechner infused his own anti-materialist perspective, drawing heavily on his adolescent investment into his budding endeavours in the study of the psychical. The psychological distinction between the mental and the physical would gain further expression in the work of Wilhelm Wundt, a student of Fechner's who established the first laboratory for experimental psychology in Leipzig. Here, Wundt would erect an influential, scientific approach to studying the psychical, one that is best described as a template for the study of embodiment rather than the body. If Berlin was to be the capital city of the “body”, Leipzig would become the capital of the embodied mind—and it is here, in the embodied mind, that intoxicants will once again be brought into conversation with the body.

For the purposes of this story of the intoxicated body, making sense of the conditions underlying the emergence of Leipzigian experimental psychology as a science of embodiment, its theoretical alterity relative to the neo-mechanist's bodily description of the mind, and its underlying conceptual tensions, is critical to understanding how intoxicants were marshalled as testifying witnesses in the trial over the nature of the biological subject. The Fechnerian project of psychophysics will provide an alternative model for not only the nature of the relationship between the mind and body, but the study of mental processes. It would be Wundt's controversial interpretation of both Fechner's principle of parallelism, and his radical empiricism, that would give rise to Wundt's distinct notion of the psychical, taking root in the theoretical opening created by the seeming failure of the “brain-psychiatrists.” Possibly, the rational validity of the entire research program of experimental psychology was a product of this failure, and Wundt, along with his students, would soon be counted among the most outspoken critiques of Meynert’s approach to the study of the mind. It is this conceptual context—one raised on a foundation developed in opposition to the neo-mechanical conception of the body—that will make possible the experimental encounter with the state of intoxication as the manipulable expression of dynamic psychological processes. To this end, let us first explore Fechner’s project of psychophysics, how it influenced the emergence of Wundtian psychology, and the foundational principles of Wundt’s experimental laboratory.

Fechner as Philosopher, From the Eternity of the Soul to Psychophysical Parallelism

The stated goal of taking any part of consciousness as an object of scientific inquiry already meant clearing the hurdle of Kant’s scientific epistemology, if Fechner’s undertaking was to be considered authentically scientific. At first glance, Fechner's conception of consciousness relative to the outside world falls squarely within a post-Kantian understanding of embodiment: perceptions, impressions, and sensations are correlated with an external reality but they are not correlative—one's experience of the world is always an experience firstly of one's senses. But this is not on account of Fechner's Kantianism (Heidelberger, 2004). Rather, Fechner takes up the bodily description of the physiological Kantianism of the likes of Müller, his contemporary, to go to war with not only philosophical Kantianism but neo-mechanistic materialism.

The key was Weber's study of just discernible differences. Ernst Weber, a teacher of Fechner, had attempted to experimentally measure bodily stimulus, specifically studying people's ability to distinguish objects of similar appearance but different weight (Fancher and Rutherford, 2016, 157). This amounted to a test of human sensory limit thresholds, a concept first introduced in 1824 by Johann Herbart (Gescheider, 2015, 1). Weber was not only able to establish that relative rather than absolute weight was the determining factor, he was able to identify that the smallest discernible difference between weights was consistently about three percent (Fancher and Rutherford, 2016, 158). Weber repeated this approach with other sensory tasks. When it came to discerning the difference in length between two lines, Weber found the smallest noticeable difference was by 1 percent, while for musical pitches the just noticeable difference between frequencies was approximately 0.6 percent (Fancher and Rutherford, 2016, 158).

Ernst Weber's experimental study of barely discernible differences in sensation would be elevated as a great leap forward for experimental psychology by the late nineteenth century, a legacy that has endured to the modern day. Yet, Weber himself, as well as many of his contemporaries, evidently understood his research as primarily concerned with physiology. Weber's eulogy in *Nature,* for example, plainly paints Weber as a talented and decorated physiologist (Nature, 1878, 286). Further still, Weber was an adamant, early, proponent of the idea that vital forces were entirely fictitious and that the body could only be understood in physical terms (Königsberger, 1906, 25). It was Ernst Weber whose philosophical attitude towards the science of physiology had most inspired the rise neo-mechanical revolution headed by du Bois-Reymond, Brücke, and Helmholtz (Königsberger, 1906). It is clear that Weber's experiments on the just-noticeable difference was intended to be descriptive of the holistic physiological function of the sensory organs in tandem with a physical brain, as opposed to psychological phenomena.

Weber's experiments were, without a doubt, profoundly influential for Fechner. When Fechner sought to mathematically describe the relationship between psychological and physical stimulus across multiple senses, Fechner called it Weber's law (Fechner, [1860] 1888, 17). The law in question was a formula wherein the intensity of the perceived stimulus (S) of a sensation is the logarithm of the physical input (P) multiplied by a constant (K), or $S = k \log P$ (Fechner, 1888; Fancher and Rutherford, 2016). These initial insights, building off the foundation set by Weber, formed the justification for the pursuit of psychophysical research as a science, as it satisfied the Kantian stipulations of mathematicity, apodictic certainty, and systematicity. Further still, Fechner absorbed Weber's method to develop a template for the measurement of all kinds of psychical events (Gescheider, 2015, 1).

However, Fechner differed markedly from Weber as to the underlying theoretical implications of these discoveries and, thus, they had radically divergent research objectives. Early on in his medical education, Fechner lost the Christian faith that had shaped his upbringing and had begun to approach the natural world mechanistically, a course that was suddenly changed by Fechner's exposure to the work of Lorenz Oken (Heidelberger, 2004, 22). For Fechner, his discovery of Lorenz Oken "suddenly shed new light on the whole world," invoking language reminiscent of a religious epiphany (Heidelberger, 2004, 22). So profound was the impact of Schelling-Oken for Fechner that he even sought a career as a *Naturphilosoph* all the way through his qualifying degrees. Struggling to find solid ground in *Naturphilosophie,* Fechner's naturphilosophic aspirations ended with frustration soon after Fechner finished his degrees, which precipitated a further commitment to physics and chemistry (though he soon found inspiration in the philosophy of Johann Herbart and the Romanticism of his friend Martin Gottlieb Schulze) (Heidelberger, 2004, 25-26, 35). Yet, as late as November, 1862 (well after the publication of *Psychophysik*), Fechner identifies himself as a *Naturphilosoph* in his personal diary, though it's not clear in what sense this was intended (Meischner-Metge, 2010, 416). In assessing the depth of any naturphilopshical influence as well as the theoretical foundations of psychophysics, it is worth briefly summarizing the underlying themes of Fechner's philosophical writings.

Fechner is not one of the many cases throughout history of a scientist setting down their work to become a philosopher (or vice versa). On the contrary, Fechner's extraordinary productivity saw him publishing influential works on psychological measurement alongside his philosophical works. A selected list of Fechner's philosophical writings includes: *Das Büchlein vom Leben nach dem Tode* (1836), *Ueber das höchste Gut* ("Concerning the Highest Good") (1846), *Zend-Avesta oder über die Dinge des Himmels und des Jenseits* (1851), *Ueber die physikalische und philosophische Atomenlehre* (1855), *Ueber die Seelenfrage* ("Concerning the Soul") (1861), *Die drei Motive und Gründe des Glaubens* (1863), and *Die Tagesansicht* *gegenüber der Nachtansicht* (1879). From the titles alone, it is clear that Fechner's interests far exceeded merely measuring sensation. Yet, topics as diverse as the eternal soul, the atom doctrine, and the basis for faith do find a certain degree of systematicity in the form of Fechner's doctrine of parallelism.

Fechner may personally trace his conception of psychophysical parallelism to Leibniz and Spinoza, with Fechner, or at least Fechner scholarship, possibly originating the attribution of the label of parallelism to Spinoza (Fechner,1888, 5; Yakira, 2010, 106). Fechner's parallelism argued that the psychical, mental, or spiritual existed independently of the body, but alongside and in close conjunction with the physical. Unlike Spinoza, Fechner's mind-spirit and body are not different expressions of an intermediary substance. For Fechner, the body is the seat of the soul; they are one and the same. To this point, Fechner did recognize Leibniz in his *Psychophysik,* but he also did not subscribe to the Leibnizian doctrine of preestablished harmony (Fechner, 1888, 1). Rather, Leibniz and Fechner share a basic underlying principle of functional congruity between the body and the mental/spiritual, with Fechner arguing that the psychical and physical share a common basis in the individual organism. In later works including *Zend-Avesta*, Fechner raised psychophysical parallelism to its most panpsychistic formulation, with Fechner proposing that the entirety of the physical world has a psycho-spiritual parallel (Fechner, 1901, vi; Heidelberger, 2004, 172-173).

This principle of psycho-spiritual served as the foundation for both Fechner's argument for the existence of the eternal soul and the atom doctrine. Fechner's 1855 *Atomlehre* is an interesting topic in light of the divisiveness surrounding atomism at the time, with the thought of Schelling and Hegel coming up against physicists such as Fresnel and Cauchy. Some German materialists struck out in support of atomism, as Karl Vogt had with in 1855 *Koehlerglaube und*

Wissenschaft—but Fechner's atomism is decisively anti-materialistic. Fechner's approach to atomism reflected his own psychophysical ontology. Fechner was a point atomist, meaning he subscribed to the notion that an atom was a centre of force or a monadological 'point' and that such points of force made up the entirety of the physical world (Fechner, 1855, 33-36; Heidelberger, 2004, 138, 147). Of course, atoms only comprise the most rudimentary unities in Fechner's ontology. Atoms themselves are not souls, nor do they have souls. Fechner saw souls in the law-likeness of the behaviour of what he perceived to be organic wholes: humans, animals, plants, as well as planets, leading up to the essentially unity of all things in God. This was not intended in the sense of an unconscious reflexivity; Fechner saw living consciousness reflected in the behaviour of all of these dynamic wholes. In Fechner's psychophysical ontology, this meant the sum of atomic reality is just the bodily accompaniment of the universal psycho-spiritual. Individual souls, in this sense, live on after death insofar as they were always only a specific unfolding of the universal soul and thus can never be destroyed, just as the atoms perpetually cycle through the physical world.

Although Fechner's philosophical, or theoretical, works and Fechner's scientific research were published separately, they are inextricably intertwined. Experimentally demonstrating a systematic relationship between the bodily and the psychical had been the grand motivation behind Fechner's description of Weber's law. Theoretically, Weber's law not only demonstrated that the psychical was studiable it made real the connection between the physical and the mental-spiritual as an object of scientific rather than philosophical inquiry, an objective relayed in the opening lines of *Elemente der Psychophysik* (Fechner, 1888, 1). One of the most striking expressions of Fechner's approach to mechanistic thinking in science was the opposition be developed between what he called the *Tagesansicht* and the *Nachtansicht* (Adler, 1991, 10).

Mechanism and physiological reductionism was what he termed the *Nachtansicht*, a term seemingly alluding to Goethe's own evaluation of Baron d'Holbachs mechanistic *Système de la nature* as depicting a hollow, lifeless, and twilit world (Goethe, 1811, 490). The contrast between *Tagesansicht* and *Nachtansicht* has roots in the very beginnings of Fechner's psychophysical project, with the *Tages-* and *Nacht-ansichte* reflecting the double-aspect of the psychical and physical first introduced in *Zend-Avesta.* Insofar as Fechner argued for the parallel, non-reductive functioning of the mind and body, the vacuity of the *Nachtansicht* lay in its fundamental rejection of precisely this underlying duality, at the expense of the possibility of any truly meaningful scientific inquiry. The science of the *Tagesansicht* needed to reflect the unity that underlies the psychophysical reality, a position that securely identifies Fechner's scientific research program as a radical empiricist manifestation of the *Naturphilosophen* that inspired Fechner early in his career.

Materialism and Anti-Materialism

The materialist and anti-materialist debates that surrounded the legacy of Gustav Fechner were as real to his contemporaries as they are today. There are some scholars who are hesitant to identify Fechner as an anti-materialist. Michael Heidelberger, who has written several impressive books on the life and work of Gustav Fechner, argues that Fechner is a non-reductive materialist:

> The position[(Fechner's)] is nonreductive, because it describes life and consciousness as having an independent, original nature that cannot be further reduced to physical phenomena, or— as the nineteenth century would have it— reduced to "the mechanics of atoms." Yet, at the same time, Fechner's position is materialistic. He sees every change in the physical world as wholly explicable by laws of nature, and he sees for every mental change some change in the physical world that precedes it (Heidelberger, 2004, 73).

In many senses, this is an excellent argument. It washes away the position held by some modern scholars and some of Fechner's contemporaries that Fechner was something of a empirically minded scientist at work and a philosopher on the side. The connection between his philosophical and methodological approaches formed the basis of Fechner's entire psychophysical research program. Furthermore, Heidelberger's evaluation of Fechner as a non-reductive materialist helps bring Fechner's dayview approach to science and radical empiricism to the forefront of his portrait of the thinker. But to ascribe the materialist label to Fechner in any capacity is already to undermine the psychophysical parallelism that forms the foundations of his work. Material, atomic reality is, in every sense, at one with the psycho-spiritual parallel; they are one and the same, neither existing without the other. It was precisely this outlook that made Fechner an object of ridicule from the younger generation of neo-mechanically minded physiologists.

Fechner's philosophical positions, as well as the role they played in his psychophysical work, were plain to see. Although Fechner comes across as fairly unapologetic in his anti-materialism, Fechner's diaries show that he was nevertheless concerned with his ideas gaining acceptance among his peers (Meischner-Metge, 2010, 417). Unsurprisingly, the reception of Fechner's ideas amongst the neo-mechanists could hardly have been worse. As early as 1849, letters between Carl Ludwig and du Bois-Reymond on Fechner's work contains a tone of what can only be described as mockery, du Bois-Reymond going as far as suggesting that Fechner must suffer from an organic brain tumor (du Bois-Reymond, 1847/1927; Meischner-Metge, 2010, 417). Fechner, 2 months and 26 days Johannes Müller's senior, would have seemed to the younger generation of Berlin physiologists a vestige an era that was swiftly fading into the distance. Even when Carl Ludwig spoke of him warmly in the letter, Ludwig spoke of Fechner as one might an amiable, yet kooky, old man (du Bois-Reymond, 1847/1927; Meischner-Metge, 2010, 417).

At this point, it is worth asking if the degree of the private hostility and dismissiveness of du Bois-Reymond and others towards Fechner's ideas were affected by perceptions of vitalistic impulses in Fechner's thinking. The German Romantics and *Naturphilosophen* had integrated vitalistic concepts into their quasi-spiritual cosmological dialectic and many of these ideas made a lasting impression on Fechner, arguably a life-long impression. Fechner's psychophysical doctrine, and with it the *Tagesansicht* in science, is undoubtedly vitalistic. The psycho-spiritual parallel, existing in tandem with the physical and yet enduring beyond it, could even be mistaken for a non-material vital force. Yet, psychophysical parallelism could hardly be described as a theory of vitalism, but rather as a vitalistic ontology in the spirit of the *Naturphilosophen.* But what if that was not the full extent of the Fechner's vitalism?

Gustav Fechner and the Cure for Opium Poisoning

Between the years 1834 and 1838, Gustav Fechner was in a period of financial desperation. As he had before, Fechner sought to earn a living by writing, this time securing a job as the editor and primary author behind the *Hauslexikon: Vollständiges Handbuch praktischer Lebenskentnisse für alle Stände.* Given the breadth involved in what Fechner described as a guide to life as relayed by the "neuste und beste Quellen," the *Hauslexikon* is a unique source of insight into Fechner's approach to a vast and varied array of subjects (Fechner, 1834, III). Fechner personally wrote over a third of the entire 8 volume encyclopedia. For those portions which he did not personally write, Fechner did edit their content and approve them for publishing, providing at least some semblance of tacit endorsement for the ideas present within. (Fechner, 1841, 103). Thus, when references to vitalistic theories of medicine appear throughout the *Hauslexikon,* they merit some degree of serious consideration.

Not only do a number of the medical entries in the *Hauslexikon* reflect vitalistic theories of bodily function, there are overt references to Brunonian concepts, the dictates of the *Hauslexikon* contained examples of the vital substance concept. The following passage addressed the appropriate medicine to provide in the context of a difficult childbirth, amongst other circumstances:

> Daher ist Kaffee ein unentbehrliches Heil- oder vielmehr Linderungsmittel in allen Zufällen von überreizter Lebenskraft, welche er in seiner Nachwirkung herabnimmt. Er ist dem Opium vorzuziehen in allen Schmerzen, wo die Kranken laut schreien u. heulen - in Ueberreizung der Wöchnerinnen bei zu heftigen Geburts (Fechner, 1841, 272).[70]

Here, the curative efficacy of both coffee and opium are aligned with their capacity to affect the vital force, even sporting a characteristic word of caution arising from the extent of opium's vital effects. Elsewhere, steam bathes are described as being medicinal because "der ganze Körper ist so heiß, u. alle Lebenskrafte sind so angeregt, daß der Eindruck der äußeren Kälte sogleich besiegt wird, u. seine anderen Folgen hat, als eine vermehrte Reaction des ganzen Organismus" (Fechner, 1841, 411).[71] Both of these examples testify to the suggestion that Fechner at least tacitly supported concepts imported from a Brunonian or Röschlaubian model of vital activity which entailed some conception of vital substances.

What is known is that Fechner had more than a passing familiarity with the literature surrounding the emerging category of alkaloids. Fechner's translation of Thenard, titled

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[70] Therefore, coffee is an indispensable remedy or rather palliative in all cases of overstimulated vitality, which it reduces in its after-effects. It is preferable to opium in all pains where the sick scream and howl loudly - in overstimulation of women in childbirth in the case of too violent births.

[71] […] "the whole body is so hot, & all vital forces are so stimulated, that the impression of external cold is immediately defeated, & has its other consequences, as an increased reaction of the whole organism."

Repertorium der organischen Chemie, contained a review of the literature on meconic acid as well as the adjacent discovery of morphine, discussing publications by Sertürner, Vogel, Robiquet, Thenard, Choulant, and Seguin (Fechner, 1826, 238). The *Repertorium* also contains passing references to *Lebenskraft*. Fechner added the commentary that Nicholas de Saussure found that Jacques Étienne Bérard erred in his finding that fruits gas off more during the day because the experimental design exposed the fruit to daytime conditions that depleted their *"Lebenskraft"* (Fechner, 1826, 45). Fechner himself makes passing, perhaps rhetorical, use of the term "*Lebenskraft"* in *Vorschule der Aesthetik* (Fechner, 1876, 124, 215). None of the examples provided above could be described as definitive evidence of mainstream vitalistic in the thought of Gustav Fechner. What these references demonstrate is that Fechner had a familiarity and association with not only theories of vitalism but Brunonian aetiology and therapeutics with vital stimulants.

All of these points provide a backdrop against which to understand the discussions of intoxicating substances in Fechner's major works. Here, Fechner's references to intoxication are infrequent, albeit reflective of an underlying framework. Speaking to research on the ability to perceive the distance between two points on the skin, Fechner remarks that "[n]ach Chloroformirung oder Einnahme narkotischer Stoffe (Morphin, Atropin, Daturin) mussen die Zirkelspitzen ausserordentlich viel weiter gestellt werden, als sonst, um sie noch als distant zu empfinden" (Fechner, 1889, 323).[72] The research Fechner cites here comes from Lichtenfels and Fröhlich, who "durch ausführliche und mehrfach abgeänderte Versuche [the effects of

[72] […] "after chloroforming or ingestion of narcotic substances (morphine, atropine, daturin), the tips of the circles must be placed much wider than usual in order for them to be perceived as distant.

intoxicants] gezeigt haben" (Fechner, 1889, 323).[73] Interest in such research is to be expected: it specifically concerns the measurement of a bodily sensation. But not all of Fechner's references are so clinical.

In fact, the majority of the references to substances of intoxication found throughout Fechner's *Psychophysik* do not directly concern the measurement of the sense whatsoever, instead appealing to the phenomenological, lived experience of intoxication.

> So schreibt Granier de Cassagnac von der Wirkung des Aetherisirens: "Es war mir, als ob alles Aeussere verschwände; ich fühlte nicht mehr das Flacon in meiner Hand, bemerkte kaum, dass ich Kleider am Leibe hatte, und der Boden, auf welchem ich stand, schien mir seine ursprüngliche Realität verloren zu haben .... die äussere und materielle Welt ist nicht mehr vorhanden[…] Ein anderer Beobachter giebt an : „Ich empfand von der Aussenwelt überhaupt, ja von meinem eigenen Körper nichts mehr. Die Seele war gleichsam ganz isolirt und getrennt von dem Körper." […] So schreibt Madden (Fror. Not. XXVI, S. 14) von der Wirkung eines Opiumrausches: "Im Gehen bemerkte ich kaum, dass meine Füsse die Erde berührten; es war mir, als glitte ich , von einer unsichtbaren Kraft getrieben , die Strasse entlang, und als ob mein Blut aus irgend einem ätherischen Fluidum bestände, das meinen Körper leichter machte, als die Luft."- Ein anderer Beobachter sagt von der Wirkung des Haschisch +): "Die hervorgerufenen Sensationen waren eine so ausserordentliche Leichtigkeit, so zu sagen Luftigkeit," .... und weiter: "die Limitationsempfindung (das Gefühl der Begränzung innerhalb der Schranken von Fleisch und Blut) fiel augenblicklich weg. Die Mauern des organischen Leibes barsten und stürzten in Trümmer, und ohne zu wissen, welche Gestalt ich trug, da ich das Gesicht, ja jede Vorstellung von Form verlor, fühlte ich nur, dass ich mich zu einem unermesslichen Raumumfange ausgedehnt habe (Fechner, 1889, 326).[74]

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[73] […] "have shown through extensive and repeatedly modified experiments [the effects of intoxicants]." Lichtenfels and Fröhlich's paper "Ueber den Puls als ein Symptom, sowie als numerisches Maass der physiologischen Arzeneiwirkung" is cited here as being published in 1857 as opposed to 1851. It attempts to associate visible changes in pulse with specific conditions, including narcotic intoxication.

[74] Thus Granier de Cassagnac writes of the effect of etherization: "It was to me, as if everything outside disappeared; I felt no longer the flacon in my hand, hardly noticed that I had clothes on the body, and the ground, on which I stood, seemed to me to have lost its original reality .... the outer and material world is no longer present[...] Another observer indicates: "I felt from the outside world at all, even

Fechner likens these sensory effects to a form of paralysis of the function of sensory nerves in the skin, even to a kind of death (death being the great paralysis) (Fechner, 1889, 327). Notably, none of these scraps of testimony were the product of Fechner's first-hand research, but were rather pulled together from accounts of various sorts. The hashish narrative, for example, came from a 1854 account found in *Magazin für Literatur des Auslands* (Fechner, 1889, 326). This suggest that, although the study of the effects of intoxicants fell outside of his own research program of measuring sensation, Fechner regardless found the information gleaned from such accounts to be scientifically meaningful.

With an eye to the course taken by neo-mechanists with regard to the nature of the connection between the body and the mind, Fechner's radical empiricism stands out for its willingness to take on the testimony of experiences that might otherwise be described as being plucked from the murky dreamworld of perception. Like John Brown or Friedrich Sertürner, Fechner appears to have recognized that the holistic, embodied experience of intoxication

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from my own body nothing more. The soul was, as it were, completely isolated and separated from the body". Madden (Fror . Not . XXVI , p. 14) writes of the effect of opium intoxication: "While walking, I hardly noticed that my feet touched the ground; I felt as if I were gliding along the street, driven by an invisible force, and as if my blood consisted of some ethereal fluid that made my body lighter than the air."- Another observer says of the effect of hashish +): "The sensations produced were such an extraordinary lightness, so to speak airiness," .... and further: "the limiting sensation (the feeling of confinement within the boundaries of flesh and blood) fell away instantly. The walls of the organic body burst and fell into ruins, and without knowing what form I was wearing, since I lost the face, even any idea of form, I only felt that I had expanded to an immeasurable circumference of space.

imparted information that was irreducible to any number of physiological processes. Looking to the foundations of Fechner's psychophysical thought, it is clear that Fechner's radical empiricism is drawn from his naturphilosophical background and perhaps even that his openness to experiences such as intoxication were shaped by a latent influence of the vital substance concept. The result was that Fechner's psychophysics functioned as a restrictive science of embodiment. Still principally concerned with measurements of sensation, Fechner's work demonstrates that was as yet theoretically open to information gleaned from the sum of perceptional experience. This is only made clearer when contrasted with the neo-mechanists, for whom embodiment was a superficial distraction from the more fundamental physiology of sensation. The issue for Fechner was that he had a framework, but lacked a distinct methodology: Fechner's psychophysics was still firmly planted in the methods of physiology. The innovation of Fechner's approach was to project the findings of Weberian sensory physiology onto a psycho-spiritual entity. The shift would occur when Wundt took up Fechner's framework and imbued it with a radical new methodological approach, one which led to the emergence of a new independent science of embodiment.

Wundt's Science of Embodiment and the Shift from Psychophysics to Psychology

On its face, many aspects of a pre-1875 biographical sketch of Wilhelm Wundt's life would portray Wundt as an unlikely candidate to carry Fechner's legacy into the future. Pages could be filled musing about the kind of research Wundt might have lead had his early electro-muscular work had been well received by du Bois-Reymond, or even had he not been passed over to take the position of his former mentor, Hermann von Helmholtz, in 1870. But these things did happen, and Wundt's legacy as the most influential figure in the history of scientific psychology is today all but assured. However, the development of Wundtian psychology was far from straightforward. Beginning as a physiologist under the mentorships of Helmholtz, Müller, and du Bois-Reymond, the emergence of Wundt's earlier psychological theories was as much an unfolding of neo-mechanistic physiology as it was an opposing position. It would be the influence of Lotze; Herbart; Weber; and, above all, Fechner's *Psychophysik* which would collide with Wundt's personal struggle in academic physiology to ultimately give rise to Wundtian parallelism. This partial parallelism, grounded in a secularization of Fechner psycho-spiritual/material divide, was the foundation of the argument for the requirement that psychology be formulated as its own experimental science. Before this could be done though, Wundt needed to develop a foundational research program through which to ground the validity of the broader project of scientific psychology, and for that Wundt would choose reaction time, a choice that would put Wundt in direct conflict with his former peers in physiology. This new experimental approach to psychology would establish Wundtian psychology as a science of embodiment, concerned above all else with the lifeworld of the subject over and against an unseen physiological substrate.

Wilhelm Wundt was born in 1832 to Max, a Lutheran preacher, and Marie Wundt in Neckerau, a town nestled inside a bend in the river Rhine just south of Mannheim (Jones, 1994, 49). The young Wilhelm was a bright, yet lonely and highly imaginative, young boy. A pronounced propensity for daydreaming has even led some of his biographers to find great significance in Wundt's childhood fantasies (Boring, 1950). Like Fechner, Wundt lost his priestly father when he was young. In Wundt's case, his father died when Wundt was in his first year of Gymnasium, a potential factor in his less than spectacular success there (Diamond, 2001, 12). Wundt began his medical degree at Tübingen in 1851, before ultimately moving to Heidelberg in 1852 and continuing his studies there until 1855 (Jones, 1994, 49). In 1856, Wundt had the opportunity to spend a semester in Berlin studying physiology with du Bois-Reymond and Johannes Müller, before returning to Heidelberg to complete a medical doctorate and habiliate (Boring, 1950, 318). With Müller, Wundt had been tasked with "extirpating nerve centres in invertebrates" while du Bois-Reymond had Wundt assessing the merits of Ernst Weber's 1846 findings concerning the effects of varied loads on muscular extension and contraction (Diamond, 2001, 20). Upon returning to Heidelberg, Wundt lectured on physiology and, from 1858-1865, worked as an assistant to the newly appointed head of the physiological institute, Hermann von Helmholtz (Boring, 1950, 318). It seemed that Wundt had completed the set of German organic-physicists and this period saw Wundt publish a number of physiological texts, among them "Ueber die Elasticität der thierischen Gewebe" (1857), *Die Lehre von der Muskelbewegung* (1858), and "Ueber das Gesetz der Zuckungen und die Veränderungen der Erregbarkeit durch geschlossene Ketten" (1858).

Up to this point, Wundt's biography reads like a perfect template for a successful career in academic physiology in mid-nineteenth century Germany. Wundt could almost be counted among Johannes Müller's students, all of whom by then occupied the top positions in medical and physiological faculties across the German-speaking world. Yet, it was in the years that followed that Wundt made his debut as a psychologist with the publication of the first of the articles that comprised *Beiträge zur theorie der Sinneswahrnehmung* (1858-1863), followed by a series of lectures later published as *Vorlesungen über die Menschen- und Tier-Seele* (1863). While Wundt was still associated with the physiological institute in Heidelberg, Wundt's *Beiträge* marks the beginning of the trajectory that would see him founding the first laboratory for experimental psychology roughly a decade later in Leipzig.

The intellectual origins and personal reasons for this shift in Wundt's interest remain, to this day, a subject of considerable scholarly debate. The tendency of many histories of Wundt has been to emphasize the philosophical foundations of his psychological approach. A complicating factor is that Wundt received no formal philosophical training. Wassmann (2009) makes a compelling case for an appraisal of Wundt's early psychological work in light of his established physiological training. Rather than speculate as to the depth of the philosophical influences on early Wundt, Wassmann points to Wundt's concepts of brain function and emotion as examples of how the foundations of Wundt's work can be found in the physiological research that surrounded him (Wassmann, 2009, 215). The tremendous merit of such an approach is that it resists the scholarly temptation to read someone's earlier work in light of their later work, an enduring example of this problem can be seen in the scholarly treatment of Freud's early anatomical and neurophysiological research with Claus, Brücke, and Meynert.

But, Wundt's short career in physiology was hardly the idyll it might seem at first glance, and the years that surrounded the publication of Wundt's early psychological work were coloured by professional disappointment and feelings of betrayal. In 1858, Wundt had published his first book as a physiologist, *Die Lehre von der Muskelbewegung*. Drawing on some of the research done under his mentorship in Berlin, the *Lehre* was dedicated to du Bois-Reymond. Much to his supreme disappointment, Wundt's debut book received little to no recognition, even by the man to whom Wundt had dedicated the book (Wundt, 2013; Diamond, 2001, 23). Wundt attributed this failure to a conspiracy led by nonother than his former mentor himself (Wundt, 2013; Diamond, 2001, 23). This falling out with du Bois-Reymond, feelings of betrayal from those at the top of the field, as well as a professional conflict with Hermann Munk in 1861 provide more than sufficient grounds to ask what kind of future Wundt saw for himself in physiology. To the credit of those who see a philosophical influence in Wundt's turn to psychology, Wundt himself was more than happy to impart this impression. The title page of Wundt's *Beiträge* even bares a quote from Leibniz in Latin (Wundt, 1862). There is little room to deny philosophical influences in Wundt's early psychological turn, not to mention that Müller, du Bois-Reymond, and Helmholtz were themselves profoundly influenced by philosophy. However, it is worth first addressing the arguments for Wundt's turn to experimentalist science and then the possible nature of Wundt's psychological turn.

Looking to the experimental philosophy of the *Beiträge* and *Vorlesungen,* there is a strong tradition of attributing a significant influence on Wundt's experimentalism to John Stuart Mill (Boring, 1950; Bistricky, 2013). As Araujo (2016) points out though, Wundt's own citations of the 1849 German translation of Mill's *Logic* are overwhelmingly accompanied by criticisms of Mill's inductive method (Araujo, 2016, 65). Wundt's own memoirs instead emphasized the influence of Robert Bunsen, who was a professor at Heidelberg during Wundt's education, for his early interest in experimentation (Wundt, 2013, 70-71). Though this helps narrativize the conditions leading to Wundt's transition from medicine to physiology, Wundt hardly needed a Mill or even a Bunsen to develop an implicit belief in the merits of experimentalism: it was the spirit of the age, everywhere around him throughout his medical and physiological education. It would have been far more remarkable, given his social and institutional context, if Wundt had been an anti-experimentalist.

What then of the foundations of Wundt's shifts to psychology? One particularly persistent attribution of intellectual indebtedness accords much of Wundt's early psychological thinking to the influence of Gottfried Leibniz (Fahrenberg, 2016). Klempe (2021) makes the argument that Wundt's debt to Leibniz draws primarily from Leibniz's mathematical thought, in particular the Leibnizian critique of mechanical causality and physicalist dynamics (Klempe, 2021). It is worth considering if any recognition of intellectual indebtedness on the part of Wundt himself are at least partially retrospective. Wundt's later publications do identify Leibniz as an important influence in his research program, though this increased emphasis on the significance of Leibniz is seen after the year 1900 (Fahrenberg, 2016, 13). In the 1850s and 60's, Wundt was clearly interested in Leibniz, but, as Araujo suggests, the extent of this early influence is fragmentary. For example, Wundt's discussion of the Leibniz quotation that opens the *Beiträge* was intended to show the (ambiguous) middle ground Wundt was assuming between Leibniz's innatism and Locke's sensualism (Araujo, 2016, 60). Alternatively, efforts to paint the young Wundt as essentially a Herbartian are themselves mired by profound contradictions between Wundt and Herbart with regard to psychology. This association is understandable, as Wundt himself cited Kant and Herbart as being amongst his greatest influences in *Grundzüge der physiologischen Psychologie* (Wundt, 1874a, 3). Yet, Herbart, quite famously, opposed the possibility of experimentation in human psychology, the very program Wundt sought to realize (Herbart, 1850, 4).

There is also the matter of the theoretical influence of Helmholtz. Much has been made of Helmholtz's legacy in the history of psychology, both in regard to his work on the physiology of perception as well as his theory of unconscious inference. As has already been discussed, Helmholtz's approach to the physiology of the senses, as well as the nervous system, was bound up in the neo-mechanical physiology of the body, rather than any explicit psychological research program. Though the historical impact of Helmholtz's work on the physiology of perception and nervous activity is important for modern neuropsychology, there is no reason to suspect Helmholtz's influence was of primary significance to Wundt's early psychological project. On the contrary, though the Helmholtz and Wundt refer to each other respectfully in their work, their relationship has been described as "uncongenial" (Titchener, 1921, 162). If not uncongenial, the relationship itself did not bear much fruit. Ivan Sechenov, a student working in the laboratory in which Wundt was an assistant, reports that Wundt sat in the company of his books in near utter silence (Diamond, 2001, 28). Wundt even seemed hesitant to make use of the relationship he had with as eminent a figure as Helmholtz for his own professional development (Diamond, 2001, 28).

It is clear is that Wundt was evidently influenced by the philosophies of Kant, Leibniz, and Herbart, but that their inspiration for Wundt's psychological turn were likely fragmentary and complimentary to the more immediate influences of Ernst Weber and Gustav Fechner. Both the *Beiträge* and the *Vorlesungen*, the defining works of Wundt's turn to psychology, came into their final published forms over several years, in the midst of which Fechner published his *Psychophysik.* This makes it relatively simple to trace the impact of *Psychophysik* by examining those components of the *Beiträge* or the *Vorlesungen* published from 1860 onward. One finds that the influence of Fechner was not one directional, flowing from the older Fechner to the younger Wundt. Instead, it was more of a feedback loop. Wundt's pre-1860 psychological articles in the *Beiträge*, which took its influence from Weber, was warmly recognized in Fechner's *Psychophysik* and Wundt's work was subsequently influenced by Fechner's then more developed psychophysical approach.

The "Erste Abhandlung" of Wundt's *Beiträge*, published in 1858, focused on the sense of touch and space perception. There can be no denying the Weberian influence on the treatise. Wundt himself identifies Weber's experimental approach as a primary inspiration for his own project (Wundt, 1862, 1). Though some of the ideas present in the article resemble Helmholtz's concept of unconscious inference, the “Erster Abhandlung” is unlikely to bare any Helmholtzian influences. As Araujo points out, Helmholtz himself did not assume his post in Heidelberg until October 1858, meaning the first portion of the *Beiträge* could not have been written under Helmholtz’s supervision (Araujo, 2014, 53). Although Wundt freely takes on Weber’s approach, the initial purpose of Wundt’s ambitious project was to actually demonstrate that Weber’s theory of tactile localization was incorrect, building on the criticisms of Kölliker and Lotze (Wundt, 1862, 5, 8). But Wundt doesn’t stop there. The “Erster Abhandlung” turns a critique of Weberian tactile perception into a broader framework for approaching further questions in the physiology of perception as such.

On this point, Wundt quotes Theodor Waitz, saying “Das Wesen der Seele widerspricht der gleichzeitigen Auffassung eines Mannigfaltigen, und gerade das Unvermögen zu dieser ist es, durch welche sie gezwungen wird, das Mannigfaltige, das ihr zu gleich gegeben wird, neben einander zu setzen (Wundt, 1862, 10).[75] “Hierin,” argues Waitz, “liegt der Ursprung der Raumvorstellungen” (Wundt, 1862, 10).[76] Wundt pairs this idea with Lotze’s observation “dass die Raumanschauung ein der Natur der Seele ursprünglich und a priori angehöriges Besitzthum sei,” which Lotze understands physiologically through his concept of *Lokalzeichens* (local signs) (Wundt, 1862, 12-13).[77] Wundt takes these observations as instructive concerning the entire host of available sense perceptions to argue that some unconscious process mediates the passage from

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[75] […] “The essence of the soul contradicts the simultaneous conception of a manifold, and it is precisely the inability to do so that compels it to place next to one another the manifold that is given to it at the same time”

[76] “Herein lies the origin of special conception.”

[77] […] “that the perception of space is original, *a priori* property belonging to the nature of the soul.”

sensations into perception, a notion familiar to Kantians everywhere (Wundt, 1862, 65). Though tempting to interpret this model as a forerunner to Wundt's psychological parallelism, there is no clear evidence that Wundt's 1858 'unconscious process' is any less physiological than du Bois-Reymond's, Helmholtz's, or Müller's neo-Kantianism on similar questions. For this reason, Wundt's "Erster Abhandlung" can hardly be called psychology in the sense of his later work—there is no necessity of a psychological parallel. It is a physiology of sensation in the methodological tradition of Ernst Weber, articulated by way of Waitzian philosophy. Both published in 1859, the second essay of the *Beiträge* was a historical review of different theories of vision and the third was a discussion of monocular vision, relying on the framework introduced in the "Erster Abhandlung".

The first three sections of Wundt's *Beiträge* seems to have garnered limited attention. A notable exception to this cool reception was the glowing praise that Wundt's *Erster Abhandlung* received from Fechner in *Psychophysik* (Fechner, 1888, 296; Fechner, 1889, 315, 317, 323). Given the painful anxieties of rejection and perceptions of conspiracy that had plagued the immediately preceding years, Wundt very likely would have been receptive to such recognition from a senior figure such as Fechner. Wundt may have also already been somewhat aware of Fechner's work pre-*Psychophysik.* Much of Helmholtz's writing of the *Handbuch der physiologischen Optik* (1856-1867) overlapped with Wundt's assistantship and Helmholtz relied heavily on Fechner's research on contrasting colours and afterimages, citing Fechner throughout the text (Helmholtz, 1867, 313, 387, 403, 418, 542, 793, 836, 868). The fourth and fifth essays of Wundt's *Beiträge* continued the theme of the preceding sections and now discussed the physiology of binocular vision, so Wundt and Helmholtz would have been consulting many of the same sources while working in the same lab. In any case, both

Wundt's sixth and final essay and the introduction added in 1862 once the collection of the essays were published as a book leave no question as to the profound impact of Fechner's *Psychophysik*, replete as they were with references to the old man of Leipzig.

The fourth and fifth articles had relied on Fechner's psychophysical studies to assess the role of attention and other factors in the function of visual perception. The "Sechster Abhandlung," in contrast to the preceding sections, is overtly psychological, or philosophical, in its objectives, though it seeks to ground its conclusions in the principles derived from the preceding visual studies. Here, Wundt overturns Herbart's thesis that competing ideas exist in the conscious mind simultaneously to argue that conscious thoughts first undergo an unconscious unification process (Wundt, 1862, 382). By Wundt's reasoning, Wundt had demonstrated this phenomenon in spatial perception, where he had argued that the conflict between the sum of different, competing stimuli and the unity of perception entailed an unconscious law-like synthesis process. This was Wundt's attempt to empirically demonstrate the psychical fact of an unconscious law-like, logical process, aided by psycho-physical research pioneered by Weber and then Fechner (Wundt, 1862, 416-417).

The introduction to the completed collection heaps praise upon Fechner as the one who above all pioneered an experimental and theoretical method of approaching the barrier between the physical and the psychical (Wundt, 1862, xxx). Most important was the recognition "dass [Fechner's law] in der That nicht ein physisches Gesetz ist," which, Wundt argues, meant "dass dasselbe Gesetz auch im Gebiet der höheren psychischen Thätigkeiten seine Gültigkeit behält" (Wundt, 1862, xxx).[78] This extension of Fechner's thesis, which Wundt suggests is apparent

[78] […] "that [Fechner's law] is in fact not a physical law," which, Wundt argues, means "that the same law retains its validity also in the field of higher psychic activities."

upon common sense reflection, argued that Fechner's law was not merely psychophysical but rather a psychical law. Thus, Wundt translated the Weber-Fechner law of dependence between stimulus and sensation into a psychological law, which stated "dass wo zwei psychische Funktionen in unmittelbarer Abhängigkeit von einander stehen, die abhängige Funktion immer wächst proportional dem Logarithmus der ursprünglich veränderlichen (Wundt, 1862, xxxi).[79] Wundt had modified Fechner's logarithmic relationship between sensation and stimulus in order to define a connection between perception and sensation. All of higher mental life—as in spatial perception—now functioned in accordance with an underlying logic, a law-like process which could be empirically encountered.

Perhaps most importantly, this represents the clear emergence of psycho-physical parallelism in Wundt's thinking. Wundt had, somewhat vaguely, alluded to the weakness of strict neo-mechanism in describing organic beings, in favour of some yet unknown future framework, as early as his introduction in *Die Lehre von der Muskelbewegung* (Wundt, 1858a, 2). In the completed *Beiträge,* Wundt openly demonstrates the early development of his own doctrine of psychological parallelism, which disinvested the psychological from any physiological dependence. This idea would see significant further development and formalization in Wundt's famous *Grundzüge der physiologischen Psychologie* in 1874. Though, Wundt's 1862 *Beiträge* demonstrates that Wundt had the foundation of his own conception of a novel approach to psychology as an empirical science of *embodiment* as early as the 1860s, even if he would later overturn nearly all of his initial theses.

[79] […] "that where two mental functions are directly dependent on each other, the dependent function always grows in proportion to the logarithm of the original variable."

In his autobiography, Wundt remarks that it was in error that “Ernst Heinrich Weber hat der um wenige Jahre jüngere Fechner den ‘Vater der Psychophysik’ genannt,” but the “Schöpfer der Psychophysik ist jedenfalls Fechner selbst” (Wundt, 2013, 280).[80] From afar, Wundt experienced these developments in the gradual unfolding of his own *Beiträge*, which drew first on Weberian proto-psychophysical physiology and then witnessed the realization of his own psychological dualism through the psychophysics of Gustav Fechner. Yet, despite undergoing significant personal and professional transformation, Wundt appears to have been stuck at a crossroads. He continued to develop his psychological approach, while also pursuing further physiological work. Wundt gave lectures on psychological themes which would later be published as *Vorlesungen über die Menschen und Thierseele* in 1863, though he also published the first volumes of *Lehrbuch der Physiologie des Menschen* in 1864. By 1865, Wundt was seeking to get out of the research assistantship with Helmholtz, as relayed through a letter from Helmholtz to du Bois-Reymond, dated February 13, 1865 (Araujo, 2014, 54).

Wundt was finally free to focus on his duties as a “extraordinary professor” when Helmholtz selected the promising young physiologist Julius Bernstein as Wundt’s replacement in 1865 (Araujo, 2014, 54). Wundt continued to lecture on physiological, philosophical, and psychological topics, as well as lecturing on anthropology, which Wundt had identified as an important source of psychological insight in the *Beiträge* (Wundt, 1862; Diamond, 2001). To list just a few, in the years between 1865 and 1871 Wundt published

[80] “Ernst Heinrich Weber called Fechner, who was a few years younger, the 'father of psychophysics'," but the "creator of psychophysics is in any case Fechner himself."

“Die physikalischen Axiome und ihre Beziehung zum Causalprincip. Ein Capitel aus einer Philosophie der Naturwissenschaften” (1866), “Ueber einige Zeitverhältnisse des Wechsels der Sinnesvorstellungen” (1866), *Handbuch der medicinischen Physik* (1867), “Neue Leistungen auf dem Gebiete der physiologischen Psychologie” (1867), and “Ueber die Entstehung räumlicher Gesichtswahrnehmungen” (1869).

The Janus moment in Wundt’s career seems to be a series of events in 1870-1871. In the span of less than two years, Helmholtz left Heidelberg for Berlin, Wundt was passed over as Helmholtz’s replacement as the head of the physiological institute, and much of Wundt’s work was subjected to intense physiological scrutiny by Julius Bernstein. By all accounts, Wundt still saw himself as a physiologist as late as 1870. If anything, Wundt’s interest in Helmholtz’s position in the first place is demonstrative of Wundt’s continued identification with physiology, even if only aspirationally. In fact, the Wundt of the early 1870s outwardly seems more determined to continue his career as a physiologist than he had in the preceding years, publishing an ambitious project titled *Untersuchungen zur Mechanik der Nerven und Nervencentren* (1871). Diamond credits this renewed effort to make a mark in neurophysiology to the opportunities afforded by Heidelberg’s rising prestige, owing to their accomplished faculty members (Diamond, 2001, 51). Though, it is worth recalling that Wundt’s *Mechanik der Nerven* was being placed on a rising wave in neurophysiology. Hitzig and Fritsch’s findings on the location of the motor cortex in dogs had been published in 1870, a project which identified itself as a confirmation of Meynert’s own theorizing about the nature of brain function. This was Wundt’s opportunity to stake a bold claim in field of physiological research that was seeing the beginning of a massive conflagration of development. Unfortunately for Wundt, *Mechanik der Nerven* was received with much of the silence that had accompanied his previous endeavours in physiology.

That very same year, Wundt became aware of a book which threatened much of what he had attempted to achieve in his challenging career. The book in question was written by Julius Bernstein, Wundt's replacement in Helmholtz's lab. Titled *Untersuchungen über den Erregungsvorgang im Nerven- und Muskelsysteme*, Bernstein's book was also published in 1871 and, much like Wundt, it discussed the physiology of spatial perception, amongst other things. The primary thesis of Bernstein's *Untersuchungen* advanced his notion of cortical irradiation to provide a physiological hypothesis for Weber's two-point touch threshold. (Bernstein, 1871, 167-169; Diamond, 2001, 54). This expanded upon Bernstein's 1868 "Zur Theorie des Fechner'schen Gesetzes der Empfindung," a boldly named text which established that cortical irradiation operated according to Fechner's law, assuming relatively consistent localized inhibition and that the intensity of the irradiation of stimulus was proportional to the intensity of the stimulus (Bernstein, 1868). Bernstein had not only proven to be a far more adept physiologist than Wundt, but his hypotheses had encroached on the psychological thesis of the *Beiträge* and subordinated the psycho-physical law to physiological reductionism.

As a physiologist, Bernstein was everything that Wundt was not: recognized for his pioneering work, a personal favourite of du Bois-Reymond and Helmholtz—in every sense the new face of neo-mechanism (Araujo, 2014, 53; Seyfarth, 2006, 2). This is not to say that Wundt was by any stretch an incapable or incompetent physiologist. Rather, Bernstein was far more literate in the styles of reasoning favoured by in neo-mechanistic physiology, particularly mathematics. The radically different impulses behind the two figures is expressed through their different conclusions on how Fechner's law should be interpreted. Wundt had emphasized the psychical dimension of Fechner's law and sought to extend it into higher order mental processes. Bernstein saw Fechner's conclusions as empirically merited, but sought to understand them physiologically (even du Bois-Reymond recognized Fechner's knack for measurement) (du Bois-Reymond, 1847/1927). By the time of the events of 1870-1871, Wundt may have begun to realize that his aspirations would not be realized as a professional physiologist. All of this is important for understanding the context out of which Wundt's famous *Grundzüge der physiologischen Psychologie* emerged in 1873 and Wundt came to be in Leipzig by 1875. Though Wundt had been hesitant to break off from the only stable field in which he was established, circumstances leading up to the year 1872 had proven that Wundt's future as a physiologist was as at least as uncertain as any other path. It is no surprise, then, that by 1873 Wundt struck out against the neo-mechanistic physiologists in order to expand upon the foundations set out in the *Beiträge* and develop it into a full-fledged research program.

The Grundzüge

In 1874, Wundt would be called to Zürich to assume a professorship in inductive philosophy, only to be called to Leipzig in 1875. The contents of both inaugural addresses reflect the transformation that Wundt had undergone in the course of writing and publishing the *Grundzüge.* Their language carried the sentiment that the materialists of the natural sciences had forgotten their origins—that the foundations of their exclusive reductionism was derived from the realm of philosophy, rather than nature itself (Wundt, 1876). The man who gave these inaugural addresses was no longer the Wundt who pandered for the recognition of his colleagues in physiology. This was a Wundt who had broken off, who was trying to forge his own path, who had picked a side. This was the ultimate function of Wundt's *Grundzüge*: it was the formation of a decisive stance, born through the culmination of over a decade of labour in psychology, psychophysics, and physiology. Most importantly for this discussion, it is a window through which to view everything that was to come.

At times deemed the most important publication in the history of psychology, the objective behind Wundt's 1874 *Grundzüge der physiologischen Psychologie* was "ein neues Gebiet der Wissenschaft abzugrenzen" (Boring, 1950, 322; Wundt, 1874a, III).[81] The title in itself was more than sufficiently described the books contents, as Wundt himself remarked in the introduction (Wundt, 1874a, 1). True to its title, the *Grundzüge* was intended to be the foundational text for a new science that reached beyond the purview of physiology or psychophysics. The reversal of "*psycho-physik"* into "*physiologischen Psychologie"* is telling in this regard. Just as the ordering of the words *psycho-physical* reflected Fechner's hope that physical measurement could uncover the relationship to the psychical, Wundt's *physical-psychology* was concerned with the direct, experimental study of the conscious process, with the support of physiological science (Wundt, 1874a, 2). Thus, physiological psychology meant using the methods of physiology to study the psychical, as opposed to studying physiological phenomena in order to study the connection to the psychical.

Central to Wundt's argument for a science of psychology were the failures of physiology and existing psychology's speculative character. Psychology, Wundt argued, had always had a role in physiology (Wundt, 1874a, 2). Particularly where the function of nervous tissue was concerned, the physiologist's observations relied upon the recognition of psychological symptoms (Wundt, 1874a, 2). Yet, the dominant trend in neurophysiology had

[81] […] "to delineate a new field of science."

already begun to favour the reduction of even higher order mental functions to neural states, in a sense subordinating the study of psychological phenomena to neurophysiology. Conversely, many theories which laid claim to the title of psychology lacked any interest in the physical, bordering on the speculative (Wundt, 1874a, 2). In this sense, the primary subject of Wundt's criticism were psychologies grounded in metaphysical presuppositions or simple observation (Wundt, 1874a, 2). Physiological psychology was to be an experimental science, in every sense as empirically valid as physiology, the subject of which was the conscious process (Wundt, 1874a, 2-3).

The question was how is the psychical to be demarcated from the physical in such a way that an independent science is not only necessary but possible? For Wundt, the answer was a secularization of Fechnerian parallelism, combined with a Herbartian critique of faculty psychology. Fechner's concept of the soul was inseparable from his understanding of the psychical. The physical and mental ran in parallel, the soul being eternal where a specific body is not. Wundt had readily taken up Fechner's stance concerning psychological parallelism; however, Wundt stopped at supporting Fechner's stance on spiritism (Meischner-Metge, 2010, 420-421). This is preserved in a letter from Fechner to his younger colleague dated June 25, 1879, with Fechner conceding that, though the pair had long debated the question of spiritism, he was finally willing to concede that Wundt's mind could not be changed (Fechner, 1879; Meischner-Metge, 2010, 420-421). An intermediary influence here was Friedrich Lange who had advocated for a psychology without a soul, and held the chair Wundt assumed in Zürich prior to his departure for Leipzig (Engstrom, 2015, 153). Wundt upheld the parallelistic principle while reducing the soul (*Seele*) to a term which denoted the logical subject of inner experience, rather than a distinct metaphysical substance (Wundt, 1874a, 9). Instead, Wundt defined terms like *Seele, Geist* (spirit/mind), *Leib,* and *Körper* in phenomenological terms. *Seele* was to spirit/mind (*Geist*) what *Leib* was to *Körper* (Wundt, 1874a, 9-10). *Geist* and *Körper* were conceptual representations of their subjects as objects, the mind and body respectively. *Leib,* meanwhile, stood in for the lived experience of having a body, and *Seele* for the experience of having an inner world. Thus, Wundt conceived of *Seele* and *Geist* as terms for the same thing at different levels of self-reflection.

This phenomenological shift which emphasized the actuality of the conscious process over speculation on the structure of mind reflected the influence of Herbartian anti-faculty psychology. Wundt enthusiastically embraced Herbart's critique of the faculty model found in Kant and Wolff, arguing that the notion of faculties remained mere possibilities that contradicted experience (Wundt, 1874a, 18). As Wundt read Herbart, conscious perceptions were received as total unities—there are no synthesizing faculties, only direct ideas, feelings, perceptions (Wundt, 1874a, 18). Wundt ultimately saw Herbart as errant in his approach, but he drew inspiration from Herbart in emphasizing the actuality of the conscious process as the object of psychological inquiry (Wundt, 1874a, vi, 18-19). The structure of the conscious process would then be investigated through rigorously controlled experimentation.

The physiological referent for rudimentary psychical events such as sensation was evidently something that Wundt considered knowable in the *Grundzüge.* The entire first volume of the *Grundzüge* was dedicated to outlining the physiology of nervous tissues and their function. He further discusses the structure of various organs involved in sensation (Wundt, 1874a). In these cases, physiological and psychical events had a clear relationship. This ceases to be the case for higher order psychical events, perhaps even for all psychical processes beyond the realm of sensation. It was toward this Byzantine realm of psychical processes, only remotely relatable to physiological events, that Wundt's psychology would guide its inquiry. What Wundt had proposed was not spiritual parallelism; it was a science of *embodiment,* the dynamic lifeworld of the subject, within the framework of partial parallelism.

The Lab and the Question Concerning Reaction Time

However grand, Wundt's self-conscious efforts to found a new branch of scientific inquiry would remain unrealized as long as it failed to generate a concrete research program. Wundt got an opportunity to realize his project once he arrived in Leipzig. As for the subject of his foundational psychological study, Wundt chose reaction time—a decision that was not without controversy.

The origins of reaction time as an object of experimental inquiry are essentially twofold. There was, of course, Helmholtz's work on the propagation speed of an action potential across a nerve fibre, which had demonstrated that the speed of an action potential was much slower than previously assumed. Of at least comparable significance was Friedrich Bessel's impactful realization that independent observers of the same astronomical events consistently had minute but mathematically significant variations in their recorded values, which Bessel termed the "persönliche Gleichung" (personal equation) (Hoffmann, 2006, 172; Exner, 1873, 606). Bessel had begun to recognize this phenomena as early as 1818, but only fully recognized and sought to investigate the universality of the effect in the early 1820s (Hoffmann, 2006, 147, 166, 172-179). In 1861-1863, Swiss-born astronomer Adolph Hirsch first used Matthäus Hipp's chronoscope device to measure the delay in what would come to be known as simple reaction time (Robinson, 2001, 164).

Bessel's personal equation had demonstrated the scope of the phenomena, although it was Helmholtz's work on propagation speed that validated further investigation into personal equation as an object of physiological inquiry. Though the emergence of the modern concept of reaction time is rightly located in the 1870s, Franz Donders' work in the 1860s was arguably the first physiological study of the matter. In 1868, Donders demonstrated that simple response times were shorter than response times that involved more complex mental tasks, in this case recognition of a vowel sound (Donders, 1868, 423). In order to come to these results, Donders pioneered the subtraction method as a means of determining what he called "physiological time" (Donders, 1868, 417, 428; Robinson, 2001, 164). This was a basic but effective methodological approach which entailed the experimenters initially taking a simple response time, followed by a second reading involving a more difficult mental process. The first time was then subtracted from the second, allowing the experimenter to determine separate values (Robinson, 2001, 164). Rather than use Hipp's chronoscope as Hirsch had, Donders used a different chronograph. Importantly, Donders understood the phenomena in physiological terms, and perceived his experiment as a continuation of the work pioneered by Müller, du Bois-Reymond, and Helmholtz (Donders, 1868, 415).

The next major development in the physiological conception of response time was Sigmund Exner's 1873 "Experimentelle Untersuchung der einfachsten psychischen Processe, Erster Abhandlung." Exner had studied with Helmholtz in Heidelberg from 1867-1868, and Exner's research on the neurology of perception was essentially a continuation of Helmholtz's work on the physiology of the nerve impulse. Exner's research would become a series on the neurophysiology of simple psychological phenomena, namely reaction time (Exner, 1875, 404). Exner's project was inspired firstly by Helmholtz and Baxt's 1870 discovery that variations in temperature affect the propagation speed of an action potential across a motor nerve fibre (Exner, 1873, 601, 606). The second component was Exner's awareness of the limited nature of the work done on the physiological nature of Bessel's personal equation (Exner, 1873, 606). Understanding this phenomenon would theoretically have helped determine "wie wird der persönliche Fehler ein Minimum" (Exner, 1873, 608).[82] As Exner understood it, the physiological question posed by the problem of the personal equation could be reduced to the minimum time elapsed in generating a bodily reaction to a direct stimulus (Exner, 1873, 609). Thus, Exner dubbed this metric "Reactionzeit" (reaction time), thereby coining the term (Exner, 1873, 609).

Exner's 1873 publications bears all the characteristics of an expeditionary investigation. It arguably casts both too wide a net and is too limited in the nature of its investigation. To the latter point, Exner focused solely on simple reaction time, despite being aware of and even citing Donders' research (Exner, 1873, 622). Instead, Exner compared simple reaction times measured at different points of the body and in response to different stimuli. He even made some observations about the effects of various intoxicants on reaction time, which is discussed in a later chapter. The primary stimulus was an electric shock at a given location, with reaction being recorded at a number of different points on the body, though an electric spark was also used as a visual stimulus in some trials (Exner, 1873, 621). Exner was then able to compare simple reaction time from right to left hand, left to right hand, left foot to right hand, from visual stimulus to hand, forehead to hand, and so on (Exner, 1873, 644-659).

Given the dominance of the physiological conception of reaction time, how was it that "the reaction-time studies conducted during the first few years of Wundt's laboratory" would

[82] […] "how to reduce personal error to a minimum."

come to “constitute the first historical example of a coherent research program, explicitly directed toward psychological issues and involving a number of interlocking studies" (Danziger, 1980, 106)? The answer is that Wundt took up physiological reaction time as an object of experimental inquiry and reconceived of reaction time as a psychological event. As early as the *Grundzüge,* Wundt had begun to consider reaction time to largely be a psychological, rather than a physiological phenomenon. Wundt was well aware of the findings of Donders, Exner, Baxt and Helmholtz, all of whom are addressed in the first edition of the *Grundzüge* (Wundt, 1874a, 498, 728, 740-741, 752). It came down to how reaction time was broken down into separate sub-processes, and where those sub-processes figured within Wundtian parallelism. The initial sense perception and the muscular movements necessary to signal a reaction were naturally physiological events. However, between these physiological events there were at least three psychological events: perception, the focusing of apperception, and the act of willing (Wundt, 1874a, 728, 734-736). This made reaction time, as something that could easily be measured and placed in a table alongside other measurements, an ideal candidate for experimental psychology’s founding research program.

The critical innovations that made Wundt’s argument possible were his definitions of differentiation, choice, and will. As Robinson points out, Wundt was critical of the distinctions Donders made between choice and differentiation in his experimental design (Robinson, 2001, 166). Donders had proposed models for simple, differentiation, and choice reactions. The choice reaction entailed hearing a vowel sound and responding with the same sound, while the discrimination reaction required that the subject respond when they heard a pre-determined syllable (Robinson, 2001, 165-166; Wundt, 1874a, 744-745). Wundt’s critique of Donders experimental design was that Donders’ discrimination reaction still entailed motor selection, and therefore choice (Robinson, 2001, 165-166; Wundt, 1874a, 744-745). This would set Wundt on a course to develop an experimental design for a pure discrimination reaction, which would debut in his Leipzig laboratory and be included in subsequent editions of the *Grundzüge.* Further still, Wundt took up Donders' subtraction method, though Wundt followed through with his preference for Hipp's chronoscope which Wundt had expressed in the initial *Grundzüge* (Wundt, 1874a, 732-733). Donders' research thus provided a backdrop against which Wundt developed his own psychological conception of reaction time.

As discussed, the emergence of reaction time as the founding research program for experimental psychology, and therefore the initial justification for the new experimental science, had its roots in the early 1870s. By the time Wundt's psychological laboratory was founded in Leipzig in 1879, psychological reaction time had crystallized into a robust and experimentally dynamic concept. Almost immediately after the formation of Wundt's psychology laboratory, a young doctoral student of Wundt's named Max Friedrich began conducting research on reaction time in response to visual stimuli in the lab (Domanski, 2004, 311). Friedrich wrote the first dissertation in the field of experimental psychology; titled "Über die Apperzeptionsdauer bei einfachen und zusammengesetzten Vorstellungen", it published in 1881 and officially awarded in 1880 (Behrens, 1980, 19; Domanski, 2004, 311-312; Friedrich, 1883). Friedrich was the first, but he was but one of Wundt's many graduate students who would work on reaction time before further psychological research. More than Wundt himself, it would be his students who would ultimately go about institutionalizing the science of experimental psychology and disseminating Wundtian ideas across the world.

Wundt had faced down an uncertain future in academic physiology and come out of the founder of a new branch of the sciences. This was in large part owed to the influence of Weber and Fechner, whose parallelistic doctrine provided an avenue of critique through which Wundt could approach what he had seen over the course of his education in physiology. While not a critique of the science of physiology, the emergence and eventual success of Wundt's new science of experimental psychology was in contrast to the totalizing equivalence between mental and neural states championed in neurophysiology. In this sense, it was implicitly critical of neo-mechanism, while still upholding the value of physiology. This was made possible by Wundt's shift from a position characterized by an *a priori* recognition of the concealed world of the body as the locus of the real to the study of the ready-to-hand, perceptional life world, to the experience of embodiment. Given the enduring attention that Wundt allotted to neuroanatomy and the experimental focus he took from physiology, this new science was not predicated on a rupture in criteria of validity. Rather, an adequate description of higher order mental processes had demonstrably eluded neurophysiological reductionism. This ambiguity concerning the physiological definition of mental processes left sufficient space for Fechnerian parallelism to have a conceptual influence.

Though Wundt would eventually attain world renown, in the early years of the laboratory experimental psychology was hardly an established science, and was not without detractors. It fell on Wundt's students to carry the new science forward, and there was one student in particular whose impact on not only psychology, but psychiatry and biology more broadly has only begun to be accounted for. That student was Emil Kraepelin, whose place in this story is about to be told. But before diving in to the Kraepelinian legacy, it is important to contextualize his work with an accounting of their age. After all, their age, the epoch of Wundt, Meynert, Kraepelin, and the like, was a golden age, not only of new ways forward in the life science but of intoxication. Perhaps it was this that made all the difference.

Chapter 8—A Post-Alkaloidal 'Golden Age' of Intoxication

Novel Intoxicants

The years running into the 1870s and 1880s introduced us to a novel conceptions of a mechanical brain and a fresh way forward in psychology, a new science of embodiment. Yet, much of this sound and fury for the most part ignored an ever growing substrate of 19th century medical, bio-social, and material existence: the rapid explosion in the range of publicly available intoxicants. The alkaloidal revolution of the early 19th century, sparked by Sertürner's identification of morphine in 1817, had been the dawn of a new age, but it was in the latter half of the German 19th century that the full implications of this event stepped into the light of day.

The number of intoxicants available to physicians and the public alike in Europe at the beginning of the 19th century had been a short list consisting of opium, cannabis, ether, and alcohol—on top of lighter stimulants like tobacco, coffee, and tea. By the 1880s, these proven fixtures were supplemented by a growing array of alkaloidal isolates available in thousands of different formulations. There had never been so many different kinds of intoxicating substances or ways to consume them, most of which were pushed into the hands of customers by way of misleading advertisements. This was a 'golden age' of intoxication. It might be called a 'golden age' because of the unprecedented access to different powerful intoxicants, because of the range of novel experiences, because of the rise of industrial pharmacy, or because of the sheer scale of drug use in the era. But, most of all, this might be called a 'golden age' because it was the twisted and distorted realization of a dream of a brighter future, one shared by organic and alkaloidal chemists of the mid-nineteenth century. To make sense of this, it is worth taking a look at the early career of Justus Liebig and the first synthesis of chloral hydrate, before looking at everything that followed.

New Synthetic Substances

The emergence of the alkaloid as a new kind of thing to be in the world had already forever changed pharmacy and medicine. The discovery of morphine had almost immediately altered how therapeutics were deployed in a clinical context by all but confirming the active principle theory of physiological action. Morphine of course remained an organic plant principle, something which was already present in the opium latex produced by the poppy. Opium was one of nature's curious manufactures, like honeycomb, shellac, or even alcohol. The discovery of morphine itself had no direct bearing on the possibility of synthesizing a new drug. However, the very idea of using the methodologies of the chemical sciences to isolate an active plant principle, thereby making it subject to a battery of chemical analyses, very well might have.

The first synthetically derived intoxicant, or drug of any kind, was chloral—the same chloral which drove asylum life in the latter-19th century. It was created by Justus von Liebig in 1832, who had fled from the beginnings of a career in pharmacy to pursue chemistry (Hofman, 1876, 102; Schmitz, 1985, 63). Though, the circumstances underlying such a development require some context (Hofman, 1876, 102). Liebig's legacy as a primary founder of organic chemistry was already secured in the 1830s when he established the "first large-scale teaching, research laboratory in any science" (Werner and Holmes, 2002, 422). In large part owing to the foundations set by Liebig and Wöhler in the 1830s, the sub-field of organic chemistry had begun to develop more quickly than inorganic chemistry by the 1840s (Werner and Holmes, 2002, 422). Though, much like his contemporaries Fechner and Müller, many of his theoretical positions seem at odds with the scientific heritage fostered by his work. For one, Liebig was, and remained, a vitalist. His 1843 *Die Thier-Chemie oder die organische Chemie in ihrer Anwendung auf Physiologie und Pathologie* boldly asserted, in just one of many similar statements, that “alle Vorgänge im Organismus under dem Einfluß einer immateriellen Thätigkeit [(Lebenskraft)] stehen, über welche der Chemiker nicht nach Willkür verfügen kann” (Liebig, 1843, 148).[83] Proclamations such as these lead some historians to ponder the question “can Liebig, in view of his eminent success as a scientist, really be considered a vitalist” (Lipman, 1967, 167)? A better question is: in what way was Liebig a vitalist and how did his vitalism factor into the research program of organic chemistry?[84]

Liebig is noteworthy, among other things, for being directly involved with Friedrich Wöhler’s famous synthesis of urea, and yet remain a vitalist.[85] Liebig’s vitalism was traditional insofar as it upheld the notion that a vital *Lebenskraft* was the determinative of the physiological character of organisms, and the primary quality distinguishing organic and inorganic matter (Liebig, 1843, 9, 11, 86, 143, 176). In this sense, Liebig’s vitalism was more reminiscent of 18th century vitalists like Riel and fundamentally at odds with the vital monism of the *Naturphilosophen*, whose theories Liebig vehemently opposed. But on the chemical level, Liebig famously argued that there was no essential chemical difference between organic and inorganic material, as stated in an 1838 paper with Wöhler:

> Die Philosophie der Chemie wird aus dieser Arbeit den Schluß ziehen, daß die Erzeugung aller organischen Materien, in so weit sie nicht mehr dem Organisinus angehören, in

[83] “all processes in the organism are under the influence of an immaterial activity [(life force)], over which the chemist cannot dispose arbitrarily.”

[84] (Everett Mendelsohn, 1965, 203) argues that questions of vitalism are secondary to the influence these ideas had in their scientific theories/methodologies

[85] Wöhler synthesis has long been idealized in some histories as the crucial experiment in the great debates concerning the vital force (Rocke, 1993, 239). Of course, the synthesis was not regarded as such at the time.

> unsern Laboratorien nicht allein wahrscheinlich, sondern als gewiß betrachtet werden muß (Liebig and Wöhler, 1838, 242).[86]

At first glance, this idea appears to reflect the one-in-many principle of *Naturphilosophie*: a vital monism that erodes the distinctions between organic and inorganic, by realizing all that is as a single organic whole.

There were many ways in which Liebig's ideas superficially resembled those of *Naturphilosophen.* Like the *Naturphilosophen,* Liebig was a teleological thinker (Brock, 2002, 311). Like the *Naturphilosophen,* Liebig rejected material reductionism. Liebig himself had even been a student of Schelling's for two years in Erlangen (Snelders, 1970, 193). However, Liebig himself had been one of *Naturphilosophie's* strongest opponents, looking back on his years with Schelling as wasted time (Snelder, 1970, 193). Liebig later summarised *Naturphilosophie* as being "an Worten und Ideen so reiche, an wahren Wissen und gediegenen Studien so arme" (Liebig, 1874, 34).[87] These were kinder words than Liebig had used in 1840 when he described *Naturphilosophie* as the "Black Death" (Brock, 2002, 67). Liebig allegedly never found Schelling's *Naturphilosophie* compelling, specifically because Liebig was keenly aware that Schelling himself had a limited understanding of the natural sciences (Brock, 2002, 27). This does not necessarily mean that Liebig was always hostile towards *Naturphilosophen.* Liebig's mentor in physics and chemistry, Karl Kastner (1783-1857), was also a *Naturphilosoph*, and yet Liebig followed Kastner from Bonn to Erlangen and stayed with him all the way through his doctorate, up until Kastner secured a grant for Liebig to study in Paris with Guy-Lussac.

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[86] The philosophy of chemistry will draw from this work the conclusion that the production of all organic matter, in so far as it no longer belongs to the organism, must not only be probable in our laboratories, but must be regarded as certain.

[87] "so rich in words and ideas, yet so poor in truthful knowledge and genuine research."

However, the Liebig of the 1820s was decidedly at odds with the naturphilosophic tradition. In 1824, Liebig wrote to Platen about his commitment to materialism (Brock, 2002, 309). Though compelling, it is not entirely clear what Liebig meant by this. Was he identifying with materialism in contrast with *Naturphilosophie?* What does this say about Liebig's vitalism in his early years? Unfortunately, Liebig's passing reference to materialism does little to suggest he was not a vitalist in his student days and early career. Vitalism can be a central part of a materialist doctrine. Though Brock and Hall speculate that Liebig's belief in the vitalism of organized bodies was informed by Johannes Müller's 1835 to 1837 publication of the *Handbuch,* there is no direct evidence to suggest this connection (Brock, 2002, 310). More importantly, Liebig's vitalism sharply contrasts with that of Müller's, a post-naturphilosophic vitalism grounded in his notion of the organizing principle. Liebig's *Thierische-Chemie* instead implicitly argued that a vital force was needed to account for the capacities for growth, tissue repair, and complexity characteristic of all living beings (Lipman, 1967, 176). Regarding the chemical composition of organic and inorganic bodies, it is true that Liebig saw no difference, except the chemical products of living beings are of sufficient complexity that they could only naturally originate from a vital entity. What likely started as a matter-of-fact vitalism became, by the 1850s, a principled effort to carve a middle way between what Liebig saw as the extreme reductionism of the neo-mechanists and the wanton speculation of *Naturphilosophie* (Brock, 2002, 311).

This was the perspective that Liebig carried into his professorship at the University of Giessen, where he would rise to fame. Alexander von Humboldt recommended Liebig for a professorship at the University of Giessen in 1824 (Brock, 2002, 35). Initially restricted from accessing laboratory time, Liebig proposed the formation of a pharmaceutical institute in 1825, but when his proposal was rejected by the university he was forced to organize it as a private venture until the university agreed to include Liebig's lab in 1833 (Brock, 2002, 43, 47). Given that Liebig's introduction to chemistry was a pharmaceutical apprenticeship in Heppenheim, it was only fitting that Liebig had once again turned to pharmacy to begin his career as an academic chemist (Schmitz, 1985, 63).

It was at this point, in the early 1830s, that Liebig began to research the structure of various "Strychnins, Morphins, Narcotins, Atropins und fast aller damals bekannten Alkaloïde," a project that would see later see Liebig declare that everything from sugar to morphine would soon be manufacturable not only in nature but in the lab (Volhard, 1898, 39; Liebig & Wöhler, 1838, 242).[88] The first step was to establish a methodology for determining the elemental constitution of organic compounds. Sertürner had been the first to fully demonstrate that a pharmaceutically active organic substance could be extracted and crystallized, now Liebig sought to create the same substances from their basic chemical components. To this purpose, Liebig developed a combustion method for determining the carbon and hydrogen content of organic substances (Liebig, 1831, 1-3). Morphine was among the first substances put through Liebig's combustion analysis, followed shortly after by strychnine and atropine (Liebig, 1831, 9). Thus, the inaugural focus of the world's first large-scale research laboratory had been the chemical analysis of morphine and other intoxicants, just as alkaloidal chemistry had begun with morphine decades earlier.

How fitting then that the following year Justus Liebig synthesized the first drug when he created chloral hydrate alongside chloroform, though Liebig was unaware of chloral's effects at the time. His process "leitet durch den Alkohol Chlorgas," which had previously been dried with

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[88] […] "strychnines, morphines, narcotines, atropines and almost all alkaloids known at that time."

calcium chloride (Liebig, 1832, 252).[89] This process was continued until hydrochloric acid ceased to precipitate at the top of the apparatus (Liebig, 1832, 253). The colourless liquid remaining after formed an oil-like droplet when put in water, dissipating in warm water (Liebig, 1832, 256). The oily substance behaved very differently only a small amount of water was added to it. Here, the substance "verbindet sich bei dem Schütteln sogleich damit, indem es sich stark erhitzt; einige Augenblicke darauf erstarrt die Mischung zu einer durchsichtigen weißen Krystalmasse."[90] Liebig named the oily liquid "Chloral" in reference to the word "Aethal," recognizing "daß bei einer vollkommenen Zersetzung des Alkohols das Chlor den Wasserstoff desselben abscheidet und an die Stelle desselben tritt" (Liebig, 1832, 252).[91] Liebig further noted how an alkali, soda, decomposed chloral to produce Chlorkohlenstoff (chloroform) as well as formic acid (Liebig, 1832, 256). However remarkable a development in the history of pharmacy and intoxication, Liebig was unaware of chloral's hypnotic properties, which would only first be recognized in 1861 by Rudolf Buchheim and written on in 1869 by Oskar Liebreich (Liebreich, 1869, 15).

The *Annalen der Pharmacie* was also formed in 1832 by Justus Liebig, Rudolph Brandes, and the alkaloidal chemist Philip Lorenz Geiger, around when Friedrich Wöhler began to work

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[89] […] "ledchlorine gas through alcohol."

[90] […] "immediately combines with it when shaken, heating up strongly; a few moments later the mixture solidifies into a transparent white crystal mass."

[91] "[…] that in a complete decomposition of the alcohol, the chlorine separates out [the alcohol's] hydrogen and takes [the hydrogen's] place"

in Liebig's lab, for lack of resources in Kassel (Liebig, 1832; Hoppe, 2007, 195).[92] Liebig and Wöhler quickly formed a highly productive professional friendship, supported by a new journal in which to share their findings. They jointly published a vast number of papers on foundational topics in organic chemistry, pioneering the "radical theory" in response to their shared work on benzoyl (Rocke, 1993, 51-52). But both figures had a role in the discovery of another intoxicant: cocaine.

Wöhler's Stimulating Discovery

The effect of coca leaves had been known to Europeans for quite some time, but the remarkable difficulty of transporting a large amount of coca leaves from the South American colonies to Europe had significantly hampered the efforts of interested researchers. The earliest attempt at an extraction from coca leaves was carried out by Heinrich Wackenroder in 1853, using an isinglass solution to yield a precipitate from ethanol and coca leaves (Wackenroder, 1853, 24). Further investigations were stalled, first by a shortage of coca leaves and then by Wackenroder's death the following year. In 1855, Friedrich Gaedcke published an article titled "Ueber das Erythroxylin, dargestellt aus den Blättern des in Südamerika cultivirten Strauches Erythroxylon Coca Lam." in *Archiv der Pharmazie* (Gaedcke, 1855, 141). Gaedcke detailed the process by which he extracted the active alkaloid found in the South American coca leaf, naming the new substance "Erythroxylin" after the coca leaf's genus, erythroxylon (Gaedcke, 1855, 141). The brief paper ran fewer than 10 pages and much of its content was committed to the discussion of the social and cultural significance of coca leaf in its South American homeland

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[92] Wöhler and Liebig had previously corresponded in the resolution of a disagreement over the characteristics of a silver compound with fulminic acid and silver cyanate.

(Gaedcke, 1855, 141-144). By Gaedcke's own report, "[d]ieser Versuch reicht nicht hin, die Identität dieses von mir Erythroxylin benannten Stoffes […] zu beweisen," citing an insufficient supply of coca leaves (Gaedcke, 1855, 150).[93] The supplies available were insufficient for Gaedcke to even say for certain whether the extract contained caffeine or was a different, related, substance (Gaedcke, 1855, 148, 150). An additional complication, one which likely precluded Gaedcke from reaching any decision about his precipitate, was the quality of Gaedcke's isolate. Gaedcke was able to produce a bitter crystalline substance, though it had emerged alongside a slick, oil-like substance (Gaedcke, 1855, 147-148). Others in Italy and Great Britain had also tried, to even worse luck (Niemann, 1860, 149-150).

Shortly after Gaedcke's publication in *Archiv der Pharmazie,* Friedrich Wöhler took it upon himself to acquire a stock of coca leaves in order to study their composition. This was eventually accomplished by enlisting the assistance of Austrian explorer Karl von Scherzer, who was able to acquire 30 pounds of coca leaves in Lima while circumnavigating the world with the *Novara* expedition (1857-1859) (Niemann, 1860, 133). Upon receiving the parcel in 1859, Wöhler gave the project to one of his graduate students, Albert Niemann, who took it on as the topic of his dissertation (Niemann, 1860, 132-133). The resulting paper was published in 1860 with the title "Ueber eine neue organische Base in den Cocablättern." Niemann would dub his alkaloidal extract "Cocaïn," the name it is known by to this day.

As Niemann himself admits, the presence of an alkaloid in coca leaves was already expected insofar as pharmaceutical chemists had come to suspect that yet undiscovered alkaloids were behind the physiological effects of all the remedies of the *materia medica* (Niemann, 1860, 130-131). This blanket assumption, Niemann suggested, had already been proven false, while

[93] "[t]his attempt is not sufficient to prove the identity of this substance I have named erythroxylin [...]."

many well-supported inquiries into presumed alkaloids were wholly insufficient to make any definitive claims (Niemann, 1860,130-131). In these opening pages, Niemann drew a clear demarcation between what he understood to be the overeager findings of ambitious alkaloidal chemists and those claims whose alkaloidal objects merited recognition as distinct substances, like Sertürner's 1816-1817 publication. Sertürner's name even appears in the first line of Niemann's text, in a clear stint to position his own alkaloidal discovery within the authority of a Sertürnerian tradition.

Niemann's method involved leaving 200 grams of chopped coca leaves in a bath of ~85% alcohol, previously mixed with 1/50 diluted sulfuric acid, at a temperature of ~40 degrees for four days (Niemann, 1860, 151). After straining out the diced coca leaves, this left Niemann with a green-light brown liquid, which he noted was still somewhat acidic (Niemann, 1860, 151). After drying the leaves and finding them so void of any bitterness that a second extraction would be unnecessary (though he nevertheless did), Niemann mixed the fluid extract with a watery paste consisting of hydrated lime and allowed this blend to sit at room temperature, with regular shaking (Niemann, 1860, 151). The precipitate was filtered out and set aside, while the tincture was distilled in a warm water bath until the remaining alcohol had dissipated, at which point distilled water was added to the syrupy residue (Niemann, 1860, 151-152). The syrupy residue had become an undissolved, resin-like mass, which Niemann filtered out and washed several more times with distilled water (Niemann, 1860, 152). Rather than pour out the last body of distilled water, Niemann added carbonate of soda, which turned the liquid a deep, dark red (Niemann, 1860, 153). The crucial final step was the introduction of a generous amount of ether, after which the container was sealed and shaken, with the ether left to evaporate on a shallow tray (Niemann, 1860, 153). What remained after 24-hours was a strongly alkaline crystallized mass, flecked with yellow-brown particulates. He persisted in purifying his isolate further and was eventually successful at rendering a purely white final product (Niemann, 1860, 154-155). Niemann's cocaine isolate was still chemically impure, but he had nonetheless succeeded at making-real a novel alkaloidal entity.

The broader consequences of organic chemistry in general, and alkaloidal chemistry in particular, did not go unconsidered by Albert Niemann. In the decades between discovering chloral and Niemann's extraction, Justus Liebig had attempted to turn organic chemistry to ever broader horizons. Liebig saw his work as having far-reaching social consequences and began to write on the chemistry of agriculture, living beings, and food, publishing *Die organische Chemie in ihrer Anwendung auf Agricultur und Physiologie* in 1840, *Die Thierchemie, oder die organische Chemie in ihrer Anwendung auf Physiologie und Pathologie* in 1843 and *Chemische Untersuchung über das Fleisch* in 1847. Though at times marred by Liebig's own lack of familiarity with the actual practice of agriculture, Liebig's agricultural chemistry was deeply impactful, with Liebig inventing chemical fertilizer and emerging as the pope of agricultural chemistry in his time (Brock, 2002, 128, 149, 176). Organic chemistry, Liebig imagined, was the key to uncovering the secrets of agriculture, medicine, and nutrition. Liebig's vision was of nothing less than a brave new world cooked up in the lab. It was precisely this dream that Niemann saw refracted in his shimmering crystalline precipitate.

Like Gaedcke's paper, Niemann's analysis was preceded by a meandering discussion of the South American practices and cultural understanding surrounding coca leaves.[94] It is difficult

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[94] There is an interesting discussion to be had concerning the rhetorical significance of the decision to include these colonial histories of coca, for a paper concerned with the identification of a chemical isolate

not to understand these remarks through the lens that Niemann himself prepares for his readers in the opening pages: one of the methodological triumph of chemical analysis over not only the vagaries of pre-alkaloidal medicine but the rushed conflations of his colleagues. Niemann thus associated the South American, and earlier European, consumption of whole plant remedies with imprecision, even backwardness, while connecting alkaloidal extracts with precision and futurality. Niemann's remarks on the significance of alkaloidal chemistry for the medical profession further illustrate this point. "Kein Arzt würde sich dazuverstehen" that plant remedies "eben noch andere wirksame Körper befinden, welche die Wirkung des reinen Alkaloides wesentlich modificieren" (Niemann, 1860, 131).[95] The physician determines which remedy to employ or how it should be delivered; however, it is the alkaloidal chemist, Niemann imagined, who must ultimately build the bridge between the murky uncertainties of medieval medicine and a future of medico-chemical precision. Niemann's work was but a pharmaceutical expression of a dream shared with Liebig, Wöhler, and other organic chemists of the mid-nineteenth century. The discovery of cocaine, like morphine, was a step into a new epoch and, with it, a new world order. Albert Niemann would die in the year 1861 at the age of 26 years old, just one year after publishing his research on cocaine. He never lived to see the radical future his work would help build.[96]

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[95] "No physician would understand" that plant remedies "contain other effective bodies which substantially modify the effect of the pure alkaloid."

[96] Niemann's cause of death is allegedly lung damage, possibly a result of his experimentation with mustard gas which had begun in 1860.

A Brave New World Cooked up in the Lab

Theoretical and experimental developments fuelled the diversification of intoxicants throughout the 19th century, though the magnitude of these events could hardly have been as noteworthy were it not for the simultaneous emergence of industrial pharmacy as well as the development of new technologies of intoxication. Just as mass consumption of opium entailed a sprawling field of poppies, public access to morphine of any significance required chemico-industrial infrastructure. Merck, Pfizer, Bayer AG—the titans of modern pharmacy trace their humble beginnings to the 19th century industrial production of alkaloids.

Merck was among the first, if not the first, modern industrial pharmaceutical drug company in history. The company traces its origins to Friedrich Jacob Merck's acquisition of Darmstadt's *Engel-Apotheke* in 1668, though it wasn't until Heinrich Emanuel Merck assumed control in 1816 that the Merck name would be associated with anything more than a local pharmacy business. Heinrich Merck had studied in Johann Trommsdorff's pharmaceutical institute from 1810-1812 (Friedrich, 1998, 508). After working in state and court pharmacies in Eisenach, Frankfurt, and Straßburg, Merck wrote the Prussian *Provisvorexamen* in Berlin in 1816 and assumed control of the family pharmacy in Darmstadt (Friedrich, 1998, 509). In light of his studies with Trommsdorff on pharmaceutical chemistry, Merck was very quick to develop an interest in alkaloidal research after Sertürner identified and named morphine in 1816-1817 (Friedrich, 1998, 509). Merck began to focus his energies on perfecting the extraction processes for the new alkaloids that were now regularly being discovered, to realize pure alkaloidal isolates (Friedrich, 1998, 509). By the year 1827, Merck's new alkaloid production company started selling morphine, wholesale.

The English morphine business followed a similar trajectory. Thomas Morson, after studying medicine in Paris, began producing morphine in the back of a retail business on Farringdon St., London and selling it between 1821-1822 (Berridge, 1999, 136). Around this time, the Society for the Encouragement of Arts, Manufactures, and Commerce also began offering premiums “for promoting and improving the cultivation of the papaver somniferum” (Jeston & Dick, 1823, 17). Morphine's "English variety sold at eighteen shilling per drachm, with the acetate and sulphate selling at the same rate" (Berridge, 1999, 136). Edinburgh’s Macfarlan and Company began morphine production in the early 1830s, buying opium wholesale from London and selling it back as muriate of morphine (Berridge, 1999, 136).

A major purchaser of morphine were patent medicine manufacturers. Patent medicines were remedies which were produced by entrepreneurs and sold directly to the consumer, generally without any obligation to divulge the content of the ‘medicine’ (Corley, 1987, 112; Woycke, 1992, 42). Though the patent medicine industry had existed since the 18th century, patent medicines only achieved significant commercial success in the mid-nineteenth century. By the 1870s, it is estimated that a quarter of all posted advertising was for patent medicines (Petty, 2019, 289). Contrasted with the 1821’s, when De Quincey famously reported “that the number of amateur opium-eaters (as I may term them) was at this time immense,” the source of intoxicants like opium and morphine used by the public was increasingly patent medicines (de Quincey, 1885, 4).[97] The annual sale of patent remedies in Britain increased from 500 000 pounds sterling

[97] De Quincey is the most famous literary promoter of narcotics, but he was far from the only one. Just as quickly as morphine became an iconic component of medical practice, morphine captured the imaginations of poets, artists, and broader society. Heinrich Heine would even name a poem after morphine before his death in 1856, and appears to have developed a serious reliance on morphine late

in the 1850s to 5 000 000 pounds by 1914 (Digby, 1999). The remedies themselves promised to not only put an end to pain and infirmity, but actually improve the customer's overall health and well-being. Ultimately, the medicines themselves consisted of little more than opium, morphine, and other alkaloids, which could quickly and cheaply allay the customer's physical or mental discomfort without any of the advertised benefits.

British economists of the 1880s described the patent medicine industry as filling the gaps in medical care for those who sought to treat their ailments without seeking the care of a doctor they could not afford (Corley, 1987, 112). In this respect, there were droves of poor and working class people in Industrial England, France, and America—the dispossessed, *les misérables*—who, despite working long hours in dangerous conditions, were largely overlooked by the medical system (Corley, 1987, 112). The receptiveness of the American market functioned on a similar principle: in light of the rural character of the American population, self-medication using commercial remedies was often the most familiar method of treating pain and disease, and in other cases perhaps the only way (Petty, 2019, 288). The patent medicine industry was there to take whatever surplus capital these people had, providing reprieve from often back-breaking labour at a fixed price (Corley, 1987, 112).

Following Niemann's publication on the isolation of cocaine in 1860, pharmaceutical manufacturers were swift to develop a means of manufacturing this alkaloidal extract too. Merck began selling cocaine as early as 1862, albeit in limited quantities (Courtwright, 2002, 47). When Freud would write *Über Coca* in 1885, he exclusively used Merck brand cocaine and credited Merck with ultimately justifying the belief that the entirety of coca's power stemmed from

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in life. Several authors have suggested this may have been his actual cause of death (Auf der Horst & Labisch, 1999).

alkaloidal cocaine (Freud, 1885a, 9, 11). From 1862 to 1879, there was considerable interest in the alkaloid among chemists from England to Russia, with some physiologists conducting variably successful experiments on the effects of cocaine on various animals (Freud, 1885a, 8).

Cocaine generally reached the general public through a different means: the cocaine tonic. In 1863, a Corsican chemist and businessman by the name of Angelo Mariani developed a tonic consisting of cocaine and Bordeaux wine for sale on the international market (Karch, 2006, 40).[98] Dubbed "Vin Mariani," Mariani's cocaine tonic was available everywhere in France by 1870, soon reaching London and New York (Karch, 2006, 40). Though competitors sprung up as the tonic's popularity grew, Vin Mariani went on to have tremendous brand success in America and across the European continent, the brand itself becoming synonymous with cocaine (Smith, 2008, 42; Karch, 2006, 41). This was largely made possible by Mariani's aggressive advertisement strategy, which would not have been out of place in the 21st century. Mariani sent free samples of his *Vin* to prominent physicians and celebrities, seeking endorsements to feature prominently in the pamphlets, books, and flyers to be found wherever Vin Mariani was sold (Karsch, 2006, 43-44). Pope Leo XIII, Pope Saint Pius X, Thomas Edison, and Ulysses S. Grant would all become disciples of Vin Mariani (Kennedy, 1985, 86; Karsch, 2006, 44). Pope Leo even offered up his photo as part of a celebrity endorsement of Vin Mariani, and Edison wrote a published endorsement as well (Karch, 2006, 43). What was remarkable about cocaine was how it ushered in a radically novel experience of being in the body. Stimulants of cocaine's sort were in short supply on the European continent. When coffee reached Europe, it fundamentally changed European society; coffeehouses became known as sites of free flowing ideas and

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[98] (Karch, 2006, 38) provides an alternative date of 1868.

excited chatter (Pendergrast, 2010, 9). Cocaine lifted the spirits, much like coffee, but it lifted those spirits to dizzying, euphoric, and heretofore unseen heights.

The German patent medicine and tonic boom did not take place until after 1869. Earlier in the nineteenth century, patent medicines had not been available in Germany as they had been in England and America. The majority of the medicines containing opium, morphine, etc. that had been purchased from German pharmacies had been by prescription, or at least with the customer's full knowledge of their content. Patent medications, contrastingly, were purchased by the consumer, without full knowledge of their contents (Woycke, 1992, 42). They were the early, practically unregulated, forbearers of modern "over-the-counter" medications. The beginning of the German restriction on patent medications ended in 1869 when the liberal Berlin Medical Society argued that the medical profession should be guided by a free market spirit—meaning that physicians would be free to choose who to treat and at what rates (Woycke, 1992, 42). One particularly catalytic facet of the Berlin Medical Society's recommendations allowed anyone to manufacture medications on grounds of operational freedom (Woycke, 1992, 50). By the end of 1871, the year that their recommendations were included in the commercial code by the Imperial government, over 1000 different patent remedies were available on the German market (Woycke, 1992, 43). One of the most ubiquitous "brands" in German patent medicine were those manufactured by F. A. Richter, who by the 1880s was making 5 million marks a year on his "Pain Expeller" and "Swiss Pills" alone (Woycke, 1992, 44). Like their French, English, and American counterparts, German patent medicines generally consisted of some blend of morphine, opium, hashish, and cocaine, depending on their advertised effects.

It was around this time that the first technological apparatus was developed for the explicit purpose of taking morphine into the body.[99] The idea of delivering a narcotic solution into the body by way of injection beneath the skin had been around since the 17th century. Christopher Wren and Robert Boyle had somewhat successfully injected a dog with a wine and opium solution in 1656, though the dog did die (Gibson, 1970, 334). In the German world, Hamburg's Daniel Major published *Chirurgia Infusoria* in 1664 and Berlin's Johann Sigismund Elsholtz wrote *Clysmatica Nova* in 1665 on intravenous injection (Reinbacher, 1998, 32; Gladstone, 1933, 190-191). Interest in injection waned until the 19th century, when Irish physician Francis Rynd published the results of what is believed to the first ever successful subcutaneous injection (Rynd, 1845, 167-168). Rynd produced a solution of "fifteen grains of acetate of morphia, dissolved in one drachm of creosote," which was "introduced to the supra-orbital nerve, and along the course of the temporal, malar, and buccal nerves, by four punctures of an instrument made for the purpose" (Rynd, 1845, 167-168).

In spite of Rynd's successful procedure, the invention of the modern syringe is generally accredited to the Scot Alexander Wood, who in 1853 developed a glass syringe attached to a hollow needle, though Charles Hunter suggested that Wood first employed the method in 1843 (Brunton, 2000, 349; Hunter, 1865, 10). The subcutaneous syringe was designed to treat pain at the nerve site, with Wood citing Johannes Müller's discussion of the effects of morphine on the excitability of exposed frog nerves (Brunton, 2000, 349).[100] By 1858, Wood declared that his

[99] Though a somewhat dishonest case could also be made that the opium pipe deserves this title. It is important to make an ontological distinction between morphine and opium.

[100] An interesting vignette on the broader significance of the vital substance concept for medical theory and practice.

method was being applied almost universally to treat pain in Edinburgh (Brunton, 2000, 350). The term “hypodermic” comes from Charles Hunter, an English surgeon, whose 1865 "On the speedy relief of pain and other nervous activities by means of the hypodermic method" made the case that hypodermic injection of an alkaloidal solution had generalized rather than merely local effects (Hunter, 1865, 11-12; Brunton, 2000, 350). Hunter’s methodology very quickly prevailed, though by the time of its publication the use of the syringe had already spread onto the European continent.

The history of the hypodermic method up until that point had been almost exclusively concerned with the delivery of narcotics. This was the initial purpose of the subcutaneous syringe. Thus, it is no wonder that use of the syringe almost immediately saw use outside of the clinical context, situationally transforming the syringe from a medical technology to a technology of intoxication. One factor which helped spread “recreational” use of the hypodermic needle was the proactive encouragement of the method for those who were “confirmed opium-eaters,” as the ‘medical’ substitute seemed farther removed from the evils of opium eating (Berridge, 1999, 141-142). This policy was particularly virulent in England. When physicians such as Clifford Allbutt began to raise the alarm about increased rates of reliance on morphine in 1870, Francis Anstie, who had been an outspoken advocate for the superiority of the hypodermic method through the 1860s, proposed a policy of tolerance. By Anstie’s estimation, the benefits of morphine outweighed any risks and that a controlled morphine-habit posed no serious risk if properly maintained (Berridge, 1999, 142). There was one subset of hypodermic morphine user who gave even Anstie pause, which was young mid- and upper-class women (Zieger, 2005, 63). Mirroring an earlier 19th century panic about upper class women and alcohol, the medical discourse surround women and morphine use had strong political undertones, drawing on themes such as a woman's lack of control, irresponsibility, and moral delicateness (Zieger, 2005, 64).

On the European continent, Paris hospitals in 1855 doled out a measly 272 grams of morphine from pharmacies to their patients, but by 1875 that number increased by 3576% to 10 000 grams (Courtwright, 2002, 37). In Germany too, hypodermic morphine use had grown to such a level that the Berliner Eduard Levinstein identified morphinism as a distinct physiological disease for the first time in his 1877 publication, *Morphiumsucht nach eigenen Beobachtungen* (Levinstein, 1877). Levinstein recognizes that Lähr's "Ueber Missbrauch mit Morphium-Injectionen" (1872) and Fiedler's "Ueber den Missbrauch der Morphium-Injectionen" (1876) were actually the first to identify hypodermic morphinism as a medical concern, though both attribute morphinism to "Psychose" rather than a distinct physiological phenomenon (Levinstein, 1877, 8). Levinstein attributed the spread of hypodermic morphinism to morphine's generous deployment in the 1866 Austro-Prussian war, much as it had been in the American Civil War (1861-1865) (Levinstein, 1877, 3; Courtwright, 1978, 101).[101] Levinstein further reported that, from then on, not only did physician increasingly begin to prescribe morphine injections for "jede anomale Empfindung" but the general public developed their own enthusiasm for the remedy and soon "[wurde] auch der psychische Schmerz durch die Morphium-Injectionen vernichtet" (Levinstein, 1877, 3-4).[102]

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[101] There is considerable debate about the extent to which the often cited linkage between the American Civil War and widespread morphine use is a myth, an exaggeration, or a justified historical narrative.

[102] […] "every abnormal sensation" but the general public developed their own enthusiasm for the remedy and soon "[even] psychic pain was annihilated by morphine injections."

Chloral hydrate also entered medical parlance in 1869 when Oscar Liebreich published *Das Chloralhydrat ein neues Hypnoticum und Anaestheticum und dessen Anwendung in der Medicin* on his discovery of the effects of Liebig's chloral hydrate. Practically overnight, the first synthetic drug came into widespread use, both within and without the medical space. Months after Liebreich's paper, the price to produce chloral decreased by nearly 90%, and chloral quickly became more popular than other sedatives like bromide and opium (Snelders et al., 2006, 110). From late 1869-1871, sufficient chloral had been imported to England to provide half a million doses, while by the summer of 1870 German factories were already producing 70 000 doses of chloral per day (Snelders et al., 2006, 111). When Heinrich Byk founded his pharmaceutical factory in 1873, it was initially founded with the express purpose of manufacturing chloral (Fischer, 1998, 13, 39).[103] Chloral possibly found its way into asylums, hospitals, pharmacies, and patent medicines across Europe and America faster than any other intoxicant in history at that time.

A child could be born in a time where alkaloids as a type of thing to be in the world were beyond consideration and yet, by their 60th birthday, treat an old wound with morphine-based patent medicines, toast their health over whiskey, use chloral as a sleep aid, and treat the hangover with a cocaine tonic—should they so desire. The latter nineteenth century had been revolutionized not only by the ever broadening range of never before seen intoxicants in different preparations but by the general public's practically unfettered access to inebriates. Not only that, patent medicine companies and pharmaceutical manufacturers could not have reached as many customers were it not for the aggressive and often misleading advertising campaigns they used to

[103] Heinrich Byk's Berlin-based chemical factory later greatly expanded, and now produces additives and measuring instruments under the name BYK Additives & Instruments.

hawk their wares. Nevertheless, powerful intoxicants had swiftly become a critical facet of the changing social order.

Yet, even as the din of pharmaceutical production rose ever louder, questions about the significance of intoxicants in the study of living bodies generally remained far afield. However, the emerging science of experimental psychology, then principally a science concerned specifically with the experience of embodiment, opened the door to a novel encounter with intoxicants. Here, the immediate experiential interface between mind, body, and world made possible in intoxication could at last garner rational validity as the object of an experimental science, radically shaping the emergence of the biological subject in the twilight years of the 19^{th} century. At the forefront of this encounter with intoxicants, testifying with his own body and mind, was the psychiatrist and acolyte of Wundtian psychology, Emil Kraepelin.

Chapter 9—The Life and Times of Emil Kraepelin: Drugs, Bodies, and Minds

Kraepelin, In Search of Morphine Dreams

Years before he moved to Leipzig and joined Wundt's lab, Emil Kraepelin had already developed a sensitive understanding of intoxicants. It happened late one evening while Kraepelin was serving as Franz von Rinecker's student researcher in Würzburg. The night in question was sometime in the winter of 1877/1878. At the time, Kraepelin, then 22, had been anxiously preparing for his qualifying *Staatsexamen*, to be completed that summer (Kraepelin, 1983, 9). Busy working on sphygmographic studies, Kraepelin was often working "bis tief in die Nacht hinein und verlor dadurch zeitweise den Schlaf" (Kraepelin, 1983, 9).[104] It is easy to imagine the collective impact of youthful insecurity, academic pressure, and sleepless nights. This was also taking place in the 1870s—a bold new world of intoxicating remedies. Thus, it felt unproblematic, even reasonable, when one night Kraepelin gave himself, "um am nächsten Morgen frisch zu sein, eine Morphiumeinspritzung von 0,02 gr," around 20 mg (Kraepelin, 1983, 9).[105] Though his intention was therapeutic in nature, the young Kraepelin's casual attitude towards using morphine without a clear medical purpose was typical of the time. Having prepped a syringe with what he believed to be the proper dose, Kraepelin gave himself the morphine injection, presumably subcutaneously. What followed was not a night of much-needed rest, of respite—but a long sleepless night of clammy nausea, retching, and vomiting (Kraepelin, 1983, 9). The *pièce de résistance* was the very first of the many migraines that would plague Kraepelin's life (Krapelin, 1983, 9). The silver-lining, as Kraepelin acknowledged in hindsight,

[104] […] "until deep into the night, and thus lost sleep at times."

[105] […] Kraepelin gave himself, "in order to be fresh the next morning, a morphine injection of 0.02 gr."

was that this visceral experience shielded him from coming to use morphine as a sleep aid, a habit that could easily turn to morphinism, a harsh judgement already passed on some of his colleagues (Kraepelin, 1983, 9). This was Kraepelin's personal introduction to the new world of intoxication, a relationship which would in many ways shape the course of his professional life. Kraepelin would carry this experience into Wundt's lab, where he in the early 1880s would conduct the first experiments on the psychological effects of various intoxicants.

Before Wundt's Lab

Born in 1856 in Neustrelitz, Emil Kraepelin was the youngest child of Karl Kraepelin, an actor who filled Kraepelin's childhood with literature, music, and theatre (Kraepelin, 1983, 1). However, it was the influence of his brother Karl, 8 year Emil's senior, that ultimately proved determinative, introducing Kraepelin to botany, zoology, and other sciences of life (Kraepelin, 1983, 3). Kraepelin relays in his memoirs that this was also how he initially became familiar with the work of Wilhelm Wundt, who his brother reportedly held in high esteem (Kraepelin, 1983, 3). A second influence on the course of Kraepelin's young life, and at least comparable to his brother's, was that of his father's doctor friend Louis Krueger, who allowed the young Emil to visit on hospital rounds and from whose library Kraepelin borrowed Wundt's *Vorlesungen über die Menschen- und Thier-Seele* (Kraepelin, 1983, 3). On the guidance of Krueger, Kraepelin began studying medicine at Leipzig in 1874 with the hope of pursuing psychiatry.

At Leipzig, Kraepelin attended zoology lectures with Rudolf Leuckart, as well as courses on chemistry and dissection with Gustav Wiedemann (Kraepelin, 1983, 3-4). The summer of 1875 Kraepelin continued his studies in Würzburg. Here, Kraepelin studied chemistry with Johannes Wislicenus, anatomy with Kölliker, and sat in on the psychiatry training courses led by Franz von Rinecker, the pharmacologist (Kraepelin, 1983, 4). Kraepelin also became well-acquainted with Hans Gierke, who allegedly informed Kraepelin of the recent publication of Wundt's *Physiologische Psychologie* as well as Wundt's plans to leave Zürich for Leipzig. According to his memoirs, Kraepelin then immediately knew he had to return to Leipzig, and have the chance to study under one of his teenage heroes, the wellspring of his initial interest is psychology (Kraepelin, 1983, 5). Though he was able to meet Wundt around the Easter of 1877, Kraepelin returned to Würzburg to accept the position of a psychiatry assistant under Franz von Rinecker—where Kraepelin had his ill-fated experience with morphine injection (Kraepelin, 1983, 6). Here, his role as a student doctor had him treating all manner of cases, in addition to updating Rinecker on the status of psychiatric patients, and measuring the contours of their skulls (Kraepelin, 1983, 7). Kraepelin recalls this being a particularly challenging period in his young life. The psychiatry hospital was poorly staffed and ill-equipped, and the inability to monitor patients at a risk of suicide led to liberal use of chloral hydrate (Kraepelin, 1983, 7).

According to Kraepelin's memoirs, it was while he was working in the psychiatry hospital in Würzburg that he wrote "Ueber den Einfluss acuter Krankheiten auf die Entstehung von Geisteskrankheiten," though some secondary literature suggests that this was written while Kraepelin was in Leipzig (Kraepelin, 1983, 9; Allik and Tammiksaar, 2016, 319). This paper is often referred to as "prize-winning," although Kraepelin recalls that technically no one else competed (Kraepelin, 1983, 9). This publication nevertheless provides insight into Kraepelin's identity as a student-psychiatrist/psychologist, prior to completing his doctorate in München and working in Wundt's lab. Kraepelin himself described "Ueber den Einfluss" as a review of the extant literature on the subject, unified through the lens of Wundt's "Mechanik der Nerven und Nervenzentren" (Kraepelin, 1983, 9). What "Ueber den Einfluss" demonstrates is the consistency of Kraepelin's early admiration and awareness of Wundt's work, here even extending it into new territories of study.

After completing his *Staatsexamen* in July 1878, Kraepelin took on an assistant position at an asylum in München under the supervision of Bernhard von Gudden, where he completed his doctorate. The period between 1878-1881 was formative for Kraepelin, in large part because it helped define Kraepelin's perspective as a psychiatrist. Gudden was a brain-psychiatrist, an empirical neo-mechanist paradigmatically associated with Meynert—though Gudden's careful approach to brain anatomy led him to criticize Meynert's audacity (Kraepelin, 1983, 11-17). As discussed earlier, it had been Gudden who had pioneered the method for using a microtome in brain sectioning. By that time, Kraepelin had already become a personal disciple of Wundt—very likely privately adhering to a doctrine of psychological parallelism rather than material reductionism. Kraepelin saw neo-mechanism as a reaction to the short-comings of *Naturphilosophie*, with one extreme being exchanged for the other (Engstrom, 2016). Thus, Kraepelin was weary of Gudden's understanding of mental illness, which saw the advancement of the medical understanding of mental illness as reliant on further developments in neurophysiology and cortical anatomy (Kraepelin, 1983, 11-17; Steinberg and Himmerich, 2013, 249). If he wanted to witness a real shift in the understanding of the nature of mental illness and help patients rather than merely sedate them, Kraepelin understood that he would need to delve into the psycho-mental phenomena that comprised the experience of mental illness itself (Steinberg and Himmerich, 2013, 249). The pursuit of this goal finally led Kraepelin back to where he had long intended to be: in Leipzig, working alongside Wilhelm Wundt.

Leipzig, Flechsig, and Wundt

Kraepelin made his way to Leipzig in February 1882, where he made short work of pursuing employment with Wundt (Steinberg and Himmerich, 2013, 249). Unfortunately, Wundt was unable to provide a paying job for Kraepelin. Instead, Wundt suggested that Kraepelin apply to be an assistant psychiatric physician in the new university hospital with Paul Flechsig. What followed would mark the beginning of a life-long personal and professional dispute between Flechsig and not only Kraepelin, but Wundt as well (Hlade, 2021, 5). Flechsig accepted Kraepelin's application, but their relationship seems to have almost immediately run awry. By Kraepelin's account, their relationship grew strained when Kraepelin refused Flechsig's offer to assist in habilitation—however, this story is not supported by any external evidence (Steinberg and Himmerich, 2013, 249). Whether such an interaction ever took place, it is clear that Kraepelin had little interest in Flechsig beyond the financial support the position provided. From their letters, we learn that Kraepelin submitted an informal habilitation project proposal to Wundt directly after being accepted for the position in the university hospital (Steinberg and Himmerich, 2013, 249). While certainly far from honourable, Kraepelin's conduct here is at the very least understandable. Flechsig was yet another brain psychiatrist, a neo-mechanistic neurophysiologist who saw psychological phenomena reflected in the sheen of a neural cross-section. In short, Flechsig was precisely what Kraepelin hoped to escape in finally joining Wundt in Leipzig. It is likely that Flechsig could have come to appreciate that Kraepelin ultimately had a greater interest in experimental psychology, as long as he fulfilled his duties in the hospital effectively. Of course, that is not what happened. It appears that, as Kraepelin increasingly focused on carrying out his research in Wundt's lab, tensions between Kraepelin's and Flechsig's competing interest resulted in Kraepelin's dishonourable dismissal in mid-June 1882 (Steinberg and Himmerich, 2013, 249-250; Hlade, 2021, 5). Flechsig naturally felt taken advantage of, a factor which complicated the grounds of Kraepelin's dismissal, but it seems to be the case that

Kraepelin did neglect his duties and instead focused his time on his psychological research with Wundt (Steinberg and Himmerich, 2013, 249-250; Hlade, 2021, 5). This all provides context for the purposes of our story, which is concerned above all with the nature of the psychological research Kraepelin was doing that caused him to fall short of his employment obligations.

Intoxicated Testimonies I: Kraepelin's "Proposal," Exner, Dietl and Vintschgau

In a series of enthusiastic letters to Wundt leading up to Kraepelin's move to Leipzig, Kraepelin wrote to a hero of his teenage years about his hopes and aspiration for the future. Perhaps most crucially, Kraepelin wrote to Wundt about his plans to habilitate in Leipzig, putting forward the following informal proposal:

> Allein – ein Königreich für ein Thema! Um einen Stoff zu haben, der wenigstens mit einer gewissen Wahrscheinlicheit zu irgendwelchen Resultaten führen würde und den man beliebig begrenzen könnte, würde ich etwa daran denken, nach Art von Dietl u. Vintschgau einige [2/3] der bekannteren Nervina (Chloralhydrat, Bromkalium, Haschisch, etwa auch Amylnitrit, Strychnin etc.) in ihrer Einwirkung auf die Dauer der Reaktionszeit zu untersuchen. Das wäre zwar nicht gerade originell, aber doch zweckentsprechend. Meine weitergehenden Pläne, Fieberkranke, Nervöse, Asthenische, Alcoholiker etc. zu untersuchen, um so dem Wesen der „psychopathischen Disposition" näher zu kommen, würden natürlich Jahre oder Jahrzehnte lang in Anspruch nehmen und somit für eine bloße Habilitationsschrift ad hoc viel zu weit führen (Kraepelin, 1881a, 1-2).[106]

[106] Alone - a kingdom for a subject! In order to have a topic which would proceed at least with a certain probability to any results and which one could limit arbitrarily, I would think, for example, to examine in the manner of Dietl and Vintschgau some [2/3] of the better known Nervina (chloral hydrate, bromine potassium, hashish, amyl nitrite, strychnine etc.) and their influence on the duration of the reaction time. This would not be exactly original, but it would be appropriate for the purpose. My further plans to investigate fever patients, nervous persons, asthenics, alcoholics, etc., in order to come closer to the essence of the "psychopathic disposition", would, of course, take years or decades and thus be much too far-reaching for a mere habilitation thesis ad hoc.

In his typical self-deprecating tone, Kraepelin describes his proposal as "nicht gerade originell" (Kraepelin, 1881a, 2).[107] His claim to unoriginality likely refers to Kraepelin's awareness of Exner's 1873 "Experimentelle Untersuchung der einfachsten psychischen Processe, Erster Abhandlung" and Dietl and Vintschgau's quite extensive 1877 "Das Verhalten der physiologischen Reactionszeit unter dem Einfluss von Morphium, Caffee und Wein." There was also his focus on psychological reaction time, already Wundt's foundational research program. The question then becomes: how were these previous studies conducted, and how did Kraepelin's proposed project differ?

As previously discussed, Sigmund Exner's 1873 "Experimentelle Untersuchung" combined Bessler's personal equation with Helmholtz and Baxt's observations on the influence of temperature on propagation speed to introduce the physiological concept of reaction time. Helmholtz and Baxt's findings further led Exner to propose "dass die Nervenleitungsgeschwindigkeit überhaupt sine sehr variable Grösse ist, dass ihr jedesmaliger Werth vielleicht noch von vielen anderen Umständen, ausser der Temperatur, abhängt" (Exner, 1873, 601).[108] One factor which Exner found to consistently affect reaction time was tiredness or fatigue (Exner, 1873, 627). Via this association between tiredness and reaction time, Exner arrived at the idea of using narcotics and stimulants in some of his experiments, on the premise that they might function as modifiers of the subject's state of exhaustion. (Exner, 1873, 627). Much to Exner's astonishment though, neither three cups of strong tea nor a subcutaneous injection of morphine had any meaningful effect on the subject's reaction time score (Exner,

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[107] […] "not exactly original."

[108] […] "that the nerve conduction velocity is a very variable quantity, and that its value may depend on many circumstances other than temperature."

1873, 627-628). Two bottles of Rhine wine did have a measurable effect, making the subject's scores more erratic and inconsistent (Exner, 1873, 628). As far an investigation into intoxicants was concerned, this was the extent of Exner's consideration: the inclusion of tea, morphine, and wine were only secondary, founded on an association which Exner's own finding appeared to dispute. The readings from the morphine and tea trials were not even included in the data tables. Further still, intoxicants were only employed in a limited battery of eye to hand reaction time tests, and not applied in tests on different stimulus points, such as reaction time from hand to hand, right hand to right foot, etc.

In 1877, Michael Dietl and the Austrian physiologist Maximilian von Vintschgau took it upon themselves to further Exner's limited drug experiments at the physiological lab in Innsbruck. The project was an entailment of their own interest as physiologists in the temporal relationship between nervous activity and muscle function. Vintschgau had been a protégé of Ernst Brücke, a foundational neo-mechanist, and had served as his assistant in Vienna's physiological institute from 1856-1857, where Exner had also studied in 1865. Dietl and Vintschgau were very upfront in identifying their research as an effort to pursue an otherwise incomplete component of Exner's research program, though they made a series of noteworthy methodological changes. They substituted tea for black coffee, a somewhat personal choice (Dietl and Vintschgau, 1877, 316). Dietl and Vintschgau further decided to limit their experiments to the reaction time of a tactile sensation on the inside of the middle finger, as opposed to the broad range of stimulus points examined by Exner (Dietl and Vintschgau, 1877, 318). The rationale for this was greater experimental control, and because a diverse set of tests were not needed to establish if there was a quantitative difference when morphine, wine, or coffee were introduced (Dietl and Vintschgau, 1877, 318-319). Dietl and Vintschgau also differed from Exner in the design of their measurement apparatus. The apparatus used by Dietl and Vintschgau was one which Vintschgau had used in an earlier 1875 study on reaction time with Hönigschmied. Here, when the subject's hand was touched with a brush device their reaction closed a circuit which caused a fixed pen to leave a mark on a paper covered cylinder, rotated at a set speed by a Helmholtz electromotor (Vintschgau and Hönigschmied, 1875, 2-8; Dietl and Vintschgau, 1877, 319-320).

For the wine trials, Dietl and Vintschgau determined that champagne was the most agreeable wine, with the most excitatory effects—therefore they procured a stash of Roederer champagne, with one attempt made with Tyrol wine (Dietl and Vintschgau, 1877, 368). Much like Exner, Dietl and Vintschgau found that wine greatly affected the consistency of their results (Dietl and Vintschgau, 1877, 376-377). Though, they themselves admitted that "betrachten wir diesen Weinversuch näher, so ist ein sicheres Resultat eigentlich nicht zu finden" (Dietl and Vintschgau, 1877, 381).[109] In this sense, their wine trials were generally consistent with Exner's results. However, they were able to contradict Exner's findings when it came to morphine and caffeine. By doing series of tests back-to-back, Dietl and Vintschgau observed that reaction time consistently reached peak lengths 20 minutes following a morphine injection, only for the reaction time to shorten again after 40-60 minutes (Dietl and Vintschgau, 1877, 357). As for the coffee trials, they found "dass durch den Caffée die Reactionszeit auffallend verkürzt werden kann" (Dietl and Vintschgau, 1877, 368).[110] Dietl and Vintschgau concluded with the suggestion that the divergence between their findings and Exner's could be explained by methodological differences.

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[109] […] "if we take a closer look at this wine experiment, we cannot really find a certain result."

[110] […] "that the reaction time can be remarkably shortened by coffee."

Exner, as well as Dietl and Vintschgau, conducted studies on the relationship between reaction time and intoxication. But the nature of reaction time itself had been the landscape across which the battle for the legitimacy of Wundt's experimental psychology had been fought and, in the eyes of some, won. Exner, Dietl, and Vintschgau comparatively understood reaction time as a strictly physiological phenomena. This put their research at odds with Wundtian psychology. In his research on reaction time, Wundt had uncovered a founding research program, one which justified the realization of experimental psychology as a distinct branch of science. Yet, Wundt's successes raised further theoretical difficulties. At the very core of Wundt's conception of psychology was the principle that a certain share of mental life was psychical and could not be reduced to the physiology of the brain. Wundt's colleagues and competitors, among them Flechsig, Meynert, and Wernicke, had in many ways failed to make good on the promises of neural reductionism in their approach to understanding mental states.

Wundt hadn't sought to overturn the initial premises of the neo-mechanists, but rather to broaden the scope of scientific research in order to account for the shortcomings of their conclusions. This raises a burning question: what experimental evidence was there to account for these two seemingly contradictory statements: the body is a physio-chemical entity, while mental states are not reducible to neural states? Was it merely a speculative assertion, a secularization of Fechner's psycho-spiritual? It is very possible this was not at all an issue for Wundt himself. Nevertheless, Kraepelin's proposal implicitly, and perhaps explicitly, provided a bridge, an experimental subject through which to realize the nature of this connection and, through it, understand the nature of the body and mind. As he expressed in his letter, Kraepelin hoped that psychological research on states of intoxication would glean valuable insights about psychopathology, and thereby the ultimate nature of the psychical (Kraepelin, 1881a, 1-2).

Exner, Dietl, and Vintschgau's research meanwhile had been a natural entailment of Helmholtz's research program centred on the nerve impulse and the physiological nature of reaction time, an elucidation of the neo-mechanistic conception of the body. Kraepelin's proposed project shifted the emphasis from the physiology of intoxicated reaction time to the psycho-mental, *embodied*, phenomena of intoxication.

Intoxicated Testimonies II: Kraepelin's Reaction Time Trials

Published in Wundt's *Philosophische Studien* in 1883, the first section of Kraepelin's "Ueber die Einwirkung einiger medicamentöser Stoffe auf die Dauer einfacher psychicher Vorgänge" discussed Kraepelin's reaction time experiments with alcohol, morphine, chloral, tea, amyl nitrate, ether, and chloroform, though only the trials on amyl nitrate, ether, and chloroform were sufficiently conclusive to merit inclusion at the time. "Ueber die Einwirkung" was preceded in 1881/1882 by a cursory outline of some of Kraepelin's earliest results, published under the titles "Ueber psychische Zeitmessung" and "Ueber die Dauer einfacher psychischer Vorgänge." Though comparably light on details, "Ueber psychische Zeitmessung" serves as something of an introduction to the sprawling, multi-decades long project which Kraepelin had just begun. Here, for the first time, Kraepelin introduces the principle that would guide all of his work with intoxicants: intoxication is model psychosis (Kraepelin, 1881c; Ban, 2006). It is this concept of intoxication as model psychosis that would ultimately make possible not only the unity of experimental psychology with psychiatry, but a novel conception of the relationship between the body and mind.

Methodologically, Kraepelin's reaction time trials built on the experimental design more or less standardized in Wundt's psychology laboratory, and thus the apparatus fundamentally differed from those used by Exner or Vintschgau, and Dietl. The experiments were conducted with the assistance of the newest version of Hipp's chronoscope, a favourite of Wundt's (Kraepelin, 1883a, 419). The newer design distinguished itself not only by its more robust construction, and "unter Anwendung des Princips der Nebenschließung auch diejenigen Versuchscombination mit einem einzigen galvanischen Strome herzustellen, zu deren Durchführung man früher zweier getrennter Ströme bedurfte" (Kraepelin, 1883a, 420).[111] This made the following experiment design possible:

> In unsern Versuchen war die Einrichtung derart getroffen, dass sowohl durch den Reiz wie durch die Reaction ein Strom geöffnet wurde. Zu diesem Zwecke wurde in der von der batterie E kommenden Strom ein Stromwender W eingeschaltet, von welchem aus jenem Zwei Wege offen standen, nämlich einmal durch den [...] Schalltrichter S oder den Fallapparat F, andererseits durch das Galvanoskop G, das Rheochord Rh, die Unterbrecher U und endlich das Chronoskop Ch. (Kraepelin, 1883a, 420).[112]

Using the apparatus, Kraepelin would test not only the simple reaction time of subjects, but also their discrimination reaction and choice reaction times. As the stimulus, Kraepelin elected for vocal vowel sounds, largely because of the relative ease and flexibility of this method (Kraepelin, 1883a, 421). While not groundbreaking, the ability to control for the timing of the stimulus within the experimental apparatus itself added a great deal of credence to Kraepelin's

[111] […] "by applying the principle of shunt closure, it is also possible to produce those combinations of experiments with a single galvanic current which previously required two separate currents."

[112] In our experiments, the set-up was such that a current was opened both by the stimulus and by the reaction. For this purpose, a commutator W was connected to the current coming from the battery E, from which two paths were open, namely through the [...] bell S or the drop apparatus F, on the one hand, and through the galvanoscope G, the rheochord Rh, the interrupters U and finally the chronoscope Ch, on the other hand. [See Figure 1 for a diagram].

experiments, as well as reflecting the strength of apparatus-based experimentation in Wundt's laboratory in the years following it's formation.

For the trials themselves, Kraepelin relays that subjects abstained from tea, coffee, or alcohol in the hours prior to a test, though how many hours is not specifically identified. In order to administer the chloroform, Kraepelin attached an inhalation mask to an iron frame, into which he could attach a chloroform soaked sponge as needed (Kraepelin, 1883a, 430-431). Amyl nitrate could have left a lingering odour, so instead a glass funnel containing a handkerchief soaked in a few drops of amyl nitrate was used (Kraepelin, 1883a, 431).[113] Both versions of this inhalation apparatuses raised novel experimental obstacles that needed to be controlled against in Kraepelin's study. Beyond the confounding effects of the experimental setting itself, just being positioned on the inhalation rig alone could foreseeably affect reaction time, while further concerns were raised about the extent to which a powerful odour—intoxicating or not—might factor in (Kraepelin, 1883a, 431). Accounting for the former concern was accomplished by running the pre-intoxication control tests with the mask in position. Out of an abundance of caution for the latter objection, Kraepelin ran a set of tests "mit reichlicher Inhalation von Aqua rosarum," which did result in a slight, though relatively inconsequential, delay in reaction time of 0.015 seconds (Kraepelin, 1883a, 431).[114] Neither adjustments could account for the near impossibility of accurately determining the dosage of an inhaled substance.

The amyl nitrate trials made up the majority of the inhalation experiments. They were conducted with a dosage that ranged between 4 and 10 drops, though the duration of each inhalation varied considerably (Kraepelin, 1883a, 433). In clinical and anatomical terminology,

[113] See Figure 2 for a panel of Kraepelin's graphs comparing reaction times with amyl nitrate.

[114] […] "with deep inhalation of rose petal water."

Kraepelin described how the inhalation of amyl nitrate began with an irritation in the nose, followed by an increased heart rate and pulsation in the head, before a general feeling of dull drowsiness overtook the subject's senses (Kraepelin, 1883a, 433). This perception was mirrored in the findings of the reaction time trials. As Kraepelin remarks, "zu diesem Wechsel des subjectiven Befindens standen die Resultate der objectiven psychometrischen Untersuchung in bemerkenswerthem Parallelismus" (Kraepelin, 1883a, 433).[115] Kraepelin found that the experience of intoxication elicited by amyl nitrate markedly slowed the subject's reaction time, beginning to improve immediately after the cessation of further inhalation (Kraepelin, 1883a, 435). After the initial lengthening phase, the subject's reaction time not only returned to baseline it very often shortened for a period of time (Kraepelin, 1883a, 437-439). These findings were reproduced in tests of simple reaction time, as well as discrimination reactions and choice reactions (Kraepelin, 1883a, 437-439).

Ether, Kraepelin reported, was far more pleasant than amyl nitrate (Kraepelin, 1883a, 442). Between 1 and 2 grams of diethyl ether was applied to a sponge and inhaled for 1-2 minutes on average, though one attempt extended inhalation up to 13 minutes (Kraepelin, 1883a, 442).[116] After a few breaths, the heart quickened and a mounting feeling of sleepiness overtook the subject. Kraepelin likened the experience to when one falls asleep quickly: the environment rapidly fades away, sounds become dampened and metallic, and the course of mental activities come to a halt (Kraepelin, 1883a, 442). All that remained of any serious content was the perception of stimulus, to which the subject, Kraepelin reported, responded only reflexively,

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[115] […] "the results of the objective psychometric examination were remarkably parallel to this change of the subjective state."

[116] See Figure 3 for a panel of Kraepelin's graphs comparing reaction times with ether.

without any seeming act of will or intent (Kraepelin, 1883a, 442). With such an account of the effects, it is little surprise that Kraepelin consistently found that ether lengthened reaction times. Though, much like with amyl nitrate there was a consistent shortening of reaction times in the second phase which followed the initial lengthening phase. Here, Kraepelin further differentiated between *leichte* and *tiefere Narcose,* with deep narcosis having a markedly greater effect both in lengthening and shortening (Kraepelin, 1883a, 445). Discrimination reactions were tested, albeit in such few numbers so as to be inconclusive—though Kraepelin nevertheless shared his findings that ether further lengthened discrimination reactions (Kraepelin, 1883a, 446).

The chloroform trials were similar to the ether trials. Regarding dosage, Kraepelin found that the amount of chloroform applied was less important than the duration of inhalation, which on average lasting 2-3 minutes with some only lasting for 30 seconds and others going up to 9 minutes (Kraepelin, 1883a, 452).[117] After a few short breaths, the nose grew irritated and, shortly after, a sudden, enjoyable feeling of fatigue overtook the subject, coupled with a general calm. Kraepelin goes on to describe how the subject's perceptions of the outer world would then grow ever more indistinct, with the sounds of apparatus beginning to sound tinny, muffled, and distant, punctuated later but a sudden return to wakefulness (Kraepelin, 1883a, 452). Much like the amyl nitrate and ether trials, there was an initial lengthening in reaction time followed by a second phase, characterized by a shortening of reaction time (Kraepelin, 1883a, 453-454). The magnitude of the lengthening and shortening phases relative to baseline was found to be strongly associated with the intensity of the subject's state of intoxication. This meant that a deeper state of narcosis was consistently correlated with a more acute lengthening of reaction times, as well as a more pronounced shortening in reaction time in the second phase (Kraepelin, 1883a, 454). In

[117] See Figure 4 for a panel of Kraepelin's graphs comparing reaction times with chloroform.

the chloroform trials, Kraepelin observed that peak lengthening in reaction times occurred just after the cessation of inhalation, corresponding with experience of suddenly returning to a state of alertness (Kraepelin, 1883a, 455). Very few differentiation or choice reactions were tested with chloroform. As with simple reactions, the intensity of the state of intoxication was a consistent predictor of reaction time, both in the lengthening and shortening phases of the chloroform trials (Kraepelin, 1883a, 456-457).

For the first section, Kraepelin provided very little by way of closing remarks, though what little he did choose to address was telling. Naturally, Kraepelin addressed his compelling findings on the two distinct phases formed in the association between intoxication and reaction time (Kraepelin, 1883a, 461). He also felt the need to address the dynamic phenomena of individual differences in reaction time (Kraepelin, 1883a, 462). But his most crucial point spoke to the importance of further research into this subject matter. First of all, even between relatively similar intoxicants like chloroform, amyl nitrate, and ether, Kraepelin found that there were measurable differences in their effects, validating further questions about the ever growing host of intoxicants in 19th century society (Kraepelin, 1883a, 461-462). Most crucially though, Kraepelin expressed his belief that experiments on intoxicants such as these, with the advent of further isolated experiments, provided the greatest insight into the ultimate nature of the inner structures of the mind (Kraepelin, 1883a, 461-462). This insight would prove pivotal for the next 20 years of Kraepelin's research, if not his entire career.

The second section of "Ueber die Einwirkung" focused exclusively on alcohol. While Exner, Dietl, and Vintschgau had not researched any of the inhalants that Kraepelin had, the effects of wine on the physiology of reaction time had been a primary feature of their trials. This was Kraepelin's opportunity to strike out against the Wundt's detractors by producing a study that clearly demonstrated the dynamic, psychical nature of reaction time. In light of this, Kraepelin is quick to critique the value of these earlier studies. One need look no further than the first page. Some of Kraepelin's criticisms were methodological: Dietl and Vintschgau didn't conduct more than 7 experiments between them, and the arbitrariness with which they selected one form of alcohol over another raised reasonable questions about whether the alcohol content of the chosen wines were the sole factor at hand (Kraepelin, 1883b, 573-574). The principle ground of Kraepelin's rebuke was that Dietl, and Vintschgau had relied exclusively on simple reaction time (Kraepelin, 1883b, 574). This was arguably a reflection of the competing concepts of reaction time that pervaded the different studies. Both Exner and Dietl-Vintschgau had understood simple reaction time to be the most basic expression of the physiological process common to more complex actions, such as recording the time at which an object passed over a telescope's reticle. Kraepelin, like Wundt, would have characterized the problem in astronomy that gave rise to Bessler's personal equation as a choice reaction rather than a simple reaction—exemplifying Kraepelin's assertion that reaction time needed to be treated as a psychically dynamic phenomena. Though Kraepelin's trials on amyl nitrate, chloroform, and ether themselves contained only a limited number of more complex reactions, the sum of Kraepelin's tests involving choice or discrimination reactions well exceeded the total number of drug trials of any kind conducted by Exner and Dietl-Vintschgau.

That said, Kraepelin took his own criticisms of Exner's and Dietl-Vintschgau's studies to heart, and incorporated these criticisms into the design of his own experiment. To account for the ambiguous nature of drinks like wine and beer, subjects were given a solution of pure alcohol and water, with nothing but a small amount of raspberry syrup to improve the flavour (Kraepelin, 1883b, 574-574). Rather than limit his study to simple reaction time, Kraepelin incorporated more complex reactions, as he had in the first section, and ran these trials in greater numbers.

Dosages of pure alcohol ranged between 7.5 and 60 gr., with trials with smaller doses lasting approximately 40-50 minutes and larger doses up to an hour and a half (Kraepelin, 1883b, 575).[118] Subjects who took smaller doses, under 30 grams, presented as only slightly excited (Kraepelin, 1883b, 576). When larger doses of up to 60 grams were taken, Kraepelin described a pleasant feeling of intoxication which arose after 6-8 minutes, a slight dizziness that kept the sense of personality intact without succumbing to serious drunkenness (Kraepelin, 1883b, 576-577). He goes on to describe how ideas flew by with great clarity and a colourful liveliness, reactions appeared to occur faster and with a pronounced decisiveness (Kraepelin, 1883b, 577). To control against the possibility that a full stomach might affect "psychischen Zeiten," Kraepelin ran control trials using carbonated water instead of alcohol (Kraepelin, 1883b, 575-576). These trials led to a slight lengthening of psychical times followed later by a slight shortening, leaving Kraepelin to speculate about whether the carbonated water had some excitatory effect or whether it was coincidental (Kraepelin, 1883b, 576). Whatever the case, the effect was so slight that any sufficiently strong results uncovered in the alcohol trials were irreducible to a full stomach (Kraepelin, 1883b, 576).

In simple reaction trials, Kraepelin found that low alcohol doses were immediately followed by a shortening in reaction, after which there was a marked, albeit erratic, lengthening in reaction time (Kraepelin, 1883b, 579). The higher doses had a quite different effect: not only was the fluctuating lengthening phase that followed the initial shortening phase at lower doses more erratic and variable, the initial shortening in reaction time at times disappeared altogether

[118] See Figures 5-7 for a panel of Kraepelin's graphs comparing reaction times with alcohol.

(Kraepelin, 1883b, 581-582). In all cases, “die absolute Größe der Verlängerung ist im Allgemeinen durchgehends geringer als die jenige der Verkürzung” (Kraepelin, 1883b, 582).[119] Though the results were fairly consistent across subjects, Kraepelin recognized that the magnitude of the shifts varied significantly between individuals, which he attributed both to each individual’s natural constitution as well as personal familiarity with alcohol (Kraepelin, 1883a, 579, 582).

In trials on discrimination reaction, low dose alcohol produced a relatively consistent shortening in psychical time overall (Kraepelin, 1883b, 584-585). It was at the higher doses that the pattern most closely mirrored those seen in simple reaction time, where an initial shortening in reaction time was followed by an erratic lengthening phase (Kraepelin, 1883b, 588). In the choice reaction trials “war […] das paradigmatische Bild der Alkoholwirkung durch vielfache Schwankungen der Beobachtungswerthe undeutlicher,” although the familiar trends were still visible (Kraepelin, 1883b, 590).[120] Kraepelin attributed this, at least in part, to the characteristic of all choice reactions to be slower and more variable (Kraepelin, 1883b, 590). There remained, nevertheless, an averaged shortening phase, followed by a lengthening phase (Kraepelin, 1883b, 590-592).

It is clear that Kraepelin led the first serious research project into the effects of intoxicants on the mind or the body. But how does Kraepelin himself conclude his initial project? How did he understand it? In the conclusion of the second section, Kraepelin finally offers

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[119] […] “the absolute magnitude of the lengthening is generally consistently smaller than that of the shortening.”

[120] […] “the paradigmatic picture of the alcohol effect was more unclear due to multiple fluctuations of the observed values.”

insight into how he conceives of the nature of the effects of these different intoxicants on reaction time. Regarding differentiation and choice reactions, Kraepelin argued the common element "ist, wie schon oben angedeutet, apperceptive Thätigkeit, die im letzteren Falle als Erfassung des äußeren Eindruckes durch die Aufmerksamkeit, im ersteren als die Vergleichung desselben mit dem bereit gehaltenen Erinnerungsbilde sich darstellt" (Kraepelin, 1883b, 599).[121] This "Thätigkeit ist es, die im ersten Stadium der Alkoholwirkung eine mäßige Beschleunigung, im zweiten dagegen eine ausgeprägte Verlangsamung erfährt" (Kraepelin, 1883b, 599).[122] Alcohol's effect was an initial quickening, followed by a slowing, of the power of apperception, which ultimately suppressed the subject's abilities through " die bei Anwendung größerer Dosen sich regelmäßig bemerkbar machende Erschwerung der Auffasssung äußerer Eindrücke" (Kraepelin, 1883b, 600).[123] This manifests itself as a difficulty in apprehending sense impressions, as well as discerning between them.

This slowing of apperception accounts for the sensory effects of acute alcohol intoxication, but it does not account for the full array of symptoms. For example, Kraepelin found that alcohol led his subjects to anticipate the stimulus, as well as respond to the apparatus more forcefully. To this latter point, Kraepelin identified a disassociation of the power of will

[121] […] "is, as already indicated above, apperceptive activity, which in the latter case represents itself as the apprehension of the external impression by the attention, in the former case as the comparison of the same with the ready held memory image."

[122] This "activity that undergoes a moderate acceleration in the first stage of alcohol action, but a pronounced deceleration in the second"

[123] […] "the impediment to the perception of external impressions that regularly becomes noticeable with the use of larger doses."

from the apperceptional faculty (Kraepelin, 1883b, 600-601).[124] Apperception, Kraepelin pointed out, is the sole avenue by which the subjects had an awareness of their setting, and so these anticipatory responses seen under the effect of alcohol are described not as willful acts but actually as demonstrative of a loss of free will (Kraepelin, 1883b, 600-601). This slight lapse in free will emerges out of the perceptional ambiguity that arises from the "Aufhebung des Unterscheidungsvermögens" (Kraepelin, 1883b, 600-601).[125]

The Experimental Frame and a Nosology of Intoxication

Employing psychological language to describe the effects of intoxicants in his study, Kraepelin clearly delineates the objectives of his research: this is an experimental rendering of the experience of embodiment, of the subject's existence in the milieu of the lifeworld. The scientificity of such a pursuit, a notion which underscored Wundt's entire research paradigm, relies on a robust experimentalism. Kraepelin's earliest drug trials were in many cases experimentally thorough. Where necessary, Kraepelin made every effort to control against confounding factors, from the possible influences of foul odours to the role of a full stomach. This is the experimental crucible that phenomena pass through in their becoming constituted as scientifically valid.

Yet, in essence, Kraepelin's experiments were consistent with the radical empiricism which Wundt inherited from Fechner, and thereby the naturphilosophical, even Brunonian,

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[124] Kraepelin was deeply concerned about the effect of practice, or conditioning, on his results; however, in cases such as these it was clear to Kraepelin that the subject's reaction far exceeded baseline deviations emergent of prior conditioning.

[125] […] "abolition of the ability to discriminate."

empiricism of Fechner's time and place. This is supported by the identities of the subjects enrolled in the experiments themselves. Collectively, fifty three trials were conducted with the amyl nitrate, chloroform, and ether, with twenty six trials being conducted with amyl nitrate, twelve with ether, and fifteen with chloroform. Well over half of those experiments, 33 attempts, were conducted on Kraepelin himself, while six and 14 attempts were carried out on two willing subjects (Kraepelin, 1883a, 430). For the alcohol experiments, 19 of the 47 trials were conducted on Kraepelin himself (Kraepelin, 1883a, 575). Kraepelin's experiments might more accurately be described as an experiential science of embodiment.

But unlike the experiential science of the Romantics, Kraepelin makes every effort to put the experiential in the distal language of the experimental frame. Kraepelin is forthcoming in identifying himself as one of the four test subjects, even as the primary test subject; however, in the language of the study what most often remains is the abstracted *Reagirenden* (respondents). This is clear in the descriptions given for the subjective effects of the different intoxicants which are attributed to the experiences of all the test subjects, and yet upon review almost require that they be written by someone who had experienced these varied states of intoxication first-hand. The experiential components of his research is freely admitted, only to be obscured by the rhetorical expectations that underlie the scientific publication as a vehicle of epistemic permissibility. Where the findings of researchers working with vital substances had been eschewed by early neo-mechanistic on a methodological level, Kraepelin was working within an experimental methodology which could approach the phenomena of intoxication as an expression of embodiment, while still aspiring to scientificity.

The role of self-experimentation and experiential science has interesting implications with respect to Kraepelin's solution to the underlying problem of the uncertain dosage of an inhaled intoxicant, as well as the analogy Kraepelin draws between psychopathology and intoxication. As mentioned, an uncontrollable element in the initial experiments had been the ambiguity surrounding the dosage of an inhalant, and Kraepelin's solution had been to develop a method of categorizing *leichte* and *tiefere Narcose* (light and deep narcosis) (Kraepelin, 1883a, 432). The categorization process necessarily relied on identification of a differentiating criteria of symptoms, which were then applied both to Kraepelin's own state of intoxication as well as those of the other participants. What Kraepelin had developed was a functional psychiatric nosology of intoxication, for the purposes of his experiment.

Though simple, Kraepelin's psychiatric nosology is a remarkable feature of his early study, in light of the overt significance Kraepelin ascribes to controlled experimentation with intoxicants for the understanding of the structure of mental states. Within this classification system, a given substances of intoxication serves as its own category, within which *leicht* and *tiefere* intoxication function as associated and yet distinct manifestations of mental dis-order. Experimentally, this variation is expressed through Kraepelin's organization of reaction times into representative tables and line graphs according to their classification as either *tiefere* or *leichte* intoxication. In the ether experiments, Kraepelin observed that under *leichte* intoxication choice reaction times initially lengthened before shortening following the cessation of inhalation, while *tiefere* intoxication saw an immediate shortening (Kraepelin, 1883a, 449-450). This structure even extends into the alcohol-centred section, despite the absence of the same dosing issues in the alcohol trials. Here too, the results of trials with a diverse range in alcohol dosages are ultimately separated into high and low dose trials. Thus, *tiefere* and *leichte* function as separate diagnoses with their respective symptomatology, even when it is possible to approach degrees of intoxication as a continuum.

Intoxication here is not merely an analogy for mental states, but rather serves as a description of mental states as such. Kraepelin believes that substances of intoxication affect discrete psychological structures which may also be affected by a given mental illness (Kraepelin, 1883a, 461-462). In this sense, intoxication, for Kraepelin, is a form of temporally and psychically localized mental disorder. Kraepelin's experiential reaction time trials then are to be understood as a self-imposed mental dis-order, classified according to a reflective diagnostic process. All of this has profound implications for Kraepelin's parallelism, his understanding of mental illness, and, more fundamentally, the ultimate scientific conception of the relationship between the embodied and the bodily.

The Physical made real in the Psychical

In the identification of intoxication as an experimentally recognizable state of forced psychopathology, Kraepelin proffers his radical reconstitution of Wundt's psychological parallelism. Insofar as Wundtian psychology was borne out of alterity—both interpersonally and conceptually—with the neo-mechanical reduction of the mind to the body, Wundt's model was ill-equipped to integrate physiology as he had hoped. Wundt's parallelism was, as discussed, a secularization of the Fechnerian duality of the ephemeral body-world and the eternal God-mind. As if this were not enough, Wundt himself invoked comparison with Leibniz's dualism. Yet, experimental psychology was flourishing. What might have been a minor shoot growing out of the cracks in neurophysiology had begun to bear fruit. It is clear to see, however, that this was a tenuous position. Neurophysiologists after the fashion of Meynert and Wernicke may have been visibly blundering their hasty equivalences between mental states and brain states, but brain research never stopped. In this sense, the dualism which had been the basis of experimental psychology's formation also threatened the validity of the entire research program. Wundt could exorcise Fechner's ghosts, but scientifically the psycho-mental remained in a state of liminality.

The answer, whether Wundt could ever see it or not, came in the form of substance of intoxication, from amyl nitrate to alcohol. By equating intoxicated states with mental states and passing this principle through the experimental crucible, Kraepelin renders the mind and body as a singular scientific object. It is in the context of Kraepelin's psychological reaction time trials that substances of intoxication intercede in the fundamental questions concerning the relationship between the body and embodiment. Where Wundtian psychology had severed the psychical from the physical, the phenomena of intoxication was the physicality of the body made real in the psychical. Substances of intoxication were material substances, and yet they directly gave rise to totalizing psychopathological processes. Kraepelin's intoxicants functioned as the connection between the scientific encounter with the mind and the understanding of the body in physiology.

Nor was this merely Wundtian parallelism. Wundt's partial parallelism functioned on a hierarchy of mental and physical functions. Rudimentary sensory and mental functions, Wundt argued, were not parallelistic. While broadly differing with the localization theorists, Wundt recognized that vision for example could be explained on a physiological basis, although a cohesive sense of spatial and visual perception implied a separate mental process. It was the realm of higher mental activities that required a parallelist view, and therefore a distinct science of psychology. But Kraepelin's intoxicant study, even on a psychical process as simple as reaction time, had indisputably demonstrated that intoxicants could exact a totalizing influence on mental processes—tearing into the very fabric of perceptional lifeworld of embodiment.

Exner's, Dietl, and Vintschgau's studies in 1873 and 1877, could not have had the same effect. Physiological reductionism found them framing the entire process in bodily terms. The perception of reaction time as a physiological process, as opposed to a dynamic psychical one, influenced their experimental decision to focus exclusively on simple reaction time. Intoxicants themselves were integrated into Exner's study not because of their effects in themselves, but as tools for the modification of physiological processes. Dietl and Vintschgau simply sought to further elucidate an inconclusive subpoint in Exner's research. Through the gathering of experimenters, subjects, and intoxicants in the setting of the psychological, rather than physiological, laboratory, the embodied experience of intoxication becomes integrated with the bodily. Substances of intoxication became testifying witnesses in a trial not of intoxicants themselves but of the ultimate nature of the relationship between the mind and the body. It is only in studying the effects of intoxicants on the mind, on the psychical process, that intoxication could be conceptualized as thinking with the body. Thus, it was on the testimony of intoxicating substances that mind and flesh, embodiment and the body, could be sublated within a unified physiological doctrine, without subjecting mental states to the reductionism of neural states. This was the beginning of a conception of the body which was not mechanical, but dynamically biological.

Simultaneously, the principle of psychological parallelism remains functionally intact as a working description of the perceptional dynamism of embodiment over and against the bodily reductionism of the brain-psychiatrists. However, it functions solely as a framing principle—a conceptual model retained as a bulwark against the naïve materialism of brain-psychiatry. For this reason, Kraepelin has, at times, been labelled as ambivalent or blurry with respect to the overt stance on the mind-body divide (Hoff, 2015, 34). This functional, as opposed to conceptual parallelism, still remains influential to this day, denoting a point of view rather than a "real" description of the mind-body relationship.

Drink and drug of all shapes and kinds had been changing the face of the medicine, of work, of leisure, and of the social order more generally in profound ways over the course of the 19th century. Now, they interceded in the minds and bodies of experimenters and experiments alike. Kraepelin, alongside his fellow subjects, brought drugs into a radically new kind of "place" in the world: the psychological laboratory—a space implicitly characterized by a distinct horizon of epistemic possibility. Even as the early psychology lab employed methods similar to the physiologically-oriented laboratories of Exner, Dietl, or Vintschgau, it existed within a very different constellation of theories and concepts. The apparatuses, cupboards, and work areas themselves were inscribed with the conditions for a distinct set of possibilities. Here, researchers and intoxicants gathered in a novel setting where intoxication could be seen as thinking with the body. Intoxication cast mind as flesh, not only by bringing substance to bear on mind, but by bringing the experience of the mind to the forefront of bodily experience. This was a novel conception of the body and mind entirely at home in an age where all manner of cocaine tonics, chloral knock-out drops, and morphine pain pills not only existed but were ready-to-hand. But, for Kraepelin, this was only the beginning. In the ensuing period, Kraepelin would greatly extend the scope of his newfound pharmapsychological research program, while at the same time aspiring to develop a psychologically-founded science of psychiatry which could compete with the shortcomings of brain-psychiatry.

Chapter 10—Kraepelin's Nosology and an Intoxicated "*Physiologie der Seele*"

The First Compendium (1883)

Aspirations of completing his habilitation by further researching intoxication as a psychopathological phenomenon were cut short when Kraepelin lost his position working under Flechsig in the university's psychiatric hospital. Flechsig's reasons for firing Kraepelin appear justified: Kraepelin increasingly neglected the work he was being paid to do in the hospital, instead focusing on the work he was doing in Wundt's lab (Steinberg and Himmerich, 2013, 249-250). Another possible factor was Flechsig's perception that his hopes of hiring an experienced clinical psychiatrist had been exploited by Kraepelin, in order to secure funding for Kraepelin's own research (Steinberg and Himmerich, 2013, 249-250). Kraepelin's little 'ploy,' as Flechsig might have seen it, now threatened everything Kraepelin had worked toward. The firing left Kraepelin without a source of income, and so he made an urgent request to habilitate using work which he had already completed (Steinberg and Himmerich, 2013, 249-250).

In lieu of a habilitation thesis, Kraepelin submitted three publications: "Ueber den Einfluss acuter Krankheiten auf die Entstehung von Geisteskrankheiten" (1881), Ueber die Dauer einfacher psychischer Vorgänge (1882), and the paper just discussed, "Ueber die Einwirkung einiger medicamentöser Stoffe auf die Dauer einfacher psychischer Vorgänge" (1883) (Steinberg and Himmerich, 2013, 250). His submission was very nearly rejected. One of his reviewers, the physiologist Carl Ludwig, who also happened to be Flechsig's foster-father, decried the papers on reaction time as essentially derivative, lacking in creative ideas (Steinberg and Himmerich, 2013, 250). To Kraepelin's good fortune, Ludwig handed to decision over to Erb, who ultimately granted Kraepelin his habilitation, which brought with it the rank of *Privatdozent* and the ability to charge fees from students (Steinberg and Himmerich, 2013, 250).

By October 1882, just 8 months after first getting the job with Flechsig, Kraepelin was officially part of the teaching staff in Leipzig's medical faculty (Steinberg and Himmerich, 2013, 255).

Kraepelin had narrowly escaped a major set-back. Yet, the premature completion of his habilitation requirements had been to his advantage. Kraepelin now had considerably greater flexibility in where and what he wanted to research. On the other hand, he was all but destitute and in serious need of an income. To help address his dire financial circumstances, Wundt encouraged Kraepelin to write a psychiatry textbook (Hoff, 2015, 32; Heckers and Kendler, 2020, 382). Published in 1883, this little textbook, Kraepelin's *Compendium der Psychiatrie*, was the infant form of the psychiatric nosology that would ultimately secure Kraepelin's place in the annals of medical history.

The first edition was simultaneously an effort to introduce Kraepelin's own ideas derived from Wundt's scientific psychology and a review of what Kraepelin perceived as being the best, current literature in psychiatry (Hoff, 2015, 32-33, Heckers and Kendler, 2020, 381). Although Kraepelin acknowledges the influence of a number of existing psychiatry textbooks, the 1880 edition of Heinrich Schüle's textbook as well as the 1883 edition of Richard von Krafft-Ebing were the most directly formative (Heckers and Kendler, 2020, 381). The nosologies proposed by both authors shared fundamental similarities, although they were slightly differently framed. Schüle classified pathologies as psychic, organic, or psychic-organic. Psychoses, meanwhile, were either psychoneuroses, mind diseases with no change in brain matter, or cerebropsychoses, which involved changes in the brain and were discernible by changes in motor function (Heckers and Kendler, 2020, 381). This more or less arbitrary identification of the affectation of motor function as not only categorically distinct but reflective of a structural change in the brain appears consistent with the relative success that neurophysiology had in localizing motor function. Krafft-Ebing's textbook instead proposed three different nosological approaches (anatomical, etiological, and clinical), which gave rise to three diagnostic categories: pathological illness, disease without pathological findings, and disorders of neurological development (Heckers and Kendler, 2020, 381).

Kraepelin interpolated the ideas of Schüle and Krafft-Ebing, alongside those of Griesinger and Hermann Emminghaus, into the first edition of his compendium. However, Kraepelin had not intended for his psychiatric textbook to merely serve as a gloss on the existing literature, but rather to bring the existing literature into agreement with Kraepelin's own aspirations for psychiatry. In particular, Kraepelin hoped, on the basis of insights gleaned through experimental psychology, "das Verständniss der psychischen Störungen so viel wie möglich zu erleichtern und überall die Wurzeln derselben in der normalen Erfahrung anzudeuten" (Kraepelin, 1883c, viii).[126] Kraepelin's compendium was, at least aspirationally, the declaration of new psychiatry, one in which the ultimate nature of psychopathological processes could be identified by direct experimentation on the mind.

At this point in his life, Kraepelin was far from the clinical nosologist he is known as today. He was more concerned about the effectiveness of diagnostic techniques, which implicitly, rather than explicitly, brought Kraepelin into confrontation with extant nosological systems (Engstrom, 2015, 155). On this point, Kraepelin doubted the value of the anatomical and etiological noso-diagnostic categories identified by Schüle/Krafft-Ebing (Kraepelin, 1883c, 189; Heckers and Kendler, 2020, 382). Despite being incapable of discerning the anatomically healthy nerves from the pathological, neurophysiologists claimed it was brain-science "die uns über das

[126] […] "to facilitate the understanding of mental disorders as much as possible and to indicate everywhere the roots of them in normal experience."

wahre Wesen der einzelnen Störungen aufzuklären berufen sein soll" (Kraepelin, 1883c, 188).[127] Such attacks levelled against the entire host of neurophysiologically minded brain-psychiatrists bordered on ridicule, but they made Kraepelin's position overwhelmingly clear. Physiological pathology could not currently, and perhaps never could, form the basis of a valid diagnostic criterion. This was both because of the immature state of the study of neural anatomy as well as Kraepelin's argument "dass es gelänge, hier konstante Strukturveränderungen aufzufinden, so würden dieselben einen wirklichen Werth doch erst durch die Beziehung auf die gleichzeitig beobachteten Funktionsstörungen gewinnen" (Kraepelin, 1883c, 188).[128] It was hardly different for etiological criteria, where the vast majority of individual cases varied so greatly in the symptomatology and duration of their effects that it was nearly impossible to make firm deliberations with any certainty (Kraepelin, 1883c, 188-189).

In contrast with the etiological and pathological criteria, "am klarsten sind jedenfalls die Beziehungen, welche zwischen der organisch bedingten Funktionsstörung und den klinischen Erscheinungen bestehen" (Kraepelin, 1883c, 189).[129] Here too, distinctions between the presentation of symptoms across groups was exceedingly difficult, but the emphasis on visible symptoms overcame both etiological metaphysics and poorly supported physiological reductionism. The conclusion: "Was es uns bietet, sind nicht etwa Krankheiten, sondern lediglich

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[127] […] "which should be called upon to enlighten us about the true nature of the individual disturbances."

[128] […] "that were it possible to find constant structural changes here, they would only gain a real value through the relationship to the simultaneously observed functional disturbances."

[129] […] "the clearest associations, in any case, are those which persist between the organically caused functional disorder and the clinical manifestations."

Symptomenkomplexe" (Kraepelin, 1883c, 189).[130] While Kraepelin provides independent reasoning for this choice of emphasis, it is difficult to ignore the potential theoretical and experimental influences behind this shift. For one, it brought Kraepelin's early psychiatry into agreement with Wundtian psychology. Wundt had approached everything outside of consciousness in physiological terms since the first edition of the *Grundzüge* (Engstrom, 2015, 154; Araujo, 2012, 41). It was thus consistent with Wundt's methodological philosophy for Kraepelin to favour the notion of symptom complexes—symptoms being the discernible rupturings of disorder into the conscious experience of embodiment.

Yet, this is not a surprising turn for Kraepelin. He had applied this very same approach in his recently conducted research on the effects of intoxicants on the reaction time. There, Kraepelin had overcome the experimental problem of controlling for the variable dosage of an inhaled intoxicant by developing a criteria for the classification of degrees of intoxication. The result had been a functional nosology of intoxication predicated on complexes of discernible symptoms. Although Kraepelin had the benefit of knowing which intoxicant had been consumed beforehand, the symptomatic and experimental expressions of deep and light intoxication were sufficiently distinct to warrant independent classification. This created a circumstance wherein Kraepelin, both reflexively (as examined) and 'clinically' (as examiner), was brought to classify states of mental intoxication solely on the basis of observable symptom complexes. To suggest that Kraepelin's intoxication trials had a direct influence on his diagnostic approach would seem dubious had Kraepelin himself not taken up intoxicants as experimental actors capable of inducing temporary psychopathological states. Kraepelin's decision to favour the clinical identification of symptom complexes as a diagnostic approach was shaped not only by his

[130] "What it offers us are not diseases, but merely symptom complexes."

theoretical faith in scientific psychology, but by recent experimental study on the effects of intoxicants on the human mind. The reaction time trials were intoxication as madness.[131] Thus, in the *Compendium,* madness was as Kraepelin had seen it, experienced it, through the haze of intoxication. Just as the intoxication trials had been a confirmation of the underlying somatic character of mental disorder, Kraepelin's early research on intoxicants had presaged his diagnostic approach. But this was only the beginning for Kraepelin—both in respect to his research on the effects of intoxicants on the body and mind, as well as the psychiatric textbooks that ultimately shaped his legacy.

The Dorpat Psychological Society

For all that he had done between 1880-1883, Kraepelin was still eager to obtain a full professorship as soon as possible. A major motivator was his desire to finally be married. When Kraepelin got engaged in the summer of 1883, his mentor kindly advised him that it would be quite some time before he would be able to offer him anything approximating a full professorship, owing to the immature state of their shared field (Kraepelin, 1983, 29). On the voyage home from a meeting in Freiburg, Kraepelin paid a visit to Gudden, who eagerly proposed a solution to Kraepelin's dilemma (Kraepelin, 1983, 29). Kraepelin would return to Munich to fill a vacancy at the asylum there, while also assuming a lectureship at the university. By the fall of 1883, Kraepelin was once again in Munich (Kraepelin, 1983, 29-30).

There Kraepelin was able to once again study the anatomy of nervous tissue, although the available staining and hardening technique greatly limited Gudden's aspirations of directly studying the relationship between groups of cells and nerve fibres (Kraepelin, 1983, 30-31).

[131] In reference to Kraepelin's concept of intoxication as model psychosis.

Perhaps to distract from this time spent reviewing brain tissue, Kraepelin also bought a personal Hipp chronoscope and obtained all the necessary materials "meine Versuche mit Arznei- und Genußmitteln noch weiter auszudehnen, sodann aber auch an Geisteskranken Zeitmessungen anzustellen, um so eine klarere Vorstellung von den seelischen Veränderungen zu gewinnen" (Kraepelin, 1983, 32).[132]

However, the move to Munich had only been a temporary solution. In 1884, Kraepelin moved to Leubus to take a position at the local asylum. Upon accepting the position, he and his fiancé were married and its seems, for a time, they lived a simple, pastoral existence in Leubus (Kraepelin, 1983, 33). Research there was limited to studying differences in the composition of urine between groups of patients and occasional tests on mental chronometry. In May 1885, Kraepelin took up a job in the psychiatry department in Dresden (Kraepelin, 1983, 36). Here too, Kraepelin was limited in his scientific research, though he recalls being quite happy. That Christmas was spent with the Wundt's, with whom he still remained a close relationship, and the following day he paid a visit to the 85-year old Fechner (Kraepelin, 1983, 38). That April in 1886 Kraepelin finally found a way to make inroads into an academic career. Emminghaus wrote to him that he was leaving his professorship in Dorpat, and he had suggested Kraepelin as a possible replacement (Kraepelin, 1983, 39).[133] Kraepelin was selected for the position and thus

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[132] […] "to extend my experiments with medicines and stimulants even further, and then also to make time measurements on the mentally ill in order to gain a clearer idea of the mental changes."

[133] Dorpat was German name for the modern Estonian, formerly Russian, city of Tartu. Culturally, the city had a long historical association with Baltic-Germans, but in the 18th century came under Russian control, and subsequently went through a gradual process of Russianization. In the time of Emminghaus and Kraepelin, Dorpat was already part of Russia.

began his first professorship in Dorpat (Engstrom & Kendler, 2015, 1191). On the way there, Kraepelin had hoped to introduce his wife to Gudden, only to discover that Gudden had drowned, possibly murdered, alongside King Ludwig II of Bavaria at Lake Starnberg (Freckelton, 2012).

Though Emminghaus was brimming with glee to be returning to Germany from what Arthur Böhtlingk likened to "the desert," Dorpat would be formative for Kraepelin (Steinberg and Angermeyer, 2001, 299). The teaching component of his position faced difficulty at almost every turn. Most of his students were Estonian, Russian, or Livonian speakers and Kraepelin could only address them with an interpreter (Steinberg and Angermeyer, 2001, 301-302). A policy of Russianization also created difficulties for German faculty members, restricting their academic freedom. Kraepelin would receive and order from the Livonian governor to sign documents in Cyrillic later in 1890 (Steinberg and Angermeyer, 2001, 302).

Despite the difficulties, the status of a full professorship nevertheless also gave Kraepelin the latitude to finally continue his scientific work. Now securely in a professorship, Kraepelin was free to continue studying the effects of different intoxicants on mental processes. In 1887, Kraepelin founded the Dorpat Psychological Society, whose membership would conduct research in a regional laboratory for experimental psychology which Kraepelin set up in a small room offered by the university's president Alexander Schmidt (Steinberg and Angermeyer, 2001, 305). Here, and elsewhere at Dorpat, Kraepelin oversaw experimental research on a wide range of psychological topics, including the perception of time and the depth of sleep. But it was also an opportunity for Kraepelin to continue research on the pet project he had started in Leipzig: the influence of intoxicants on mental processes.

Intoxicated Testimonies III: Dorpat and Heidelberg

Much of Kraepelin's research on this subject would not be published until 1892, when he collected his research from his time at Dorpat and later Heidelberg into the first book on the topic. A notable exception is Heinrich Dehio's dissertation 1887 "Untersuchungen über den Einfluss des Coffeins und Thees auf der Dauer einfacher psychischer Vorgänge," which was written on the basis of research conducted in Kraepelin's lab. Given Dehio's frequent reference to his mentor's own experimental objectives, it is worth briefly reviewing Dehio's dissertation before discussing the content of Kraepelin's book.

The objective of Dehio's dissertation was to expand upon Kraepelin's 1883 publication on the effects of amyl nitrate, ether, and chloroform by conducting similar studies on the effects of a stimulant (Dehio, 1887, 7). Dehio chose caffeine, on account of its availability, and made a comparison between the effects of pure caffeine and those of tea central to his experimental design (Dehio, 1887, 8). This was additionally an opportunity to overtake Exner and Dietl-Vintschgau, who had both studied tea and coffee respectively, but done so with methods which Dehio deemed unreliable (Dehio, 1887, 8-9). Kraepelin had published some cursory results on the effect of caffeine, and other intoxicants, on reaction time in "Ueber psychische Zeitmessung" (1881), though these were far from extensive. Unlike these earlier trials, Dehio also administered his subjects subcutaneous injections of pure caffeine at doses of 0.5g, which elicited a light, albeit discernible, state of intoxication (Dehio, 1887, 14). In other cases, subjects drank Russian tea (Dehio, 1887, 14). The majority of the trials were conducted on Dehio himself, while the remaining attempts were made on Dehio's colleague, and fellow student of Kraepelin's, A. Sohrt (Dehio, 1887, 15).

Though Dehio's thoughts are doubtlessly his own, his dissertation provides a snapshot of the methodological and conceptual developments Kraepelin's own work had undergone while he was at Dorpat. Dehio's reaction time trials contained the simple and choice reactions, as Kraepelin's 1883 paper had, but he also tested "word reactions," which saw respondents reading mono-syballic words off cards, as well as "higher reactions" (Dehio, 1887, 23-25, 36). These higher reactions included repeating the next number in a given sequence and doing basic arithmetic (Dehio, 1887, 36-37). The methodological changes led to interesting results: Dehio found that caffeine and tea tended to shorten simple reactions, though it was inconclusive, and they had no discernible effect on choice reactions, but they definitely shortened word and "higher" reactions (Dehio, 1887, 20, 31, 35, 37). In some cases, there was a slight lengthening in reaction times following the initial shortening, but generally the initial shortening was followed by a return to average reaction time and caffeine, as well as tea, had the effect of reducing variation in reactions overall.

From these observations, Dehio was able to draw two primary conclusions. The first was that, at the level of a strict comparison of caffeine dosages between tea and pure caffeine, tea was able to achieve comparable effects on reaction time to pure caffeine, but at a much lower caffeine dosage (Dehio, 1887, 46). This raised questions about whether there was some additional alkaloidal component of the tea which was either directly affecting reaction time or amplifying the effects of the caffeine. The second was conceptual. Kraepelin had proposed that the intoxicating effects of alcohol arose from a slowing of the process of apperception, while the initial shortening in reaction time at low doses was a by-product of the stimulation of will time (Dehio, 1887, 49-50). Dehio suggested that, in contrast, tea had no recognizable effect on will time (demonstrated by the negative effects on choice reactions) and that, although low dose alcohol and tea both initially shortened reaction times, tea did so primarily through a moderate shortening of apperception time, a far cry from the disturbance caused by alcohol (Dehio, 1887, 49-51). Any elongation in reaction times after tea or caffeine was very moderate, and Dehio attributed it either fatigue or a slight elongation in the process of apprehension (Dehio, 1887, 50-51). Dehio, and likely Kraepelin as well, understood these findings as a tentative confirmation of Kraepelin's initial hypothesis that different substances of intoxication modified different psychic processes, and in different ways (Dehio, 1887, 48-49). Such confirmation was, however, only cautiously acknowledged: there were still so many different forms of intoxication to research. Nonetheless, Dehio finished his text on a hopeful note, confident "dass wir hier ein Hülfsmittel gefunden haben, welches uns eine durchaus neue, bisher unerschlossene Bahn naturwissenschaftlicher Forschung zu eröffnen verspricht" (Dehio, 1887, 55).[134]

Heidelberg and Arzneimittels

Dehio's dissertation reflected a development in the theoretical and methodological approach to the study of various intoxicants on mental process in Kraepelin's laboratory during his time in Dorpat. But Kraepelin would not be in Dorpat much longer. On the 9th of November, 1890 Kraepelin received his appointment to a chair at Heidelberg (Steinberg and Angermeyer, 2001, 316). Little is known about how Heidelberg became aware of Kraepelin as a candidate. Surviving letters between Kraepelin and Wundt implied prior contact, and spoke hopingly about the opportunity to bring scientific psychology into an entirely new region (Steinberg and Angermeyer, 2001, 316-317). Kraepelin had always intended Dorpat to be a stepping stone, though the ever strengthening influence of Russianization may well have helped push Kraepelin

[134] …confident "that here we have found a tool that promises to open up a completely new, previously unrealized path of scientific research."

out the door (Steinberg and Angermeyer, 2001, 317). The Russian ministry officials had repeatedly complicated Kraepelin's scientific and clinical work, with Kraepelin going as far as personally overseeing the furnishing, renovation, and installation of linoleum in the clinic, even enlisting the help of his assistants in installing the wiring, in order to avoid meddling by Petersburg (Kraepelin, 1983, 42-45).

Once at Heidelberg, Kraepelin continued his research on experimental pharmapsychology, compiling his work into the book *Ueber die Beeinflussung einfacher psychischer Vorgänge durch einige Arzneimittel* in 1892. Here, Kraepelin discussed the effects of alcohol, tea, morphine, chloral, amyl nitrate, chloroform, ether, paraldehyde, caffeine, nicotine, and cocaine on the wide array of mental processes, including reading, writing, arithmetic, recall, and many more (Kraepelin, 1892a, vii-viii). Concerned with how the mercurial nature of mental processes might interfere with his results, Kraepelin had intended for these to be "Stoffe von so energischer Wirkung, dass dieser letzteren gegenüber alle anderweitigen Ursachen mehr in den Hintergrund treten mussten" (Kraepelin, 1892a, 3).[135] This was not merely research on the effects of various medicines—it was the self-conscious study of the intoxication of the "Seelenleben" of embodied consciousness (Kraepelin, 1892a, 3).

Alcohol, Chloral, Morphine, Paraldehyde, Ether, and Amyl Nitrate

Kraepelin's earlier work on the effects of alcohol have been discussed at some length, and Kraepelin reviews his earlier work in *Ueber die Beeinflussung.* A new addition from his

[135] "Substances of such an energetic effect that [concerns about interference caused by normal variation] had to take a back seat to all other causes".

unpublished research from the period between 1881-1883 are his association studies on alcohol, which used the experimental method for measuring simple reaction time but in response to sound association (Kraepelin, 1892a, 51).

When it came to testing the effects of alcohol using the post-1883 methods, Kraepelin had a far greater number of test subjects to pick from. Apart from himself, Kraepelin identified Denio, "Herren Dannenberg (Da.), Hahn (Ha.), Heerwagen (He.), Michelson (M.) und Oehrn (O.)" as respondents (Kraepelin, 1892a, 68). This may have been to his detriment, however—since it exaggerated individual differences that were less visible in a smaller cohort and would have been diffused in a larger study. Subjects were given either ~20 or ~30gr. of pure alcohol, depending on when the trial was conducted. The experiments in question took place between December 1888 and December 1889 and amounted to a total of 27 runs: 7 on reading, 10 on arithmetic, and 10 on memorization of figures (Kraepelin, 1892a, 68-69).[136]

In the trials that saw subjects work on adding strings of numbers, alcohol elicited a generalized decrease in work performance, which was immediately seen after consumption (Kraepelin, 1892a, 72-73). The secondary effect on the adding was an increased variability in the rate with which the work could be completed (Kraepelin, 1892a, 74). Kraepelin found that the task that required subjects to memorize sets of twelve digits varied greatly between individual subjects. In the first quarter of an hour, there was an overall decrease in their subject's abilities to complete the task, though some subjects initially performed the task better than they had prior to consuming alcohol (Kraepelin, 1892a, 76). Nevertheless, the consistent pattern found in nearly all cases was an initial drop in performance followed by a consistent improvement toward

[136] See Figure 8 for a panel of Kraepelin's graphs comparing reaction times with different intoxicants, including separate measurements for distinguishing divergent processes.

baseline starting 30 minutes after ingestion (Kraepelin, 1892a, 77). Kraepelin emphasized that any initial increase in work performance was certainly a fleeting phenomenon, though he personally found that it was consistent with subjective experience (Kraepelin, 1892a, 77). He related this to his findings in the low dose simple reaction trials, where he also found an initial improvement in performance. On this point, Kraepelin offers the same possible explanation: those subjects who initially performed better possibly had some increased resistance to alcohol's effects, whether by natural constitution or familiarity with the substance (Kraepelin, 1892a, 77-79). Another factor of that Kraepelin was constantly aware of were differences between individual learning methods, which likely accounted for some degree of variation between individuals but not enough to alter the trend (Kraepelin, 1892a, 86).

Reading times, too, were generally slowed under the influence of alcohol, with at least one case of an initial acceleration (Kraepelin, 1892a, 87-88). On these grounds, Kraepelin suggested that under certain conditions alcohol caused an acceleration of speech. Though, Kraepelin also noted this only occurred once in trials using a 30gr. dose, which appeared consistent with his findings elsewhere about the stimulating effects of lower doses of alcohol (Kraepelin, 1892a, 88). Reading did seem more sensitive to this initial acceleration effect, but also appeared to function at a deficit for a longer period of time as well (Kraepelin, 1892a, 88). He did further tests on muscular flex response using a dynamometer, as well as alcohol's effect on the prediction of time intervals, where alcohol's effects were consistent with the other tests (Kraepelin, 1892a, 91, 99, 105).

For Kraepelin's trials using paraldehyde, he ran sets of word and association reactions, using a hand operated contact key (Kraepelin, 1892a, 148-149). Of the 14 observations, 7 were on Kraepelin himself, 4 on Lehmann, and 3 on Ernst Rehm (Kraepelin, 1892a, 149). Contrary to

Kraepelin's expectations that paraldehyde would merely lengthen reaction times, paraldehyde produced an array of seemingly paradoxical results (Kraepelin, 1892a, 149-150). Instead, Kraepelin observed fluctuations between increases and decreases in response times (Kraepelin, 1892a, 150). This behaviour appeared to scale with dosage, showing the highest magnitude in fluctuation at 5 gr. doses and lower variation at 2 gr. (Kraepelin, 1892a, 150). Kraepelin proposed that these were not merely irregularities: there was a recognizable acceleration of some psychic processes. The onset of this acceleration effect was recognizable starting ~5 minutes after ingestion and peaked at the 23-27 minute mark (Kraepelin, 1892a, 150). Combined with the negative results, Kraepelin realized that the shortened intervals were the result of a tendency towards premature reaction under the influence of paraldehyde (Kraepelin, 1892a, 149-152). An explanation proposed by Kraepelin is the appearance that paraldehyde elicited an initial elongation in reactions, to which the subject then unconsciously attempts to compensate for (Kraepelin, 1892a, 152). Where word reactions were used in place of discrimination reactions, paraldehyde appeared to have similar effects as other reactions but on an enlarged scale (Kraepelin, 1892a, 153).

These findings suggested to Kraepelin that paraldehyde had a strong stimulatory effect on the will impulse, apparent not only from the subjective feeling of a quicker reaction, but the frequency with which premature reactions were registered in the experiments (Kraepelin, 1892a, 155). Simultaneously, there was a marked lengthening in times when premature reactions did not occur. This combination of prolonged reactions with a tendency toward premature responses suggested to Kraepelin that paraldehyde had a two-fold effect: at once, unfettering the will process and suppressing psychical apprehension (Auffasung) (Kraepelin, 1892a, 157-159).

Kraepelin had previously done a limited series on the effects of chloral hydrate, just two sets of experiments. On these two occasions, he gave himself a dose of 2 gr. chloral, and tested simple and choice reaction times (Kraepelin, 1892a, 161). Simple time slowed very rapidly after ingestion but the resultant lengthening was relatively small, seeing a return to baseline after ~30 mins (Kraepelin, 1892a, 161). Subjectively, Kraepelin experienced that chloral took a long time to induce feelings of fatigue, which was more apparent in the results from the choice reactions where it approximately 6-14 minutes to show an effect (Kraepelin, 1892a, 162). The response was a visible slowing of the election process, an elongation of far greater magnitude than was seen in simple reactions. Notably, there was no shortening or premature reactions, as had been seen in paraldehyde (Kraepelin, 1892a, 162). Kraepelin also conducted two additional sets of trials with chloral closer to 1892, now using 1 gr. of chloral during sets of choice and word reactions (Kraepelin, 1892a, 164). Both experiments saw significant lengthening in reactions, with results that were fundamentally similar to the earlier chloral trials (Kraepelin, 1892a, 165).

Morphine, along with tea, exerted what Kraepelin felt was one of the most distinct affectations of the psychological process, and he considered it to be of the highest practical and theoretical interest (Kraepelin, 1892a, 166). However, Kraepelin was also extremely wary of inducing morphinism in any of his subjects, so he limited trials to himself (Kraepelin, 1892a, 167). In the 1881-1883 period, Kraepelin did two sets of experiments with morphine, both of which were choice reactions conducted under the influence of a 0.01 gr. subcutaneous injection of morphine muriaticum (Kraepelin, 1892a, 167). What Kraepelin measured was a gradually increasing moderate elongation in reactions, which reached its highest value after 80 minutes (Kraepelin, 1892a, 167). In the second set of experiments, there was a shortening below baseline in the first half hour that was too consistent to be a mere fluctuation (Kraepelin, 1892a, 167-168).

Kraepelin conducted two additional sets of experiments on morphine, this time taking 0.01 gr. in an oral solution prior to conducting trials on choice and word reactions (Kraepelin, 1892a, 168). Immediately after ingestion Kraepelin registered a moderate prolongation in choice reactions which peaked at the 35-40 minute mark (Kraepelin, 1892a, 168). The word reactions produced a completely different result. Here, Kraepelin observed a sudden shortening in reactions which reached peak magnitude below baseline levels at 30-35 minutes and then slowly returned to normal levels (Kraepelin, 1892a, 168). Both responses were so distinct, and relied on so many individual measurements, that Kraepelin had absolute confidence in the validity of these observations (Kraepelin, 1892a, 168-169). Kraepelin proposed that this twofold effect of morphine was likely because it facilitates the perception of external impressions, while choice acts are made more difficult. Kraepelin also repeated his earlier work on amyl nitrate and ether and found that the observations in his 1883 paper remained reliable (Kraepelin, 1892a, 170-171).

Intoxicated Psychosis

While created only through great effort and with considerable assistance, it is difficult to deny that Kraepelin's 1892 *Ueber die Beeinflussung* remained consistent with the objectives laid out in his initial letter to Wundt, back when he was proposing a habilitation project.[137] Further

[137] The reference to effort involved in the creation of the text is a paraphrase of Kraepelin's own words in the text's introduction. Here, Kraepelin relays the tremendous difficulty involved in not only developing controlled experiments, but in making the many individual observations and measurements. Given that Kraepelin conducted most of the measurements on himself, this is inescapably also in reference to all of the instances in which Kraepelin drugged himself in different ways, particularly with soporifics, which was surely "exhausting."

still, Kraepelin appears hopeful about the value of his research, as he says:

> Ein kurzer Rückblick auf den ganzen, bis hierher von uns zurück gelegten Weg lässt, wie ich meine, erkennen, dass wir durch die hier angewandten Methoden in den Stand gesetzt sind, diejenigen Veränderungen in unserem Seelenleben, die wir sonst nur durch das trügerische Hülfsmittel der Selbstbeobachtung in ganz allgemeinen Umrissen zu schildern vermögen, nunmehr in bestimmten Zahlenwerthen auszudrücken und auf gewisse sehr einfache Elementarstörungen zurückzuführen (Kraepelin, 1892a, 227).[138]

Kraepelin's psychological research on the effects of intoxicants on the mind had not only developed a method of experimentally encountering "einfache Elementarstörungen" but had also established a more or less fixed relationship between constellations of symptoms and the disorder of certain mental processes. Each and every substance has a discrete effect on mental life, expressed through the combination of various affectations of basic mental processes (Kraepelin, 1892a, 228). This was sufficiently predictable for Kraepelin to speculate about the action of taking various drugs at once. For example, Kraepelin suggested these results demonstrated why alcohol helps with morphine abstinence, while also exhibiting how the underlying psychological character of alcohol and morphine euphoria are different (Kraepelin, 1892a, 226). With regard to mental processes, this research brought Kraepelin to the realization that motor disturbance were almost always accompanied by sensory and intellectual disturbances, while sensory and intellectual disturbances accompany one another so closely that they appear inseparable (Kraepelin, 1892a, 228).

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[138] A brief review of the whole path we have travelled up to this point shows, as I believe, that through the methods applied here we are in a position to express in definite numerical values those changes in our soul-life which we are otherwise only able to describe in very general outlines through the deceptive aid of introspection, and to now trace them back with certainty to very simple elementary disturbances.

Given the scope of the maturation of Kraepelin's research into the effects of intoxicants on the mind relative to the period surrounding his habilitation and the first *Compendium,* it is worth considering whether these new developments correspond with the changes Kraepelin made to further editions of his textbook. The second edition of the *Compendium,* now called *Psychiatrie: Ein kurzes Lehrbuch für Studirende und Aerzte,* was published in 1887 and can be distinguished from the first edition by the expectation that symptom complexes will converge on discernible patterns (Heckers and Kendler, 2020, 382-383; Hoff, 2015, 36). The 1887 edition also immediately followed Dehio and Kraepelin's research on caffeine (Dehio, 1887; Steinberg and Müller, 2005, 140-141). There, Dehio suggested that in caffeine they had found some degree of confirmation for Kraepelin's initial hypothesis that different intoxicant affected different psychic processes in different ways, especially as Kraepelin's 1883 paper focused on substances with roughly similar effects (Dehio, 1887, 48-49). This hopeful albeit ambiguous position is consistent with the changes that Kraepelin made in 1887 with respect to the suggestion that symptom complexes would likely converge on a pattern, much as the caffeine trials had tentatively associated specific psychic disturbances with particular substances.

The more remarkable shift came with the 1893 edition, which, once again, shortly follows Kraepelin's 1892 *Ueber die Beeinflussung.* The major theoretical development of *Ueber die Beeinflussung*, as stated by Kraepelin, was the realization that distinct forms of mental disturbance, as those triggered by intoxicants, could be associated with the affectation of specific psychological processes. This was a shift from the position seen in 1883 where the finite distinctions between substances of intoxication were so poorly defined that low and high doses functionally operated like independent symptom complexes. By 1892, the associations made with certain forms of intoxication had been more or less clarified and the behaviour of particular substances at higher or lower doses were given a consistent definition.

It is this subtle, yet precise, theoretical shift that one finds reflected in the nosological concept underlying the 1893 edition of *Psychiatrie.* Here, Kraepelin advocated for the classification of mental disturbances on the basis of long term, careful observation of the entire course of a given mental disturbance, which could then be grouped on the basis of these broader similarities (Kraepelin, 1893, 242-243; Heckers and Kendler, 2020, 383). Crucially, there is the impression that real illnesses could emerge through this method of classification, something more rigid than mere symptom complexes. He also introduced the diagnostic category “dementia praecox,” which would in time become schizophrenia (Decker, 2007, 339). This is nothing if not a perfect mirroring of the theoretical development seen in Kraepelin’s research on intoxicants, where earlier ambiguity surrounding symptomatology had been overcome in the recognition that patterns of mental disturbance could be understood as the observable affectation of specific psychic processes. Methodologically, many of these ambiguities, for example those pertaining to alcohol, were ultimately explained through the use of a wider array of subjects and greater breadth in experimentation, just as Kraepelin had proposed in the 1893 edition of *Psychiatrie.* By the 1896 edition of *Psychiatrie*, the classification scheme proposed in 1893 would concretize into the basis of what would become his psychiatric nosology, already giving rise to the concept of unitary psychiatric diseases (Heckers and Kendler, 2020, 383). Thus, it seems conceivable that much of the foundation of Kraepelin’s psychiatric nosology was not derived from the few clinically focused moments in Dorpat and his first years at Heidelberg and instead largely drew on his hard-won experimental research on the effects of intoxicants on the mind.

Physiologie der Leib und Seele

It follows that just as Kraepelin increasingly saw mental disturbances as inducible by the use of substances of intoxication that his conception of mental illness, and thus of the mind, was increasingly understood in biological terms. As discussed earlier, Kraepelin's 1883 research on intoxicants demonstrated that even early on Kraepelin diverged from Wundtian parallelism to propose a more dynamic conception of the body and mind. But it was here, in the late 1880s and 1890s, that this notion matured into a strictly biological conception of the subject, emergent of what Kraepelin referred to in "Psychologische Forschungsmethoden" (1888) as a "Physiologie der Seele" (Kraepelin, 1888, 13).

In the first *Compendium,* Kraepelin left no secret concerning his disregard for the possible benefit of neurophysiology/-anatomy in the development of diagnostic criteria. This position was of course a reflection of Kraepelin's more general disagreement with those neurophysiologists who sought to reduce mental states to neural states, through the lens of the neo-mechanistic body. Kraepelin's influence on his students has even been identified as the origin of the famous accusations of "brain mythology" against the likes of Meynert, Wernicke, Flechsig, and so on, with Kraepelin's successor in Munich, Franz Nissl, first applying the term to Flechsig (Hlade, 2021, 4).[139] In this sense, Kraepelin remained steadfast in his opposition to the brain-psychiatrists, a position that his students happily carried forward well into the 20th century (Hlade, 2021, 4-5). Yet, as has already been established, this did not mean that Kraepelin truly opposed a unified physical conception of the mind.

Kraepelin, himself, seems to have upheld his belief that mental disturbances were ultimately somatic illnesses (Engstrom, 2015, 156). The error of the neurophysiologists, brain-

[139] As discussed earlier, this was also the origin of Karl Jasper's use of the term.

psychiatrists, and neo-mechanists had been to derive clinical theory from anatomical research, where Kraepelin saw no possibility of dissecting the complexities of the human mind by way of anatomical research alone (Engstrom, 2015, 155-156). As discussed, it had been an *a priori* recognition of the body as the locus of the real, of thinking about the mind via the body, that had led others toward neuropathology in the first place. The issue for Kraepelin had been that there was no place in these perspectives for the dynamic complexity of the psyche. A totalizing biologistic conception of the mind and body was tenable if it was inclusive of perceptional diversity of mental life.

To this point, Kraepelin had taken up the methodologies of scientific psychology to bring the testimonies of substances of intoxication to bear on the physical character of the mind, and body. Kraepelin had used intoxication to think about the mind with the body, rather than describe the mind through the body. This had allowed him to understand and uphold the mind in all of the dynamic vigour of embodied perceptional existence while simultaneously grounding it in the physical. It was, as previously stated, the physical made real in the psychical. It was only in conjunction with an approach to the mind grounded in experimental psychology and the clinical encounter that neuropathology had its place, and there is evidence of this in Kraepelin's work.

Though the 1883 *Compendium* was firm in its position on the questionable value of neuropathology for psychiatry, this attitude softened in subsequent editions. The 1887 edition of *Psychiatrie* saw Kraepelin advocating for the importance of including pathological research in the development of psychiatric classifications, particularly the postmortem findings of patients with similar symptom complexes or clinical pictures (Kraepelin, 1887, 211; Heckers and Kendler, 2020, 383). Into the late 1890s, Kraepelin had continued his research on the effects of intoxicants on the mind, with several of his students publishing on their work. There, Kraepelin reports of witnessing “Nissls schöne Tierversuche mit subakuter maximaler Vergiftung, bei denen sich mit überraschender Deutlichkeit die ganz verschiedenartige Beeinflussung der Nervenzellen durch die einzelnen Gifte verfolgen ließ” (Kraepelin, 1983, 121).[140] To further emphasize this point, the 1896 edition of *Psychiatrie* introduced a category of metabolically-induced mental disorder understood as “auto-intoxication” (Heckers and Kendler, 2020, 383). States of intoxication not only provided an experimental foundation for the development of Kraepelin’s psychiatric nosology but also served as the bridge between the psychical and the physical.

This was the physical made real in the psychical. Kraepelin’s physiology of the soul was not in contrast, or even separate from, the physiology of the body. They ultimately became the shared foundations of a holistic conception of the body and mind, one which was inclusive of the psychical experience of embodiment and of the body as object. Where the neo-mechanists encountered the body within a framework predicated on the *a priori* exclusion of the psychical, the mind, the psyche, remained an anomaly. Where strict parallelism undermined the potential for closeness between the psychical and the physical, the body, the vehicle of lived activity, remained an anomaly. By upholding distinct scientific encounters with the body and the mind as the basis for any reckoning of mental life and deriving it from the experimental testimony of intoxicating substances, Kraepelin shaped what is now meant by biologism. This was the emergence of the biological subject, a being for whom the sum of physical and mental existence was understood as bio-chemical in origin, and yet the study of said being was irreducible to physiology.

[140] “Nissl's beautiful animal experiments with subacute maximum poisoning, in which it was possible to follow with surprising clarity the quite different influence of the individual poisons on the nerve cells.”

The biological subject turns out to have been an intoxicated subject. And it was all emergent of the emotional experience of intoxication as a way of knowing, itself taken as the object of the experimental encounter. Though Kraepelin's work would inspire a number of his students to continue research in the effects of intoxicants around the world, the impact of Kraepelin's intoxicated studies on the mind was not limited to the scientific community. Arguably, any notion of a cohesive biologism, one which participates in the dominant conceptual *a prioris* underlying an array of subjectivities, can expectedly be found in other discursive communities, as far as they are concerned with the body and mind. To this point, it is important to also look to the emerging body philosophies of the period. For Nietzsche, Freud, and Weber, thinkers whose work would leave indelible marks on our sense of the modern, it would be not only the work of Wundt andKraepelin which would shape their conceptions of the body, but intoxicants themselves. In the mingling of the scientific encounter with intoxicants with their own intoxicated lives, each would, in their own way, argue for a foundational conception of the biological subject.

Chapter 11—Drunken Songs of Tomorrow: Nietzsche, Freud, Weber and Intoxication

Toward a trans-psychological biology of the subject

There is an illusory gap between the world of philosophy and that of the sciences. Such perceptions somehow even persist when it comes to the formations of subjectivities, including the biological subject. There is no denying that physiological and bio-medical research directly intercedes in one's conception of what it means to be. Medicine, institutional policies, civil health ordinances, and economic regulatory practices, just to name a few, are all constantly negotiated, and renegotiated, at the sites where they cross into our lives—they all help make us up as subjects. This diversity of inputs, given expression through the radical particularity of place and time, entails a certain expectation that conceptual transmission will almost always be partial, fractured, and distorted. Yet, there is a peculiar degree of conceptual cohesion surrounding the emergence of the biological subject toward the end of the nineteenth century, one which persists to this day. As has hopefully been made clear, this cohesiveness is irreducible to any presumed facticity of being-in-a-body. It could, however, be expected that such cohesiveness would entail commonality between seemingly conflicting points of view, that the horizon of possible questions that could be asked *à propos* the body and mind, both within and without the sciences, had radically shifted.

The answer to this query can be found by turning to the work to those late-19th century social theorists and philosophers who abandoned spirit in favour of a purely biological understanding of the human being. One need look no further than some of the period's most influential personalities: Friedrich Nietzsche, Sigmund Freud, and Max Weber. Peering into not only their work but their lives, a common thread emerges. Drunkenness, narcosis, and inebriation pervade not only their work but their lives. Further still, for each of them intoxication figured centrally in their lives at the critical junctures of their interface with the emerging experimental interest in substances of intoxication, notably that of Kraepelin. For Nietzsche, this would culminate in a bio-psychiatric philosophy of intoxication. For Freud, his cocaine-charmed life would mark his only real turn to experimental psychology, followed ultimately by the emergence of psychoanalysis. And, for Weber, his personal struggles with intoxication would translate into a fetishization of sobriety, one which would figure centrally within his conceptions of modernity, his scientific epistemology, and his notion of the body. Each would, in turn, find their own language to give voice to a dynamic biologistic perspective which was both consummately organic and yet inclusive of a distinct, scientific concept of embodiment.

Nietzsche: Psychiatrist of the Will

The scholarship surrounding the potential sources of Nietzsche's ideas, even his possible influences, owes a great debt to his efforts to obfuscate their origins. Even in his central published works, Nietzsche writes for a learned reader. He constantly levels highly focused critiques against the works of individuals who he rarely, if ever, names, instead leaving it to the reader to be versed well-enough in what Nietzsche himself had read. It is perhaps a great irony then that today Nietzsche is mostly wasted on the youth. Even after his vast personal library is catalogued, the contents of his letters and journals are considered, and records of his lifetime borrows from the library are queried, what we're left with is still a picture of negative space. That is, the blank space left by the impression of what Nietzsche would seem to have read, and yet lacks direct evidence. This dilemma is the product of both the simple reality that people do not generally leave a record of everything they have read and Nietzsche's own self-conscious efforts at self-mythologizing. Nietzsche obfuscates his origins and, where an association of his is well-known, he misrepresents the nature of other's influence. A very obvious example of this is his friendship with Paul Rée, a person with whom Nietzsche shared a profound personal and intellectual closeness until their falling out, after which Nietzsche overcompensated in attesting to the complete lack of influence that Rée ever had on Nietzsche's thought (Holub, 2015, 157-160).

One domain of great interest concerning Nietzsche's philosophy, and about which considerable effort has been made to understand its influence, is physiology. Earlier on, Nietzsche began to advocate for some kind of material doctrine of the body. Some have suggested this was emergent of Nietzsche's lifelong poor health, plagued by a condition that would leave him periodically blinded and near constant indigestion, Nietzsche's awareness of his body was repeatedly conditioned through illness (Dahlkvist, 2014, 138). Nietzsche was thus primed to have an interest in both medicine and physiology, not only in order to understand these subjects but in a sense to make use of them himself (Dahlkvist, 2014, 138).

Another factor was his friendship with Paul Rée. Nietzsche and Rée met in May 1873 and became close friends by 1876, at the latest. It was Rée passion for the natural sciences, and especially the life sciences, that inspired Nietzsche to broaden his reading in the late 1870s and early 1880s. Prior to their friendship, Nietzsche did have a more than passing interface with serious scientific publications. An important starting point was Nietzsche's first encounter with Friedrich Lange's *Geschichte der Materialismus* in 1866, which was not long after his introduction to the works of Schopenhauer (Brobjer, 2017, 26). After reading Lange, Nietzsche produced a science reading list in 1868 which included Helmholtz's *Über die Erhaltung der Kraft,* Wundt's *Über die Menschen- und Thierseele*, Lotze's *Streitschriften,* and Oken's *Zeugung,* among others (Brobjer, 2017, 28). It is not clear which books he ultimately got around to reading. In 1872-1873, Nietzsche became acquainted with Zöllner's *Über die Natur der Kometen: Beiträge zur Geschichte und Theorie der Erkenntnis,* a text which would have a tremendous impact on Nietzsche's thinking (Brobjer, 2017, 32). As early as 1874, Nietzsche made a passing reference to du Bois-Reymond in *Schopenhauer als Erzieher*, and at least two more references are found in the *Nachlaß* from that spring/summer (Nietzsche, NF-1874, 35[12]). It was around this time that Nietzsche may have come to own du Bois-Reymond's 1874 *Über Geschichte der Wissenschaft* (Campioni et al, 2003, 199). In 1875, Nietzsche purchased and read Julius Bernstein's *Die fünf Sinne des Menschens* (Campioni et al., 2003, 141). *Unsere Körperform und das physiologische Problem ihrer Entstehung* by Wilhelm His was purchased in 1876 (Campioni et al, 2003, 299).

1876 marks the period when Nietzsche personally divested his thought from the influences of Schopenhauer and Wagner, which Brobjer argues was at least partially due to the influence of some of the books listed above (Brobjer, 2017, 34). 1876 was also the beginning of Paul Rée and Nietzsche's close collaboration. Rée had studied philosophy at Leipzig, and had even had the opportunity to study science in Berlin (Holub, 2015, 157). Although he would not become a physician until later in his life, it appears Rée was always deeply influenced by scientific research and especially physiology, medicine, and Darwinism. That Rée's scientific proclivities would leave an impression on Nietzsche is all but a foregone conclusion. The two spent a winter together in Sorrento, and all the surviving documentary evidence paints the clear image of two men who partook in an extremely free exchange of ideas (Holub, 2015, 157). For a while, it appears that Nietzsche was even something of a follower of Rée, both in his scientific and historio-psychological interests (Holub, 2015, 157).

Under the influence of Rée, Nietzsche himself relays that in the period between 1876-1883 he read almost nothing but books of scientific interest (Brobjer, 2017, 35). Among many others, Nietzsche acquired Michael Foster's James Johnston's *The Chemistry of Common Life*, Foster's *A Text Book of Physiology*, Bain's *Mind and Body: The Theories of Their Relation* and Adolf Fick's Ursache und Wirkung, as well various scientific periodicals (Brobjer, 2017, 37, 39). Thus, when Nietzsche wrote "Was ist ein Wort? Die Abbildung eines Nervenreizes in Lauten," or equated bodily activity with intellectual virulence, this was not rooted in a vulgar materialism but rather a relatively learned awareness of the current scientific literature (Nietzsche, 1873 [1975], WL-1, § 1.; Nietzsche, 1881 [1975], M-18, §18.).[141,142] Nietzsche was arguably even a positivistic materialist during the period between 1873 and 1883, jotting down in a note in 1876: "Positivismus ganz nothwendig" (Nietzsche, 1876 [1975], NF-1876, 20[19]).[143] Even as Nietzsche himself grew critical of positivistic approaches and developed a broadly sceptical attitude, Nietzsche's rejection of metaphysical notions and his focus on the somatic character of mental phenomena remained clear.

An additional, and perhaps more crucial, influence exacted during Nietzsche's friendship with Rée was that of psychology. Perhaps more than anything else, Nietzsche understood his philosophical work to be psychological in nature, at times lamenting that he was still not popularly referred to as a psychologist (Golomb, 2015, 1). After first identifying himself as a

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[141] "What is a word? The representation of a nerve stimulus in sounds."

[142] All citations for Nietzsche refer to the digitized version of the standard critical edition of Nietzsche's complete works. As such, all citations are formatted thus: (author, year of publication [year of digitization], reference abbreviation in KGWB, page number).

[143] "Positivism [is] totally necessary."

psychologist as early as 1878, Nietzsche would sporadically renew the label throughout the remainder of his life (Nietzsche, 1878, BVN-1878 [1975], 762). He even subtitled his final publication "Aktenstücke eines Psychologen" ("From the Case Files of a Psychologist") (Nietzsche, 1889 [1975], NW-Titelblatt). The question is in what sense did Nietzsche understand himself to be a psychologist and how did he figure his approach in to the psychologies that were blooming in the late 1870s/early 1880s?

Here too, Paul Rée was a formative influence. Nietzsche had been exposed to Rée's moral psychology as early as 1875, when Nietzsche read Rée's 1875 *Psychologische Beobachtungen* (Holub, 2015, 168). Two years later in 1877, Rée debuted *Der Ursprung der moralischen Empfindungen,* a psychological genealogy of the moralistic sense. Though these were precisely the subjects that would make Nietzsche famous, it appears these interests originated in Nietzsche and Rée's interactions.

Nietzsche's psycho-philosophical interests in a broader sense do, however, predate his close friendship with Rée. As early as "Über Wahrheit und Lüge im außermoralischen Sinn" (1873), Nietzsche expanded on Kant's criticial work in arguing that any perceived categories of conscious themselves are mere leaps of metaphor. "True" knowledge of self and the world could not be independently verifiable, and yet we encounter the world knowingly. Metaphysical frameworks, speculative systems, transcendent principles—these were valuable not on account of their epistemic durability, but because of the mythical purposes they served for individuals and for society (Nietzsche, 1873 [1975], WL-1, § 1.; Nietzsche, 1874 [1975], HL-Vorwort-1.). Nietzsche's understanding of the psychological implications of this argument were clearly articulated in "Vom Nutzen und Nachtheil der Historie für das Leben," where the shared cultural imaginary, given expression through a culture's historical sense, could function either as a force for stagnation or for excitation (Nietzsche, 1874 [1975], HL-1).

Thus, it already becomes clear what Nietzsche intends when, shortly after becoming close friends with Paul Rée, Nietzsche begins to write about the need for a "new psychology" (Golomb, 2015, 1; Liebscher, 2014, 362). It would be psychology, Nietzsche suggests, that would provide the instruments with which to diagnose and understand the great crises of his age. However, Nietzsche scarcely makes explicit how he would methodologically ground such a psychology. A strong hint can be found in Nietzsche's discussion of experimentation in *Morgenröte: Gedanken über die moralischen Vorurteile.* Here, the principle of experimentalism is identified with the generation of new possibilities, futures, and ways of being (Bamford, 2016, 13). Nietzsche muses about experimentation not only in a laboratory, but on the social, moral, and perspectival structure of ourselves and our societies, declaring that "Wir sind Experimente" (Nietzsche, 1881 [1975], M-453).[144] Sharply contrasting philosophies of compassion and security, Nietzsche identifies experimentation with risk, potentiality, and new beginnings (Bamford, 2016, 13-14). Though any experimentation can only ever be preliminary for Nietzsche, it is clear that experimenting *psychologically,* in concert with experimentation in the natural sciences, is a significant way in which the Nietzsche's new psychology would function.

Of course, what Nietzsche here refers to as experimentation falls outside of the typical understanding within the purview of the physical sciences, and certainly Nietzsche's psycho-physically minded contemporaries. In this sense, Nietzsche's words here betray the influence of scientifically-minded psychological-philosophers, notable Zöllner, Lange, and Hartmann (Lehrer, 2015, 182). Yet, given the respect, even admiration, Nietzsche expresses for the natural

[144] "We are experiments."

sciences and his self-identification with psychology, it is worth nonetheless considering the possible influences of scientific psychology in the tradition of Wundt and Kraepelin.

As with many of Nietzsche's influences, Nietzsche never explicitly names the likes of Wundt, nor his followers, in his published works. Though there is fairly substantial evidence that Nietzsche had more than a passing interest in Wundtian thinking. To begin with, the works of Eduard Hartmann are found in Nietzsche's personal library, and there Nietzsche would have been exposed to Wundt's ideas in some detail (Campioni et al., 2003, 276; Lehrer, 2015, 182). As mentioned before, Nietzsche also listed one of Wundt's works in his earlier attempt at a scientific reading list. Then there are the contents of Nietzsche's letters and survings papers. In the summer of 1877, Nietzsche wrote to Rée with some urgency about his efforts to arouse the editor of *Mind*'s interest in their work, very casually remarking that an essay by Wundt was being published there (Nietzsche, 1877 [1975], BVN-1877, 643). On a list written in 1879, Nietzsche simply writes "Wundt 'Aberglaube in der Wissenschaft,'" without further context (Nietzsche, 1879 [1975], NF-1879,47[15]). Paul Rée also wrote to Nietzsche about Wundt in 1881. Taken together, it is difficult to deny that Nietzsche had a sustained awareness of Wundt's publications as far back as the late 1860s, and that this sustained awareness translated into discussions with Rée until at least the early1880s. The *pièce de résistance* though is a letter sent by Nietzsche to his publicist, Constatin Georg Naumann, on November 8, 1887, requesting that free copies of his upcoming book, *Zur Genealogie der Moral*, be sent to "Herrn Professor Dr. Wundt, Leipzig[;][…] Herrn Professor Dr. Leuckart, Leipzig[;][…] Dem Geheimrath Prof. Dr Helmholtz[;][…][and] Herrn Professor Dr. Du Bois-Reymond Berlin," among others (Nietzsche, 1887 [1975], BVN-1887, 946). This extends the timeline of Nietzsche's interest in Wundt until the final year of his conscious life, while affirming his sustained interest in physiology.

The evidence overwhelmingly suggests that Nietzsche had an awareness of Wundt's work and even had aspirations of capturing the interests of the experimental psychologist. But there is further still evidence of not only Wundt's influence on Nietzsche, and perhaps Nietzsche's influence on Wundt. This is particularly clear in Nietzsche's notion of will, the beating heart of Nietzsche's mature psychological-philosophy. It is first in *Jenseits von Gut und Böse* that Nietzsche introduces his fully developed notion of the *Wille zur Macht.* Here, Nietzsche outlines his conception of will in contrast to a dualistic or spiritual idea, instead identifying not a single will but a ceaseless combat of unseen wills bubbling beneath the surface of all psychological processes (Cowan, 2005, 48). In this sense, the *Wille zur Macht* referred not to a particular will but to the process of conflicting wills behind all organic life. Crucial to this idea is both the non-causal nature of Nietzsche's will and the opacity of the willing-process relative to human awareness. As Nietzsche poetically mused in *Also Sprach Zarathustra,* "[w]enn ich diesen Baum da mit meinen Händen schütteln wollte, ich würde es nicht vermögen[;] [a]ber der Wind, den wir nicht sehen, der quält und biegt ihn, wohin er will" (Nietzsche, 1883 [1975], Za-I-Baum). [145]

Nietzsche's idea of the will as diffused throughout every facet of embodied experience, rather than an explicit faculty or volitional force, draws direct parallels with the idea of will found in Wundt's *Grundzüge.* In the 1874 *Grundzüge,* Wundt had already described "all organic and psychic functions as more or less complex expressions of a basic form of 'will' at the origin of all physiological and psychic activity" (Cowan, 2005, 50). All psychical and physical actions, both intended and unintended, are "modalities" of the willing process (Cowan, 2005, 51).

[145] "If I wanted to shake this tree with my hands, I would not be able to do it; but the wind, which we do not see, twists and bends it wherever it wants."

Whereas instinctual reactions were acts of certain sets of wills, decisions, such as those tested in choice reactions, involved a more sophisticated "Kampf solcher widerstreitender Motive," a combat of wills (Wundt, 1909, 225; Cowan, 2005, 51).[146] Although Nietzsche never identifies Wundt as an influence, the evidence establishing Nietzsche's awareness of, and interest in, Wundt, as well as the clear parallels between Nietzsche's psychological model and Wundt's leaves little room to deny the possibility of influence. This is also true of Nietzsche's interest in experiment as a central component of his concept of a new psychology, one which was foundational to what Nietzsche even referred to in *Jenseits* as a "Physio-Psychologie" (Nietzsche, 1886 [1975], JGB-23).

Having established the likelihood of a Wundtian influence, it becomes easier to question whether Kraepelin, in particular his work on intoxication, figured into Nietzsche's conception of the subject as well. Kraepelin's work, including "Ueber die Einwirkung einiger medicamentöser Stoffe auf die Dauer einfacher psychicher Vorgänge," was published in Wundt's journal, *Philosophische Studien,* alongside many of Wundt's own works, where Nietzsche could access them. Further still, Kraepelin was a serious contributor to Wundt's foundational research program of reaction time. Those interested in Wundt's early experimental work almost inevitable crossed pathes with Kraepelin's experimental encounters with intoxicants as pathological states. Even outside of Wundt-adjacent publications, Kraepelin interacted with texts and individuals with which Nietzsche had a strong association. In 1885 and then in 1886, Kraepelin wrote reviews of Paul Rée's *Die Entstehung des Gewissens* and then *Die Illusion der Willensfreiheit: Ihre Ursachen und ihre Folgen*, in the *Literarisches Centralblatt* (Kraepelin, 1885, 1697-1698; Kraepelin, 1886, 41). However, for the most part, possible influences have to be assessed on the

[146] […] "struggle of conflicting motives."

basis of changes in Nietzsche's thought, especially in the symbolic and polemic function of intoxication, and intoxicants.

In opio veritas

Intoxication had been a focal point of Nietzsche's symbolic lexicon as early as *Die Geburt der Tragödie aus dem Geiste der Musik,* where the constrictive, logocentric impulse of the Apollinian was contrasted with the boundless, dynamic creativity of the Dionysian. The scope of the available literature on Nietzsche's Dionysus as both signifier and signified is nigh boundless. It is a dominant fixture of Nietzsche's corpus. But there is a clear shift in how Nietzsche takes up intoxication, and intoxicants, in his symbolic language as he moves from the so-called middle period into his mature philosophy.

By the writing of the *Fröhliche Wissenschaft* (1882)*,* intoxication plays a central, albeit still symbolic, role in Nietzsche's thought (Ciaccio, 2018, 119). As Nietzsche wrote of the failings of modern theatre, "Die stärksten Gedanken und Leidenschaften vor Denen, welche des Denkens und der Leidenschaft nicht fähig sind — aber des Rausches […] [u]nd Theater und Musik das Haschisch-Rauchen und Betel-Kauen der Europäer" (Nietzsche, 1882 [1975], FW-86).[147] This had been the Dionysian principle at play behind Nietzsche's temporary infatuation with Wagner—music, art, the aesthetic experience, had long been the premier European narcotic. Narcosis and stupefaction are expressive of the deleterious potentials of excesses in a given form of intoxication, while intoxication more generally is simultaneously the much needed antidote to

[147] In theatre, "the strongest thoughts and passions are made available to those who are incapable of thought and passion - but of intoxication [...] [and] theatre and music are the hashish smoking and betel chewing of Europeans."

constrictive metaphysics. Nietzsche reflected on this very principle in *Ecce Homo,* remarking "[w]enn man von einem unerträglichen Druck loskommen will, so hat man Haschisch nöthig[,] [w]ohlan, ich hatte Wagner nöthig" (Nietzsche, 1889 [1975], EH-Klug-6).[148] Within his emerging, physio-pharmacological lexicon, the narcotic excess becomes representative of the complacent, immovability of what was once the Apollonian, the antidote to which is joyful, invigorating intoxication.

That Nietzsche would use the symbolic language of intoxicants to denote the aesthetic is not at all surprising. By this time, Nietzsche was already long adrift in a sea of very real intoxicants, a situation that would only intensify over the course of his life. Opium, chloral, potassium bromide, hashish, at times even alcohol—all were consumed frequently and at incredibly high doses (Anderson, 2011, 110; Ciaccio, 2018). In one letter to Paul Rée and Lou Salome dated the 20th of December, 1882, Nietzsche wrote, in an opium-induced stupor, "*in opio veritas*: Es lebe der Wein und die Liebe" (Nietzsche, 1882 [1975], BVN-1882,360).[149] Famously, he would acquire many of these by signing fraudulent prescriptions written by "Dr. Nietzsche" (Anderson, 2011, 110). His own mother relayed that Nietzsche would buy chloral in bulk and relied on it for sleeping, and Nietzsche's sister would attribute his descent into madness to Nietzsche's use of still-unknown Javanese narcotic (Ciaccio, 2018, 118). Thus, intoxication was not merely metaphorical for Nietzsche. It was a literal expression of Nietzsche's perspectival encounter with his own everydayness. Intoxication was not merely a distant, occasional notion—

[148]"if one wants to do away with an unbearable pressure, hashish is necessary; thus, I had a need for Wagner."

[149] "in opium, truth: long live wine and truth."

it was a constant companion in Nietzsche's daily life. *Rausch* was, in this sense, both literal and emblematic.

Pharma-pathologies of the Will

Accompanying his long standing discussion of intoxication, a major conceptual shift occurs in Nietzsche's mature works. As has been demonstrated, Nietzsche identified his philosophical work with his own aspirations for an "experimental" physio-psychology. However, in Nietzsche's earlier work his psychological project rarely directly engaged with his symbolic language of intoxication. From the time of *Jenseits* onward, Nietzsche begins to invoke the psychiatric language of psycho-pathology (Cowan, 2005, 53). Nietzsche begins to speak of "Willensschwäche" and "Willensverlust" (Cowan, 2005, 53). Moral impulses become the product of a pathology of the will process, which, as discussed, informed all manner of mental and physical activity, as he recorded in a fragment from his *Nachlaß*:

> Wir sagen z.B. [...] Man wird ein anständiger Mensch, weil man ein anständiger Mensch ist: das heißt weil man als Capitalist guter Instinkte und gedeihlicher Verhältnisse geboren ist… Kommt man arm zur Welt, von Eltern her, welche in Allem nur verschwendet und nichts gesammelt haben, so ist man „unverbesserlich", will sagen reif für Zuchthaus und Irrenhaus… Wir wissen heute die moralische Degenerescenz nicht mehr abgetrennt von der physiologischen zu denken: sie ist ein bloßer Symptom-Complex der letzteren […] Schlecht: das Wort drückt hier gewisse Unvermögen aus, die physiologisch mit dem Typus der Degenerescenz verbunden sind: z.B. die Schwäche des Willens, die Unsicherheit und selbst Mehrheit der „Person", die Ohnmacht, auf irgend einen Reiz hin die Reaktion auszusetzen und sich zu „beherrschen", die Unfreiheit vor jeder Art Suggestion eines fremden Willens. Laster ist keine Ursache; Laster ist eine Folge… Laster ist eine ziemlich willkürliche Begriffs-Abgrenzung, um gewisse Folgen der physiologischen Entartung zusammenzufassen (Nietzsche, 1888 [1975], NF-1888,14[113]).[150]

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[150] We say, for example [...] One becomes a proper person because one is a proper person: that is, because one is born as a capitalist of good instincts and prosperous conditions... If one is born poor, from parents who have done nothing but squandered in everything and saved nothing, then one is

The moral behaviour that Nietzsche had opposed for over a decade now is no longer merely emergent of the psycho-historical process; they are "symptom complexes," as Kraepelin would say—the discernible effects of a disordering of the psychological process. This would be a defining change in Nietzsche's approach, one which marked the transition from Nietzsche's early-middle period metaphysics of the will to a fully biological conception of the subject. In *Götzen-Dämmerung,* Nietzsche went as far as identifying "[j]eder Fehler in jedem Sinne ist die Folge von Instinkt-Entartung, von Disgregation des Willens: man definirt beinahe damit das Schlechte" (Nietzsche, 1888 [1975], GD-Irrthuemer-2).[151] Each example of perceived decline, of decadence, in both the individual and in society becomes a "Symptom" (Nietzsche, 1888 [1975], GD-Vernunft-6, GD-Moral-2, GD-Moral-5, GD-Deutsche-6). Nietzsche the psychologist becomes Nietzsche the psychiatrist, diagnosing the psychical dis-eases of the day and reframing them as disorders of the mental process.

"irredeemable," one wants to say ripe for the penitentiary and the lunatic asylum... Today we no longer know how to think of moral degenerescence as separate from physiological degenerescence: it is a mere symptom-complex of the latter. Bad: the word here expresses certain incapacities, which are physiologically connected with the type of degenerecy: e.g. the weakness of the will, the insecurity and personal being-in-the-majority of the "person", the impotence to suspend the reaction to any stimulus and to "control" oneself, the bondage to any kind of suggestion of an alien will. Vice is not a cause; vice is a consequence... Vice is a rather arbitrary conceptual delimitation to summarize certain consequences of physiological degeneration.

151 "every mistake, in every sense, follows from instinctual degeneration, from disgregation of the will: one almost uses it to define the bad".

Some scholars, notably Cowan, Lampl, and Haaz, have credited this pathological turn in Nietzsche's later work to the influence of Ribot and French medical sciene, seeing them as central to Nietzsche's late development of a body-philosophy (Cowan, 2005, 56; Lampl, 1987; Haaz, 2002). They make a strong case, and Nietzsche's awareness of their works is clear. But a closer look at the interactions between Nietzsche's Wundtian conception of the will, his long-established symbology of intoxication, and his pathological turn suggests a further, Kraepelinian, dimension to his late body-psychology.

In a note from 1888 titled "Zur Physiologie der Kunst," Nietzsche appears to have listed in what way, physiologically-speaking, the effects of art are likenable to those of intoxication, writing: "1. der Rausch als Voraussetzung: Ursachen des Rausches 2. typische Symptome des Rausches 3. das Kraft- und Füllegefühl im Rausche: seine idealisirende Wirkung" (Nietzsche, 1888 [1975], NF-1888,17[9]). Nietzsche employs the same symbolic language of intoxication, but Nietzsche appears to be grounding it even more explicitly in the embodied psycho-physical experience of intoxication. In *Götzen-Dämmerung* (also 1888), where Nietzsche the psychiatrist makes his first major debut, Nietzsche introduces the language of "der Rausch des grossen Willens" (Nietzsche, 1888 [1975], GD-Streifzuege-11).[152] Here, intoxication is explicitly identified as a temporal modification of the will. However, intoxication is not merely a modification of the will; it is a pathology of the will, as Nietzsche says in another fragment from 1888:

> Es giebt zwei Zustände, in denen die Kunst selbst wie eine Naturgewalt im Menschen auftritt, über ihn verfügend, ob er will oder nicht: einmal als Zwang zur Vision,

[152] "intoxication of the great will."

> andrerseits als Zwang zum Orgiasmus. Beide Zustände sind auch im normalen Leben, nur schwächer, im Traum und im Rausch (Nietzsche, 1888 [1975], NF-1888,14[36]).[153]

Here, intoxication, and intoxicants, cease to merely serve as a representation of the effects of art. Instead, intoxication refers to a temporal pharma-pathology of psychological processes, that have the power to give rise to orgiastic expression.

Nietzsche's identification of intoxication as a pharma-pathology of the psychological process was not merely an accident of the characteristic looseness of Nietzsche's symbolic language. It figures heavily into his diagnoses of the age. Speaking on the failures of German culture, Nietzsche remarks that they should come as no surprise, as "nirgendswo sind die zwei grossen europäischen Narcotica, Alkohol und Christenthum, lasterhafter gemissbraucht worden" (Nietzsche, 1888 [1975], GD-Deutsche-2).[154] He goes as far as pondering "[w]ie ist es eigentlich möglich, dass junge Männer, die den geistigsten Zielen ihr Dasein weihn, nicht den ersten Instinkt der Geistigkeit, den Selbsterhaltungs-Instinkt des Geistes in sich fühlen — und Bier trinken" (Nietzsche, 1888 [1975], GD-Deutsche-2).[155] Beer drinking here is both symptom and cause, directly disordering the psychological process. This is a radical departure from the metaphorical equivalence between drugs and stupefecation in Nietzsche's earlier work, which

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[153] "There are two states in which art itself appears in man like a force of nature, affecting them whether they want it or not: on the one hand as a compulsion to vision, on the other hand as a compulsion to orgiasm. Both states are also present in normal life, only weaker, in dreams and in intoxication."

[154] "nowhere are the two great European narcotics, alcohol and christiandom, being abused more licentiously."

[155] "How is it actually possible that young men, who dedicate their existence to the most spiritual of goals, do not feel the first instinct of spirituality, the instinct of self-preservation of the spirit, in themselves - and drink beer?"

was a more dynamic example of Marx's similar equivalence between religion and opium. This is even a departure from a simple attribution of societal narcosis to intoxicants. Intoxication is identified with explicit physio-psychological effects on Nietzsche's overtly Wundtian conception of the psychological process. Such notions reek of nothing less than Kraepelin's work in the 1880s interpreted through the lens of Nietzsche's psycho-historical moral theory.

In this way, intoxicants, and their study within the budding field of scientific psychology, are located at the point of transition from Nietzsche's metaphysical, or at least linguistic, conception of the will to an overtly biological subject. By Nietzsche's final years, the mores of society that Nietzsche had spent most of his public life condemning had become pathologies. Pathologies of the physio-psychological process, of the will, of the will to power. Better put, they were pharma-pathologies of the will: psychological disturbances both caused by and analogous to the effects of intoxicants on the body and mind. It appears that Nietzsche own considerable, career-long, use and abuse of opium, potassium bromide, chloral, and hashish initially provided the symbolic language through which he could express what he perceived as the crises of his day, only for the symbolic to become the real. Nietzsche was not a scientific researcher. What Nietzsche's physiological and psychological research provided was a grammar, a language, and finally a framework with which to translate the experience of intoxication into a dynamically biological conception of subjectivity. The newly discovered biological subject, for all its pathologies, was an intoxicated subject.

As for Kraepelin's direct influence, Nietzsche, as is typical, leaves little explicit evidence to go on. But from the language of symptom complexes to the association between intoxicants and pathologies of a Wundtian conception of the psychological process, the late Nietzsche is rehearsing Kraepelin. In any case, it is the marriage of intoxicants and psychology that ultimately give rise to Nietzsche's conception of the biological subject. It is, perhaps, a fitting coincidence that Erwin Rohde (jr.), the son of Nietzsche's dear friend Erwin Rohde, would be a pupil of Kraepelin's, even going on to become an experimental pharmacologist (Kraepelin, 1983, 136, 242).

Freud: The Cocainization of Mind

The legacy of Freud is sometimes contrasted with that of Emil Kraepelin. Both born in 1856, their divergent impacts on the shape of mental health care over the course of the 20th century has even led to calls of a Freud-Kraepelin dualism (Trede, 2007, 237). This characterization is undoubtedly at least partially grounded in a mischaracterization of both figures: Freud as metaphysician of the psyche and Kraepelin as hard-nosed brain scientist. Yet, both figures understood all mental disturbances to ultimately be biological in character, while also upholding the practical necessity of a non-physiological science of the mind. The real point of departure between these two men was the nature of *the cure*, though that too was not always the case. For, at the turning point in Freud's shift towards his psychoanalytic method one finds not only substances of intoxication, but a brief foray into experimental psychology. Such was the substance of Freud's 1884/1885 *Über Coca,* and related publications. In cocaine, Freud had sought, and, for a moment, found, a radical cure for a host of then barely treatable psychiatric maladies. How he came to these conclusions was not only through self-experimentation with cocaine intoxication, but psychophysical research on the influence of cocaine on reaction time and muscular output. In this sense, Freud's psychological cocaine studies would mark the first serious break with neo-mechanist neurophysiology, and paved the way for Freud's later psychoanalytic work. Combined with a possible Kraepelinian influence, Freud's shift toward a composite, biological conception of the psyche relied, ultimately, on the cocainization of the mind.

During the period surrounding the publication of *Über Coca* and the subsequent papers on cocaine, Freud had been an assistant in Meynert's psychiatric clinic, starting in 1883 (Dalzell, 2011).[156] Though they would later have a quite famous falling out. Freud evidently at least initially held Meynert in high regard. There is evidence that Freud for a time found inspiration, even cause for emulation, in the aspirations of his supervisor and teacher. Although the Austrian neurologist Gabriel Anton (1858-1933) did not regard Meynert's teaching very highly, Freud found Meynert to be a venerable teacher and "the greatest brain-anatomist of his time" (Freud, 1899; Dalzell, 2011, 68). The depth of Meynert's initial impact on Freud is clearly reflected by changes in the contents of Freud's publications, as he moved from Brücke's academic physiology lab to Meynert's psychiatric clinic.

Under Brücke, Freud had published "Über den Bau der Nervenfaser und Nervenzellen beim Flußkrebs" (1882), a study of the anatomical structure of the nerve cells and fibres found in fresh-water crayfish. "Über den Bau" had been a commendable piece of research on the neurophysiology of invertabrates, although it was arguably reserved with regard to its possible theoretical implications. After taken the assistanceship under Meynert, the content of Freud publications changed dramatically. "Die Struktur der Elemente des Nervensytem" (1884) was, by comparison, a highly theoretical text which attempted to translate Freud's earlier histological studies on the structure of nervous tissue in crayfish into a model for understanding the cellular structure of the nervous system in general. In this way, "Die Struktur" clearly reflected the

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[156] When *Über Coca* is cited the text being referred to is the 1885 reprint with the addition of supplementary addenda (Nachträge).

influence of Meynert's theoretical emphasis on nerve fibre superstructure, and perhaps even shared in some of Meynert's boldness. "Ein Fall von Hirnblutung mit indirekten basalen Herdsymptomen bei Skorbut" (1884), meanwhile, was an impressive neurological case study after the Meynertian fashion, which tracked the rapid decline of a young man who arrived at the clinic with a case of scurvy (Freud, 1884b). Both publications demonstrated the formative effect of not only Meynert's mentorship, but the transition to a clinical setting.

Freud would attain the post of lecturer in 1885, which would provide an opportunity to secure funding for his sought after trip to Paris in order to intern with Charcot (Grzybowski and Żołnierz, 2020, 268). This trip has often been marked as a breaking point in Freud's personal, and conceptual, relationship with Meynert—a key step toward the development of psychoanalysis (Grzybowski and Żołnierz, 2020, 268;de Marneffe, 1991, 71; Miller et al., 1969, 608). But what if the sea change in Freud's life was something else altogether? What if it were in fact his 1884-1885 self-experimental studies on the effects of cocaine intoxication? In Meynert's psychiatric clinic, Freud became well-acquainted with the employment of chloral, morphine, and other intoxicants to suppress, or mask, the worse symptoms of their patients, for whom they could offer little cure. Cocaine would be Freud's first attempt to find a cure (Ciaccio, 2018, 118).

Freud contextualized his own cocaine study within the broader history of European coca research, reaching back in to the earliest days of Spanish intervention in the Americas (Freud, 1885a, 1-5). Early European accounts of coca use amongst the indigenous populations of South America served as a quasi-mythological backdrop for many of the claims Freud would go on to make, relaying stories of elders who had never been touched by illness and whole populations who had used coca their whole lives without repercussion (Freud, 1885a, 4-6).

Studying the effects of cocaine "in wiederholten Versuchen an mir und Anderen," the chief concern of *Über Coca* was the benefits of cocaine's euphoric properties, which Freud relayed with intoxicating exuberance (Freud, 1885a, 11):[157]

> Die psychische Wirkung des Cocainum mur. in Dosen von 0.05-0.10 gr. besteht in einer Aufheiterung und anhaltenden Euphorie, die sich von der normalen Euphorie des gesunden Menschen in gar nichts unterscheidet. Es fehlt gänzlich das Alterationsgefühl, das die Aufheiterung durch Alkohol begleitet, es fehlt auch der für die Alkoholwirkung charakteristische Drang zur sofortigen Bethätigung. Man fühlt eine Zunahme der Selbstbeherrschung, fühlt sich lebenskräftiger und arbeitsfähiger; aber wenn man arbeitet, vermisst man auch die durch Alkohol, Thee oder Kaffee hervor gerufene edle Excitation und Steigerung der geistigen Kräfte (Freud, 1885a, 12).[158]

These eurphoric highs were not without their drawbacks. Freud reported how his early, oral consumption of a 0.05g of cocaine in solution elicited some gastro effects, numbing of the mouth and lips, a quickening of the pulse, and a drying of the mucous membranes that persisted for hours after consumption, most of which abated with repeated consumption (Freud, 1885a, 11-12). But it was worth it, for the exhilarating effects of cocaine allowed Freud to complete long lasting mental and physical labour without any sign of fatigue (Freud, 1885a, 13). Better yet,

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[157] […]"in repeated experiments on myself and others."

[158] The psychic effect of Cocainum mur. in doses of 0.05-0.10 gr. consists in an exhilaration and enduring euphoria, which differs in no way from the normal euphoria of the healthy person. There is a complete absence of the feeling of being altered which accompanies the exhilaration caused by alcohol, and there is also an absence of the characteristic urge to immediate activity which accompany the effects of alcohol. One feels an increase in self-control, feels more vital and able to work; but when one works, one also misses the noble excitation and increase of mental powers caused by alcohol, tea or coffee.

Freud found that cocaine’s euphoria was not followed by depression or fatigue, nor did high doses lead to a clouding of *Bewusstseins* (Freud, 1885a, 14, 15).

Such miraculous effects led Freud to propose cocaine as a powerful cure for select illnesses of the psyche. One such benefit of cocaine, as Freud understood it, was that it had the potential to fill a substantial hole in the pharmacopoeia of psychiatry, as he suggested:

> Vielen Aerzten schien das Cocain berufen, eine Lücke im Arzneischatz der Psychiatrie auszufüllen, welcher bekanntlich über genug Mittel verfügt, die erhöhte Erregung der Nervencentren herabzusetzen, aber kein Mittel kennt, die herabgesetzte Thätigkeit derselben zu erhöhen (Freud, 1885a, 16).[159]

In this way, Freud had found a risk-free “Zaubermittel” of immediate import to psychiatrists around the world (Reichender, 1988, 172). The 1885 “Ueber die Allgemeinwirkung des Cocains” was even put to print by Freud with the intent of spreading awareness of cocaine’s potential for psychiatrists (Freud, 1885c, 49). Freud had found in cocaine not only a treatment for psychical disturbances “welche wir als Schwäche- und Depressionszustände des Nervensystems ohne organische Laesion deuten,” but as a cure (Freud, 1885c, 51).[160] Through repeated small doses over a long time span, Freud suggested that the course of these illnesses could be reversed—melancholia, neurasthenia, sexual asthenia, hysteria, even hypochondria had reportedly been cured through coca-therapy (Freud, 1885a, 16-17).

Best of all, Freud was confident that at moderate doses even chronic use had no negative effects on one’s health, though he at least acknowledged that extreme use led to symptoms

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[159] To many physicians, cocaine seemed to be called upon to fill a gap in the pharmacopoeia of psychiatry, which, as is well known, has enough means to reduce the increased excitation of the nerve centres, but knows no means of increasing the reduced activity.

[160] […] “which we interpret as states of weakness and depression of the nervous system without organic laesion.”

similar to "Alkoholismus und Morphinismus" (Freud, 1885a, 4, 6). Yet, combating precisely these two newer afflictions was one of the potential applications Freud saw in cocaine therapy. Freud reported that it had been shown to reduce withdrawl symptoms in those reliant on morphine or alcohol, and reduce the need for them (Freud, 1885a, 21, 22). Freud wrote of having read and heard of, at times, miraculous reversals in not only the reliance on morphine, but actual renovation of the general health of the patient when morphine was subsitituted for cocaine (Freud, 1885a, 22). Once patients switched to cocaine they could then far more easily wean off of it (Freud, 1885a, 20-22). Freud spoke to this with the theory that cocaine had "eine direkte antagonistische Wirkung gegen des Morphins" (Freud, 1885a, 22).[161]

Where chloral, morphine, or paraldehyde could only hide a patient's symptoms in a doss of droopy eye lids and quiet hearts, cocaine cured—bringing light to dreary minds, and maybe their guts too. The potential Freud saw in cocaine therapy would ultimately be deemed misguided. But it may very well have been the first major step towards the emergence of psychoanalysis, and to understand that development it is important to work through the psychophysical turn at the heart of Freud's cocaine study and discern what it owed to Kraepelin.

Cocaine and Reaction Time

While those elements just discussed already ranked *Über Coca* as perhaps the most substantial publications on cocaine's therapeutic benefit in its time, one of the most remarkable elements of the study is Freud's often overlooked turn to psychophysical measurement. Freud's measurements of cocaine on muscular strength and simple reaction time were published in his 1885 "Beitrag zur Kenntnis der Cocawirkung," with some brief remarks on dynometrics and

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[161] […] "a direct antagonistic effect against morphine."

reaction time making their way into the addenda of *Über Coca* (Freud, 1885b, 7; Freud, 1885a, 25). This would be Freud's only real foray into the world of psychophysical, or, in the Wundtian sense, psychological, experimentation, and it centred around cocaine.

For the reaction time experiments, Freud borrowed Exner's "Neuramoebimeter,"which was in many ways comparable to the apparatus Exner had used in past experiments on simple reaction time starting in 1873 (Freud, 1885a, 25; Freud, 1885b, 7). Though Freud clearly took some methodological guidance from Exner, the intent, and thus the conceptual foundations of their experiments were radically different. As discussed, Exner had experimented with intoxicants as controlled modifiers of fatigue, while Freud designed his experiment with the objective of understanding the psychological and physical effects of his miracle cure—cocaine. Freud's simple reaction time trials produced clear results: the introduction of cocaine consistently produced simple reaction times which were shorter than the baseline, and more even (Freud, 1885b, 7; Freud, 1885a, 25). The tests with the dynamometer were consistent with the reaction time experiments. Here, Freud found that there was a marked increase in motor power starting 15-20 minutes after ingestion, which only gradually decreased over the following 4-5 hours (Freud, 1885b, 5-6; Freud, 1885a, 25).

The psychophysically measurable effects of cocaine, Freud observed, were directly preceded by the onset of *Euphorie* (Freud, 1885b, 7). Both the shortening of reaction time and increased muscle output run "also der Coca-Euphorie parallel und scheint auch eher von der centralen Arbeitsbereitschaft, von der Hebung des Allgemeinbefindens, als von einem directen

Einflusse auf motorische Apparate herzurühren" (Freud, 1885a, 25).[162] Freud was suggesting that the curative potential of cocaine relied on the uplifting euphoria of cocaine intoxication, rather than the direct effects of cocaine on the nervous system (Reicheneder, 1988, 173; Freud, 1885b, 7). Specifically, it is attributed to cocaine's apparent stimulation of the subject's willingness to work, which could be understood as the will more generally (Freud, 1885a, 25).

Given the content of Freud psychophysical research on cocaine intoxication, it is worth considering the extent to which Kraepelinian ideas figured into Freud's experimental concept, and theoretical conclusions. Much like Nietzsche, Freud would actively and passively, through both omission and misrepresentation, obfuscate some of his formative influences. To this point, Freud and Kraepelin almost certainly had some form of now lost written correspondence, and was aware of each other's work (Dalzell, 2011, 116). Exner, Freud's colleague, would have been well aware of the reaction time research being conducted by Wundt and Kraepelin, and potentially could have directed Freud to their research (assuming Freud was not already aware).

Most of the justification for speculation concerning a possible influence by Kraepelin lies with the conceptual shift embodied in Freud's singular turn to psychophysics, and how that is reflected in his publications. There is a strong similarity between Kraepelin's 1883 description of alcohol as affecting consciousness by disordering apperception, while also dysregulating the will, and Freud's identification of cocaine as stimulating the will, without affecting consciousness (Freud, 1885a, 14, 15). Similarly, Freud's theorization that cocaine had an

[162] [They] "run parallel to the coca euphoria and also seems to originate more from the central readiness to work, from the elevation of the general condition, than from a direct influence on motor apparatuses"

antagonistic effect against morphine, while also suggesting that cocaine discernible psychophysical expression was a product of its euphoria (Freud, 1885a, 22). Since the euphoria, in Kraepelinian terms, is merely the subjective experience of the drug's effect on psychological sub-processes, it appears Freud is suggesting that cocaine antagonism with morphine also occurs at the level of opposing influences on psychological sub-processes. The degree of Freud's indebtedness to Kraepelin's 1883 research on intoxicants cannot be definitively established with the evidence available at this time. There remain, however, serious questions regarding the extent to which Freud's turn to psychophysics, his study of an intoxicant, and a shift into the realm of psychology can only be said to coincidentally overlap with Kraepelin.

In any case, Freud's experimental intoxicant study had a clear effect on the course of his later psychoanalytic work. It was here, in his work with cocaine intoxication, that Freud firmly breaks with his neurophysiological education and instead attributes physiological expression to psychological causes. One possible way of orienting this development in Freud's thinking, before even the shift in his therapeutic method under Charcot, is to identify this development as a case study in Freud's emerging interest in the psycho-somatic character of illnesses yet unexplainable through brain lesions (Springer, 2002, 21-22). Scheidt meanwhile saw Freud's cocaine studies as illuminating Freud to an unseen subconscious, which then developed while he was in Paris with Charcot (Scheidt, 1973).

Freud's intoxicated dabbling in experimental psychology marked a clear point of departure in Freud's conceptual understanding of the body and mind. This is made apparent by Freud's subsequent transition from the neo-mechanism of Meynert and Brücke to a partial parallelism, where "everything has an organic basis but not everything is organic" (Panhuysen, 1998, 20-21). Psychoanalysis was to psychiatry what histology was to anatomy insofar as psychoanalysis was essentially a hyper-specialization in the psychical frame of reference concerning the function of the mind (Trede, 2007, 239). In this sense, Freud's intoxication trials formed the basis of his conception of the biological subject. Where he had once seen nothing but the body, Freud's passage into psychoanalysis via an experimental psychological encounter with cocaine opened unto new vistas in how the somatic duplicity of psychological embodiment and the physiological body could be upheld. The subject he saw sitting across from him was biological through and through, and, as Freud reportedly liked to say, "behind every psychoanalyst stands the man with the syringe" (Campbell, 2007).

Max Weber: Work, Sobriety, and the Struggle of the Will

In the cases of Nietzsche and Freud, the argument for the significance of intoxication in their work is almost given over freely. As an employee of the psychiatric clinic and a neurophysiologist, Freud had been a direct interlocutor in many of the same discussions as Kraepelin, and conducted direct psycho-physical research on cocaine. Nietzsche, meanwhile, had been nothing less than the self-proclaimed emissary of a wine-god. But Max Weber, a principle figure in the history of sociology, was a patron of sobriety. Or so it would seem. Weber's language of sobriety lay at the crossroads of his conceptions of objectivity, science, capitalism, power, and the will—in short, the essential questions of European modernity. However, the basis of Weber's interest in sobriety was not a historical-sociological analysis of sobriety as a social phenomena. Rather, it was emergent of the interplay between his own intoxicated life and his interactions with the work of Emil Kraepelin, occasioned by Weber's own foray into the psychophysics of labour. To make sense of this, it is important to first sketch out the context surrounding Weber and Kraepelin's dispute concerning the extension of experimental psychiatry into the factory, before exploring the finer elements of Weber's conceptions of sobriety and intoxication.

In the years surrounding 1900, Kraepelin made several attempts to extend the psychiatric clinic into broader society, most notably in the domains of criminality, alcoholism, and labour (Brain, 2001, 658). This was emblematic of Kraepelin's difference with Wundt. Where Wundt was content to focus on the development of psychology as a legitimate science, Kraepelinan psychiatry had, since its inception, been grounded in an aspiration to apply experimental psychology—to bring it into the lives of others. Back in 1880, Kraepelin's *Die Abschaffung des Strafmasses - Ein Vorschlag zur Reform der heutigen Strafrechtspflege* had advocated for a reckoning on society's approach to criminality, instead aspiring to identify and treat the criminal (Kraepelin, 1880; Engstrom, 1991, 112). In 1906, Kraepelin went about realizing this earlier preoccupation with the publication of *Das Verbrechen als soziale Krankheit* (Kraepelin, 1906b). That very same year, Kraepelin published *Der Alkoholismus in München,* going on to publish "Die Schildknappen des Weinkapitals an der Arbeit" and "Die Psychologie des Alkohols" in 1911. He further sought to develop a scientific explanation for political upheaval stemming from working class life, exemplified by his interest in the aversion to work (Engstrom, 1991, 129). The experimental foundations of nearly all of these ideas are found in their nascency in Kraepelin's research on intoxicants, which at this point was still being done in Kraepelin's lab (Kraepelin, 1899; Kraepelin and Hoch, 1896).

What united these topics was a shared etiology emergent of what Kraepelin understood as a disorder, or dysregulation, of the will (Brain, 2001, 658; Engstrom, 1991, 111-113). This very psychological phenomena, one which so affected Nietzsche's thought, had already been experimentally realized and patterned onto the minds of the masses in the lab when Kraepelin's intoxicant research established the effects of alcohol and other drugs with an affectation of the will. By way of an equivalence between pathological mental states and intoxication, Kraepelin had taken the testimony of a wide array of substances of intoxication to find experimental validity in the concept of a disturbance of (Wundtian-)will. There, he had laid bare the "*Mechanik der Geisteskrankheiten*" (Kraepelin, 1883c, 187). Thus, just as a young Kraepelin had hoped to reform psychiatry on the empirical foundations of experimental psychology, the scientific validity of Kraepelin's psychiatry justified, perhaps even demanded, the extension of the clinical enterprise into the broader social order.

The relationship between Kraepelin's extensive research on the effects of alcohol and his crusade against alcoholism, which he waged with fanatical zeal, makes predictable sense (Engstrom, 1991, 116-117). Even Kraepelin's 1892 *Ueber die Beeinflussung* briefly analyzed the long-term effects of repeated alcohol use, and there is a certain consistency in the suggestion that repeated disordering dis-orders. Though, Kraepelin's intoxication trials had also raised a host of auxiliary questions and observations. Particularly important for Kraepelin were the phenomena of fatigue from repetitive tasks and improvement through practice—the effects of both of which were constantly considered in order to discern the "real" effects of intoxication (Kraepelin, 1892a, 239, 246, 249). This would transform into a series of publications on mental labour titled *Ueber geistige Arbeit*, though the foundations of Kraepelin's experimental encounter with the phenomena of practice and fatigue stem directly from his study of the effects of intoxicants on psychological processes. Select tests enlisting the use of a more rudimentary dynamometer had been among the experimental studies included in Kraepelin's 1892 *Ueber die Beeinflussung* (Kraepelin, 1892a, 95). In 1902, Kraepelin and his student Oseretzkowsky expanded on these earlier trials, publishing the results of a series of experimental ergographic studies where they studied the effects of workplace intoxicants like coffee, alcohol, and tea (Kraepelin and Oseretzkowsky, 1902).

Building on this work, Kraepelin used his ergographic studies on repetitive physical activities to develop a "work curve," a means of measuring a participant's, or worker's, personal fatigue equation for psycho-physical labour (Brain, 2001, 660, 665; Kraepelin, 1903, 6). This, Kraepelin argued, allowed for the determination of an individual's "Arbeitkraft" (work power), relative to their personal rates of fatigue following repeated completion of a mundane task and mitigating factors, such as practice (Kraepelin, 1903, 6-7). Simultaneously, it was the mental processes behind the phenomena of fatigue, repetition, and practice which gave rise to the neuroses of the working class—just as constant pressures wore down machines in a factory, so too did the monotonous vigour of labour wear down the minds of men (Brain, 2001, 659).

All of this was crucial for Weber. When Weber went to conduct empirical field research on the lives of those working in the factory—the very epitome of Weber's conception of rational modernity—he relied both on Kraepelin and, as will soon be discussed, his own intoxicated encounters. A point of almost immediate contention for Weber was the legitimacy of Kraepelin's "work curve" (Brain, 2001, 665, 666). Much as Kraepelin himself had come to recognize, the work, or performance, curve represented the output of an unrecognized array of sub-processes (Weber, 1995, 168-170; Brain, 2001, 664). In this sense, the work curve, at best, did not function as a meaningful representation of any psycho-physical phenomena. At worst, its development as a metric actively obfuscated the path to Kraepelin's intended object of study. Kraepelin's solution had been to attempt to break the work curve down into its constitutive sub-processes, though Weber suggested this only accentuated the issues apparent with the initial work curve (Brain, 2001).

A further point of criticism raised by Weber centred on the Kraepelin's duality of recovery and fatigue, where Weber questioned both the psychological unity of fatigue and its differentiation from tiredness (Brain, 2001, 666). The distinction between the perception of tiredness and the measurable phenomena of fatigue had been a significant auxiliary focus of Kraepelin's earlier intoxication trials. Through the lens of Kraepelin's intoxicated research, Exner's inability to make sense of the results from his limited study of the effects of morphine, wine, and tea on physiological reaction time rested on Exner's failure to discern between tiredness and fatigue. Where Exner had hoped to elicit fatigue using drugs that instilled a sensation of tiredness, Kraepelin's work with various intoxicants suggested that the subjective perception of tiredness (Müdigkeit) could exist unaccompanied by any signs of psychological fatigue (Ermüdung) (Exner, 1873, 627; Kraepelin, 1883; Kraepelin, 1892a, 198). They were, in fact, separate phenomena, an idea which, for Kraepelin, was bolstered by experimental evidence (Kraepelin, 1892a, 198). This distinction, Weber suggested, "opened a window onto some of the conceptual problems of industrial psycho-physics" (Brain, 2001, 666-667). Even if experimentally valid, the readily apparent interrelations of fatigue and tiredness exemplified the way in which the apparatus of experimental psychology led to theoretical distinctions which provided few meaningful insights into something like a worker's fatigue-rate.

Outwardly, Weber's analysis broke with Kraepelin. The endeavour to extend the laboratory into the factory was confounded by the nature of the factory as so much more than the mere locale of an abstracted labour process. The factory was a constellation of labour practices; of material circumstances; of dynamic motivators; and of housing conditions (Brain, 2001, 668). None of the factory's radical particularity could be captured in a single laboratory. Nevertheless, though Weber appeared to dissent with Kraepelin on methodological grounds, Weber still based his conclusions on Kraepelinian categories (Lazarsfeld and Oberschall, 1965, 189). Weber was actually hopeful that experiments similar to Kraepelin's could be conducted in a factory, but was deterred by the exorbitant cost (Lazarsfeld and Oberschall, 1965, 189; Weber, 1995). Further still, Weber's criticism of the fatigue/tiredness distinction fell short of an outright critique—Weber merely problematized the epistemological validity of Kraepelin's direct translation of the experimental artifact of fatigue into as dynamic a setting as the factory floor. In this way, the fundamental psychological categories associated with Kraepelin's experimental interactions with intoxicants of all kinds were essentially untouched by Weber's analysis.

Between drunkenness and sobriety

But this was not the full extent of Weber's interface with intoxication. Weber's fetishization of mental sobriety entailed a twofold fetishization of intoxication, one which struck at the very heart of Weber's epistemology and conception of modernity. Though Weber was hesitant to become mired in the debates raging between historicists and positivists, Weber nonetheless understood that the work he hoped to do in the social sciences necessitated a clear epistemology. By Weber's appraisal, the scientific study of cultural life differed significantly with that of the physical world, particularly concerning the transsubjective validity of their conclusions, as he wrote in *Die "Objektivität" sozialwissenschaftlicher und sozialpolitischer Erkenntnis:*

> Es gibt keine schlechthin „objektive“ wissenschaftliche Analyse des Kulturlebens oder, – was vielleicht etwas Engeres, für unsern Zweck aber sicher nichts wesentlich anderes bedeutet, – der „sozialen Erscheinungen“ unabhängig von speziellen und „einseitigen“ Gesichtspunkten, nach denen sie – ausdrücklich oder stillschweigend, bewußt oder

> unbewußt – als Forschungsobjekt ausgewählt, analysiert und darstellend gegliedert werden (Weber, 1904 (2018), 174).[163]

The social sciences, or the scientific analysis of cultural existence, can never be objective (in the way natural science can be), explicitly because there is no transcultural, or sufficiently totalizing, perspective through which to properly take in and organize the minutiae of human life. Further still, while the natural sciences sought to make knowable the laws of the physical world, even identifying the laws of the social existence would tell you nothing about the radical particularity of social reality (Weber, 2018, 174-175).

However, the impossibility of objectivity does little to undermine the historical expectation, or need, for objectivity. On the contrary, the historical naturalization of the requirement for clarity, objectivity, and scrutability is precisely what Weber meant when he said "es ist das Schicksal unserer Zeit, mit der ihr eigenen Rationalisierung und Intellektualisierung, vor allem: Entzauberung der Welt" (Weber, 1917, 22).[164] The conceptual *a prioris* behind the systematization and rationalization of the cosmos was, for Weber, what it meant to be modern, manifested in, for example, the emergence of the industrial factory and all that it entailed.

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[163] There is no such thing as an absolutely "objective" scientific analysis of cultural life or - which perhaps means something more specific, but for our purposes certainly nothing essentially different - of "social phenomena" independent of special and "one-sided" points of view, according to which they are - explicitly or implicitly, consciously or unconsciously - selected, analyzed and representatively structured as objects of research.

[164] "it is the fate of our times, to be characterized by rationalization and intellectualization, by, above all, the disenchantment of the world." Notably, Nietzsche says almost the exact same thing in "Über Wahrheit und Lüge."

Objectivity in the social sciences was thus an impossible, but nevertheless desired, ideal—a professional value to strive for, rather than a conceptual category.

How then does Weber characterize the ideal quasi-objectivity of the social scientist? He employs the language of sobriety. For Weber, sobriety represented an essential quality of a industrial capitalist's comportment toward social and material relations, as he wrote in *Die protestantische Ethik und der Geist des Kapitalismus*:

> Und ebenso ist es natürlich eine der fundamentalen Eigenschaften der kapitalistischen Privatwirtschaft, daß sie auf der Basis streng rechnerischen Kalküls rationalisiert, planvoll und nüchtern auf den erstrebten wirtschaftlichen Erfolg ausgerichtet ist (Weber, 1905 (2016), 206-207).[165]

With the emergence of industrial capitalism, of rationalization, "die alte behäbige und behagliche Lebenshaltung wich harter Nüchternheit," where it is "nüchternen Selbstbeherrschung und Mäßigkeit, welche die Leistungsfähigkeit ungemein steigert" (Weber, 2016, 189, 182).[166] In this way, the characteristic of sobriety is identified with the rationalization of modern capitalist society, the very same perspectival comportment behind the emergence of the social sciences. Weber had actually identified the duties of the university academic with upholding a "pitiless sobriety of judgement" as early as *Die Evangelisch-sozialen Kurse in Berlin im Herbst dieses Jahres* (1893) (Hennis, 1991, 36-37). It thus becomes clear that, for the social scientist, the pursuit of the unattainable ideal of objectivity finds expression in the aspiration toward sobriety.

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[165] And in the same way, of course, it is one of the fundamental characteristics of capitalist private enterprise that all is rationalized on the basis of strict arithmetical calculation, planned out and soberly directed toward the desired economic result.

[166] […] "the old sedate and comfy way of life gave way to cold, hard sobriety," where it is "sober self-control and moderation, which immensely increases the efficiency."

Sobriety itself, however, is a negative state. Without intoxication, sobriety ceases to be sobriety: it is merely the baseline condition. The language of sobriety in this sense simultaneously implies intoxication, and intoxication was something Weber knew something about. Drinking had been "the primal pleasure for the young Weber" (Radkau, 2009, 29). Ever a willing drinking-buddy, "alcohol-tinged male company, even without deeper friendships, seems to have been what excited [Weber] *par excellence*" (Radkau, 2009, 37). Weber's drinking even came to affect Weber's mental well-being, saying "that he suffered from obsessional thoughts and, especially after nights of drinking, sometimes imagined for the whole of the next day that he was Jumbo the elephant and lived in a zoo" (Radkau, 2009, 46). Weber was also a regular user of opium into the 20^{th} century, specifically to help combat his insomnia (Radkau, 2009, 155). But, the youthful pride that Weber once took in holding his drink and drugs would turn to regret as his life progressed. In 1919, Weber's "anger at the years of self-intoxication burst out of him, and his fight against insomnia suddenly turned into a fight against the 'torture' of a life 'which has to be beaten down with a thousand drugs and poisons, from opium to cocaine, in the form in which it moves'" (Radkau, 2009, 154).

It appears arguable, if not readily apparent, that Weber's operative metaphor of sobriety was emergent of his own preoccupation with intoxication. Sobriety becomes both a symptom and a biological description of the prolific rationalization that Weber saw as defining the world around him, one which he had fought with his intoxicated body and productive mind. It was undeniably an appropriate metaphor. The selfsame rationalization behind the disenchantment of the world was also the root of the tendency to think of the subject biologically, and bio-rationalization found embodiment everywhere in a society Weber recognized was run on intoxicants.

The Weberian modality of sobriety can further be contextualized through the earlier discussion of Weber's interface with Kraepelinian ideas, particularly concerning the will and intoxication. Weber clearly expressed how sober-mindedness was an essential quality of capitalist enterprise which made possible the finite planning and execution necessary to achieve their economic goals (Weber, 2016, 206-207). This evaluation is a prime example of Weber's own conception of power, as the capacity to "carry out his own will despite resistance" (Warren, 1992, 19). In this sense, power is as dependent on the capability to exert one's will as it is on the capacity to effectively direct one's will. It was Kraepelin who, as discussed, experimentally developed the conception of intoxication in general and drunkenness in particular as a disordering of the psychological processes involving the will (Kraepelin, 1883b, 600-601; Kraepelin, 1892a, 157-159). To be sober then both figuratively and literally meant to guard your psychological constitution, to care to your will. It was for such a reason that Kraepelin himself swore off alcohol (Kraepelin, 1983, 79). Sobriety, as Weber's crucial metaphor for perceptional modernity, was ultimately then about the struggle of biological society against the Kraepelinian disordering of the will rooted in intoxication. Capitalism's successes depended on it being stone-cold sober. Peering through a Weberian lens, one need look no further than the teetotalism movement's emergence alongside industrial capital to see that this is the case.

At its core, Weber's perspective on the sober modern was emergent of his own intoxicated way of knowing: intoxicants, and the struggle with them, shaped the grammar of Weber's diagnosis of his age. Intoxication not only lent itself to the vocabulary Weber made use of, but the scientific discourse made the distinction between intoxication and sobriety available as both a figurative and literal description of the relationship between industrial capitalism and the will. The state of intoxication itself quite apparently made these associations real for Weber, by directly intervening in his lived experience.

For Weber, as it had been for Nietzsche and Freud, it was not only merely his engagement with psychological research on the nature of intoxication that shaped his understanding of the modern self, but his own intoxicated life. It was the experience of intoxication that brought these concepts to the fore, forced him to feel something. The perceptional state of intoxication is what made them real, with Weber's own desire, and struggle, for sobriety becoming his personal case study on alcohol's ability to disorder the will. Each, in turn, impressed upon their work an intoxicated way of knowing—a tacit, embodied encounter of the self with the body and the world, which is subsequently concealed by the rhetorical dance of knowledge-making. It is worth pondering if, after all, the entirety of the biological subject rests on the unseen influences of substances of intoxication.

Chapter 12—Conclusion

> Dans l'opium, ce qui mène l'organisme à la mort est d'ordre euphorique. Les tortures proviennent d'un retour à rebrousse-poil vers la vie. Tout un printemps affole les veines charriant glaces et laves de feu.
> Je conseille au malade sevré depuis huit jours d'enfouir sa tête dans son bras, de coller l'oreille contre ce bras, et d'attendre. Débâcle, émeutes, usines qui sautent, armées en fuite, déluge, l'oreille écoute toute une apocalypse de la nuit étoilée du corps humain (Cocteau, 1930, 23).[167]

In 1930, the French writer Jean Cocteau recorded his musings about life, love, and intoxication in a diary he wrote during his second attempt to break free of the requirement of smoking opium, a fashionable habit among artist, poets, and philosophers in 20th century pre-war France. More poetry than diary, Cocteau's recollection is far from prosaic, fraught with aphoristic phrases such as "vivre est une chute horizontale" (Cocteau, 1930, 37).[168] Cocteau's *Opium: journal d'une désintoxication* has been called "the diary of a lost love" (Barnes, 1970, 58). But, more than that, the quote above, just one of many throughout the memoir, speaks to the text as a rumination on the relationship between embodied experience of intoxication and the body. Cocteau's *Opium* reflects the mind of an individual who, despite being far from the bio-medical sciences himself, conceives of and subjectively experiences the body as biological, a confluence of nerve centres, tissue, and pumping blood. *Opium* is the narcotic memoir of a biological subject.

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[167] Opium leads the organism to death in a euphoric mood. The torture is the process of returning to life against one's desires. An entire spring excites the veins to madness, bringing with it ice and fiery lava. I recommend the patient who has gone without for eight days bury his head in his arms, stick his ear to those arms, and wait: catastrophe, riot, factories exploding, flood; the ear can detect the entire apocalypse in the star-lit night of the body,

[168] […] "life is a horizontal fall."

This has been the story of just such a subject—at least in part. More specifically, this has been a narrative about the historical emergence of the biological subject through various expressions of intoxicated ways of knowing. At each step, intoxicated ways of knowing, as well as resistance to them, made particular conceptual associations real in the perceptional experience of the intoxicated. By its end, biological modernity can be seen reflected in the inebriated lives still being led to this day.

Fuelled by colonization and driven by experimentalism, the explosive development of the early-modern pharmacopoeia dovetailed with the Newtonian pursuit of a scientific medicine. Though there were several 18th century experiments in medical system building—it was John Brown's quantifiable principle of excitablity, a vital conception of the body borne out of the experience of intoxication, that took hold. Central to his therapeutic model were vital substances, remedies, in particular intoxicating remedies, that effected the organism by stimulating the vital principle.

In Germany, Brunonianism, for a moment, enjoyed tremendous success, owing in large part to Andreas Röschlaub's intrepid efforts to bring the concept of Brunonian excitability into agreement with the language of Romantic philosophy. This placed intoxicated excitability, and vital substances, at the forefront of *Naturphilosophie,* with the effects of opium serving as the Schelling's primary example of cause and effect. The vital substance concept even figured into the appearance of alkaloids as a kind of thing to be in the world, via Sertürner's own intoxicated encounters. Through Oken, the vital substance concept made its way into Johannes Müller's *Handbuch*, where, even as Müller distanced himself from Romanticism and Brown, he nevertheless pursued anatomical justification for the vital substance concept.

So inextricable were vitalism and vital intoxicants in mid-nineteenth century Germany that when Müller's students, Brücke, du Bois-Reymond, and Helmholtz, attempted to divest physiology of vitalism they expelled any residual conceptions of the vital substances. At its most extreme, it appeared that this position on the part of the "neo-mechanists" expressed itself in the form of a complete disassociation with researching the effects of intoxicants on the body. Physio-chemical reductionism meant privileging the experimental encounter with the physical world as the singular source of meaningful scientific knowledge. For the next generation of neo-mechanists, this translated into an interest in physiologically establishing the theoretical equation of mental states with brain states, the most prominent examples being the cortical theories of psychiatrists Meynert and Wernicke. They, too, maintained critical distance from any scientific encounter with intoxicants, outside of clinical practice.

Yet, the world of the late 19^{th} century churned with intoxicants. Morphine, alcohol, and opium had been made readily available for mass consumption, but there were other intoxicants then too. There were novel alkaloids, like cocaine, which promised effects then unseen on the European continent, as well as the rapidly expanding domain of synthetic drugs, among them chloral and amyl nitrate. Intoxicants only became more present; advertised in newspapers, available in sundry stores—substances of intoxication were a critical facet of society. Thus, in hindsight, it comes as little surprise that, out of the budding science of experimental psychology, substances of intoxication would intervene in the unfolding deliberations concerning the nature of the body and mind to concretize a new form of subjectivity. By taking the experience of intoxication as the object of experimental study, Kraepelin imparted scientific validity to the embodied, perceptional encounter between intoxicated subjects, the body, and the world. It made possible the scientific identification of intoxication as the physical made real in the psychical—

upholding a subject that was fully biological, and yet only studiable through parallel science of conscious embodiment. Further still, intoxication would help shape Kraepelin's nosologies of mental illness, by providing an experimental model for understanding psychopathological states. Intoxicated ways of knowing directly intervened in the experimental process to make recognizable not only a biological subject, but a dynamic conception of the biological mind, then beyond the reach of neurophysiology.

But this does not capture the significance of the developing interest in the psychology of intoxication, nor its import for the biological subject. Foundational to the cohesiveness of the biological subject is the realization of a biologistic conception of the mind and body as a conceptual *a priori*—both expressed through, and furthered by, the concept's place in the late nineteenth century philosophies and social theories of the body. To this point, both the influence of psychological research on intoxication and their own most drunken, stimulated, and narcotized experiences were found to be central to Nietzsche's, Freud's, and Weber's own concepts of the body and its place in modernity.

In this sense, the history of the biological subject is a history of physiology, of psychiatry, of chemistry, of neuroscience, of pharmacy, of philosophy—but, above all else, it is a history of the epistemological structures, both conceptual and perceptional, that make up the body and mind. What I have striven to establish is that intoxicated ways of knowing should be included in such a history. It may very well be the case that intoxicated ways of knowing serve a further role, perhaps another equally as impactful one, role in the history of the bio-medical sciences. More questions come to the fore than have just been answered, specifically: what does this mean for the bio-chemical identity of the modern brain? How does this factor into a society that increasingly employs the language of neurotransmitters and "chemical imbalance" to describe basic psychological wants, needs, and disturbances? An answer may very well be found through an exploration of intoxicated ways of knowing. But that’s for another story.

Appendix

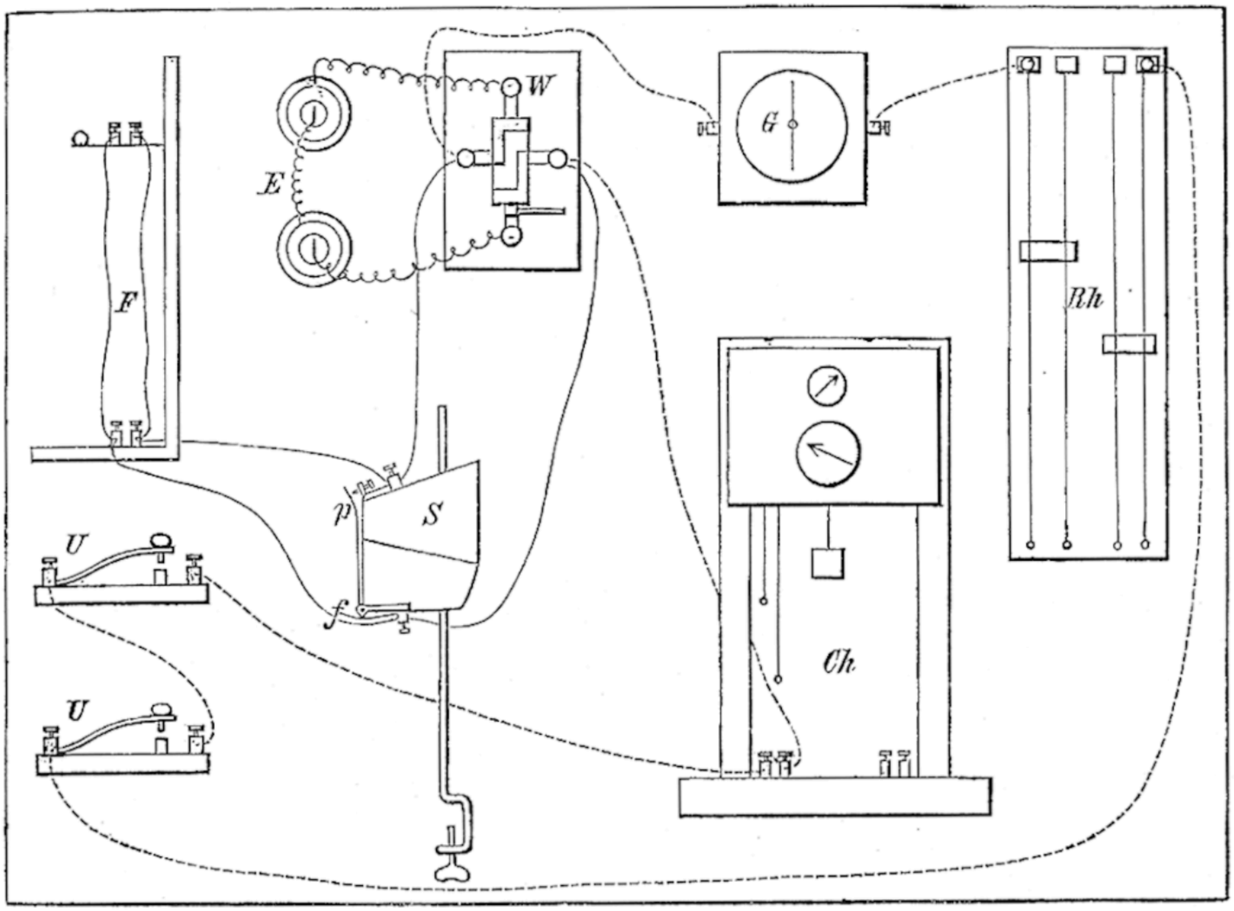


Figure 1: *Diagram of Kraepelin's apparatus (Kraepelin, 1883a, 421).*

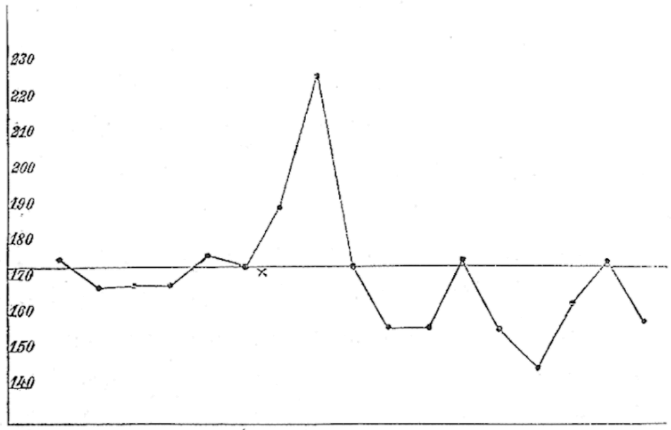


Fig. 2. Amylnitrit. Einfache Reaction.
(E. Kraepelin, 8.V. 82.)

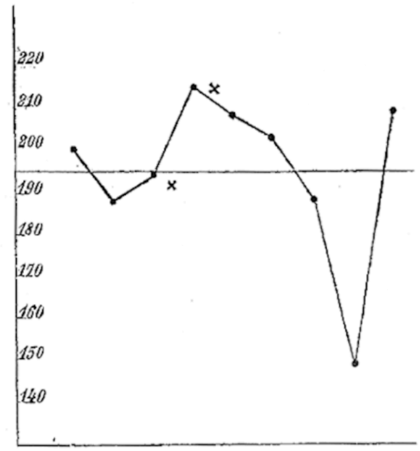


Fig. 3. Amylnitrit. Unterscheidungs-Reaction. (E. Kraepelin. 9.V. 82.)

Figure 2: Amyl nitrate's effect on simple reaction (top left), discrimination reaction (top right), and choice reaction (below) (Kraepelin, 1883a, 435, 437, 439).

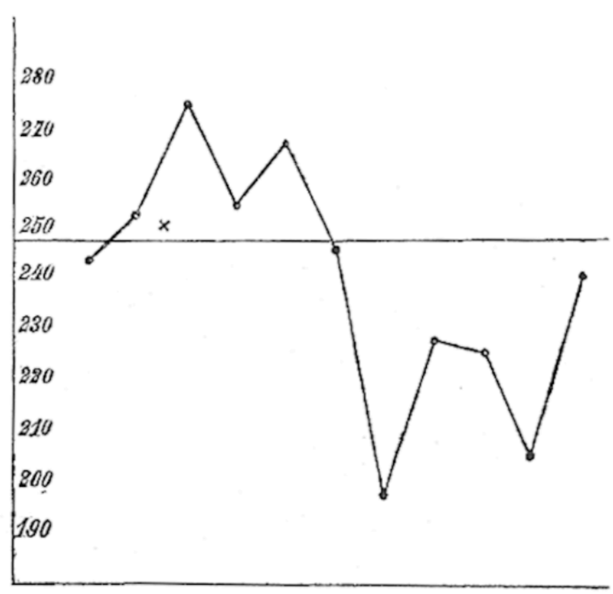


Fig. 4. Amylnitrit. Wahl-Reaction.
(M. Trautscholdt, 15.V. 82.)

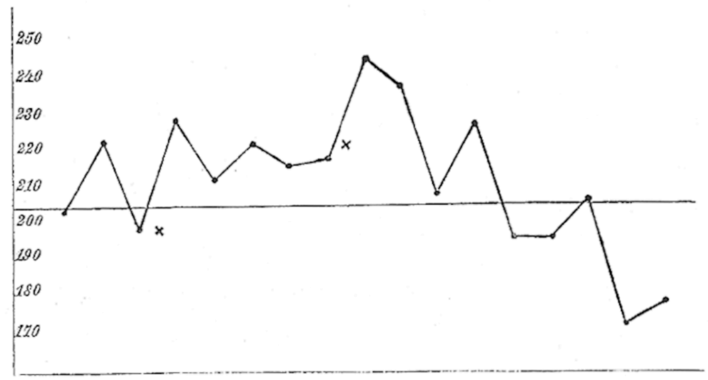
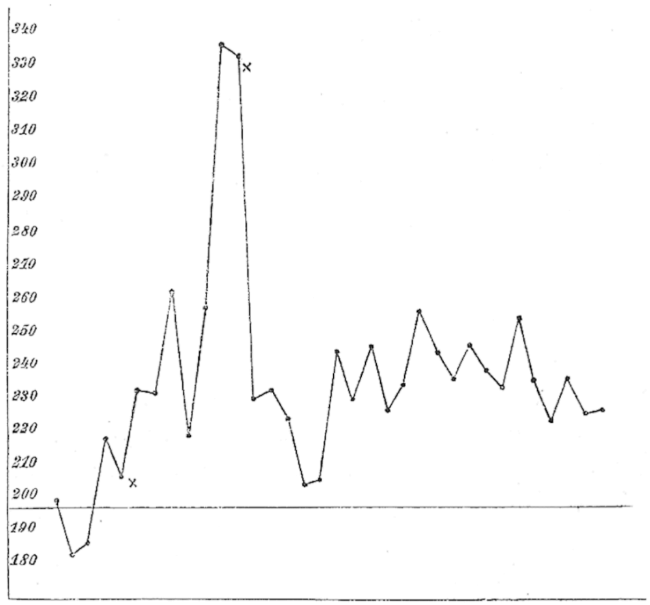


Fig. 5. Aether. Einf. Reaction. (E. Kraepelin, 10. V. 82).

Figure 3: Effects of Ether on simple reaction (top), discrimination reaction (middle left), choice reaction (deep narcosis), and choice reaction (light narcosis) (Kraepelin, 1883a, 443, 447, 450, 449).



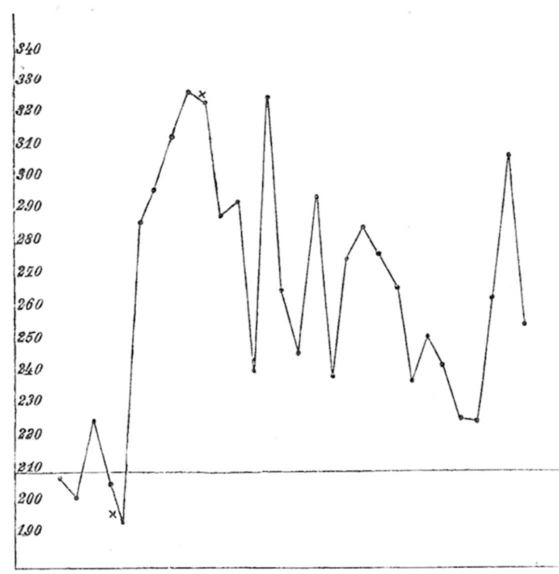


Fig. 8. Aether, Wahl-Reaction. Tiefe Narcose. (E. Kraepelin, 25. V. 82.)

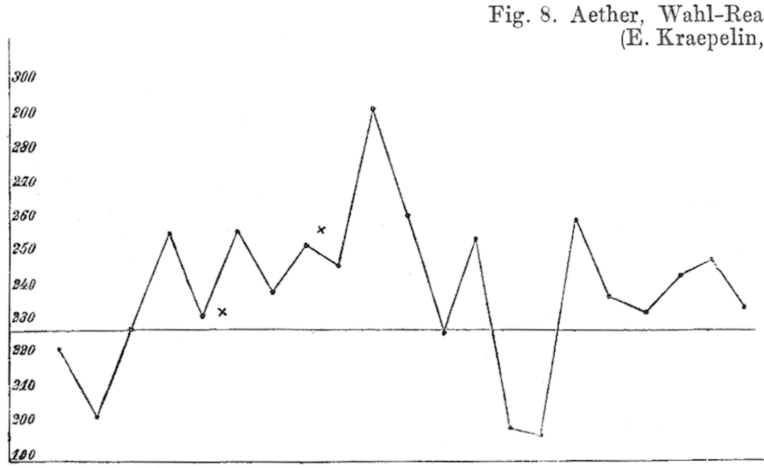


Fig. 7. Aether, Wahl-Reaction. Leichte Narcose. (E. Kraepelin, 25. V. 82.)

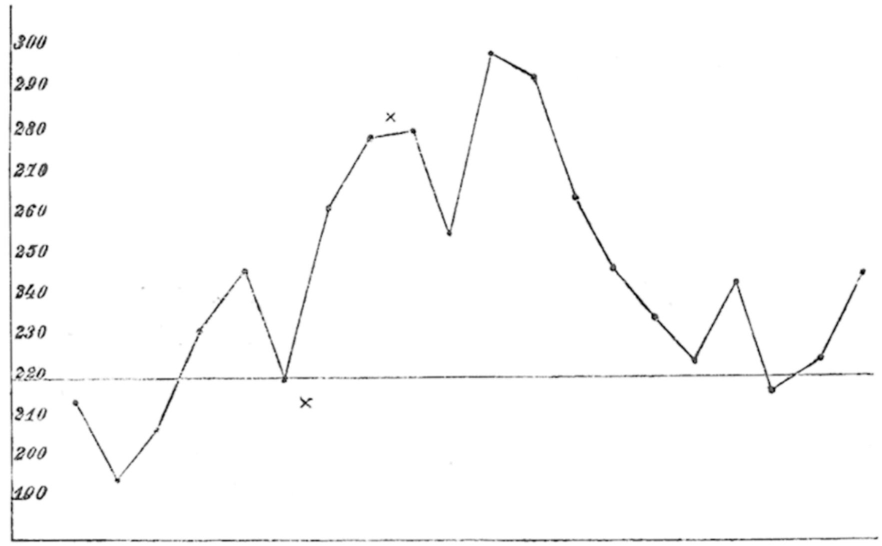


Fig. 10. Chloroform. Unterscheidungs-Reaction. (E. Kraepelin, 21. V. 82.)

Figure 4: Chloroform's effect on simple reaction left), discrimination reaction (top), and choice reaction (right) (Kraepelin, 1883a, 454, 456, 459).

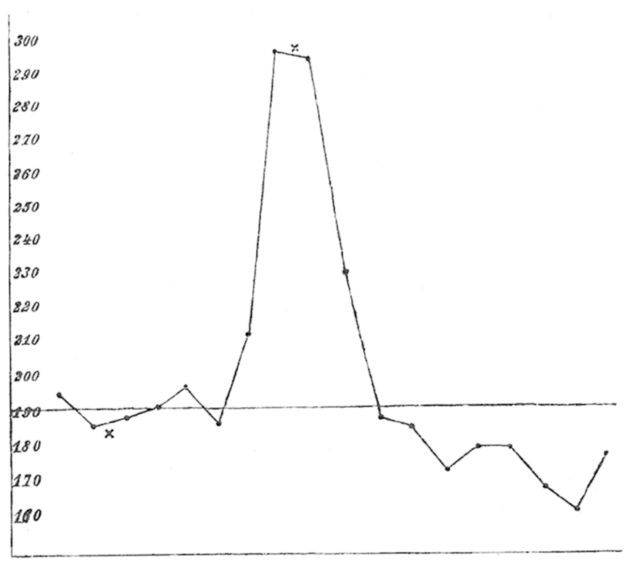


Fig. 9. Chloroform. Einfache Reaction. (E. Kraepelin, 13. V. 82.)

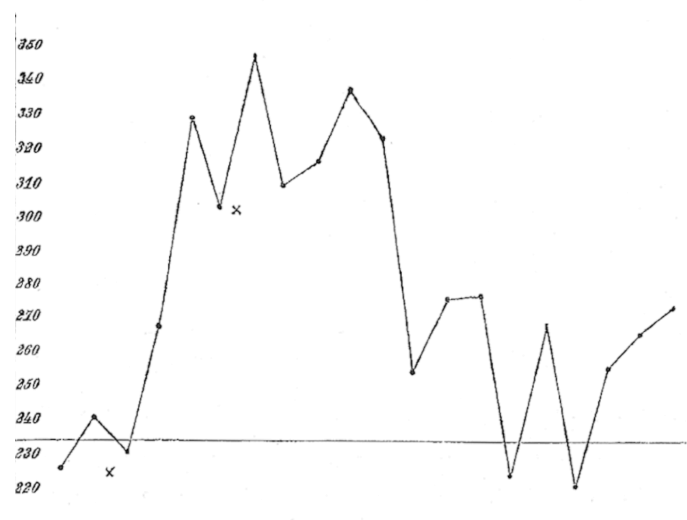


Fig. 11. Chloroform. Wahl-Reaction. (E. Kraepelin, 14. V. 82.)

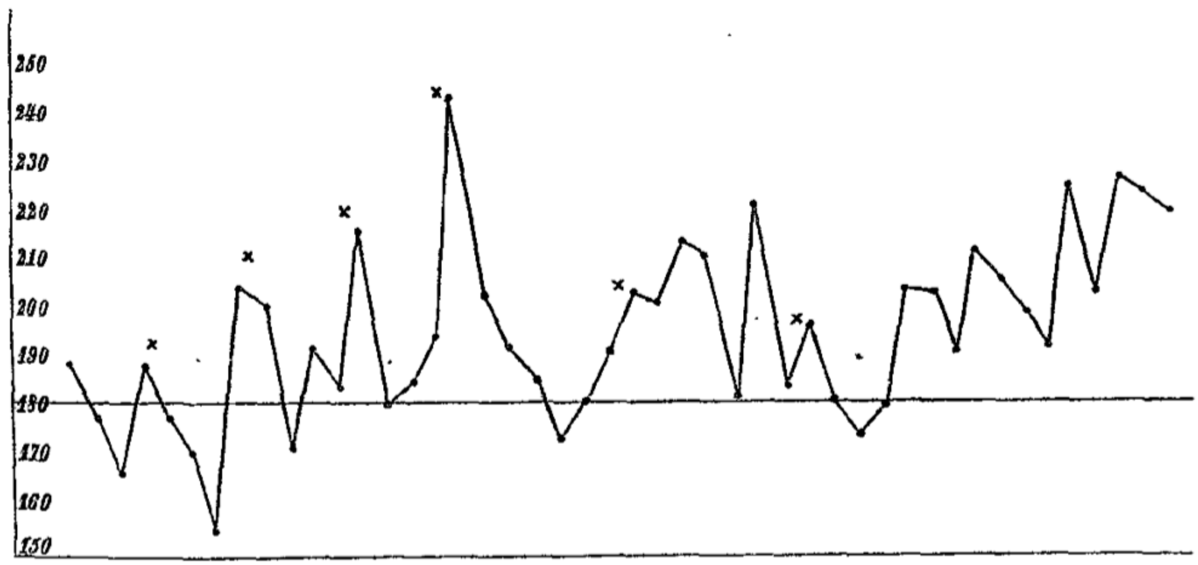


Fig. 1. Alkohol, 6 × 7,5 g. Einf. Reaction. (E. Kraepelin, 9. V. 82.)

Figure 5: Alcohol's effect on simple reaction low dose (top) and high dose (bottom) (Kraepelin, 1883b, 579, 581).

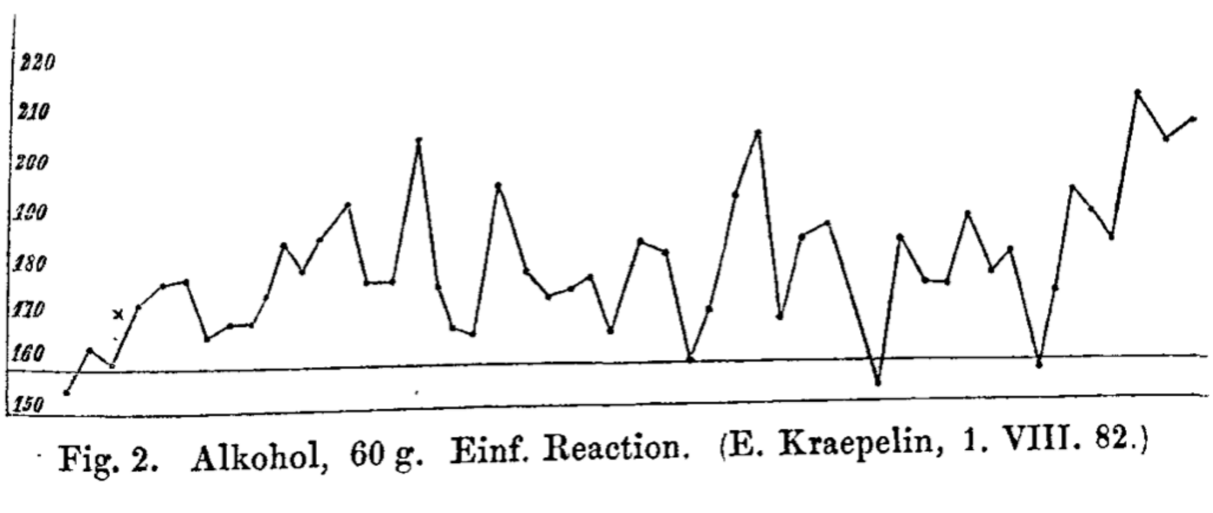


Fig. 2. Alkohol, 60 g. Einf. Reaction. (E. Kraepelin, 1. VIII. 82.)

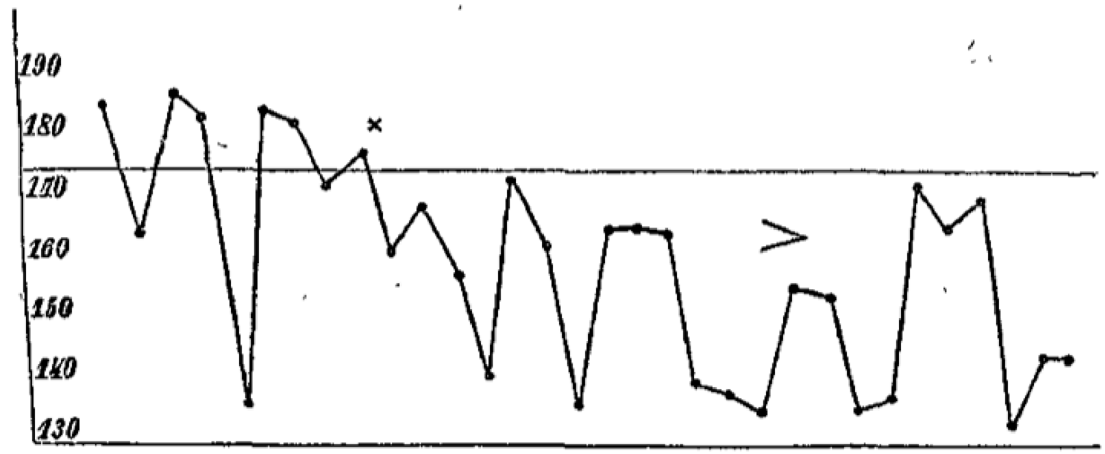


Fig. 3. Alkohol, 45 g. Untersch.-Reaction. (E. Tischer, 1. VIII. 82.)

Figure 6: Alcohol's effect on discrimination reaction time low dose (top) and high dose (bottom) (Kraepelin, 1883b, 585, 588).

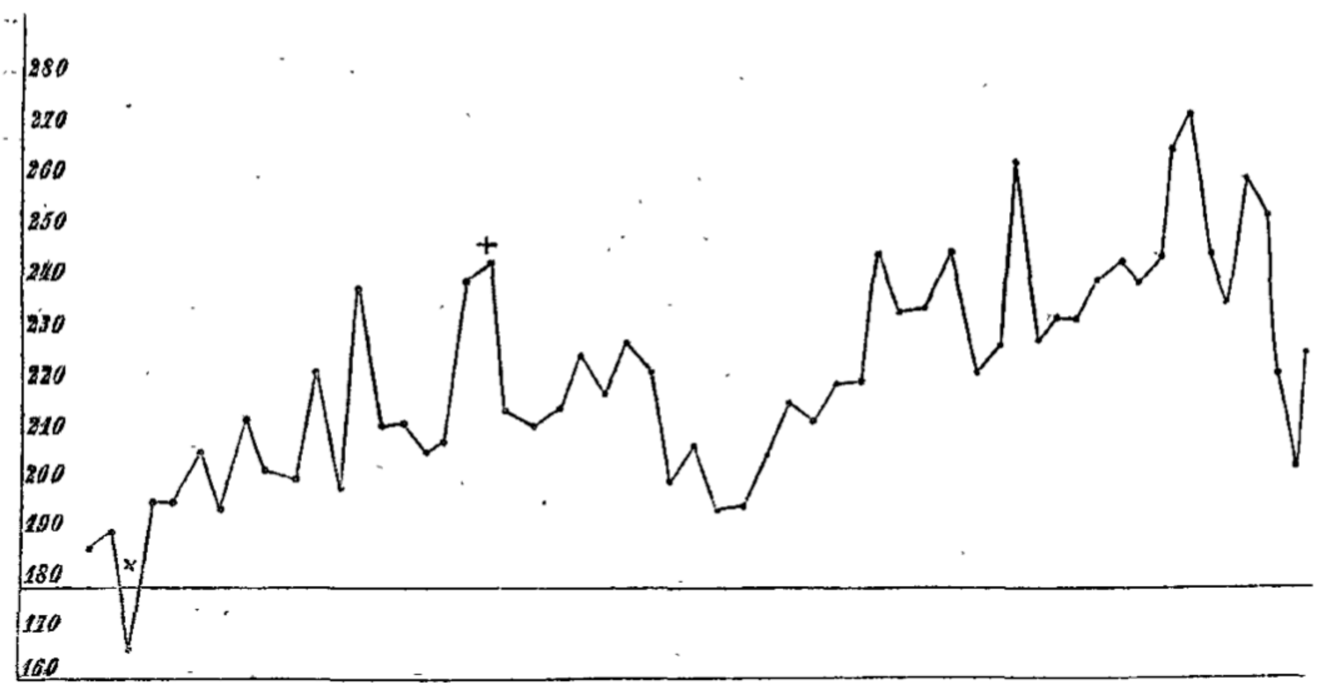


Fig. 4. Alkohol, 60 g. Untersch.-Reaction. (E. Kraepelin, 3. VIII. 82.)

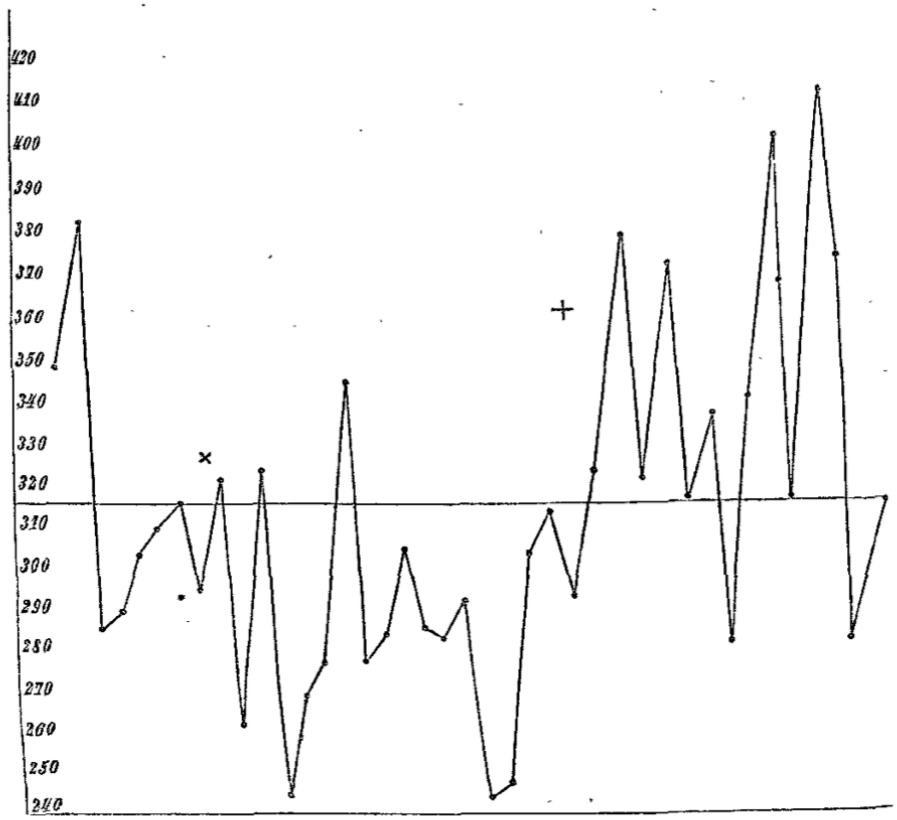


Fig. 5. Alkohol, 30 g. Wahlreaction. (E. Tischer, 27. VII. 82.)

Figure 7: Alcohol's effect on choice reaction low dose (top) and high dose (bottom) (Kraepelin, 1883b, 591, 592)

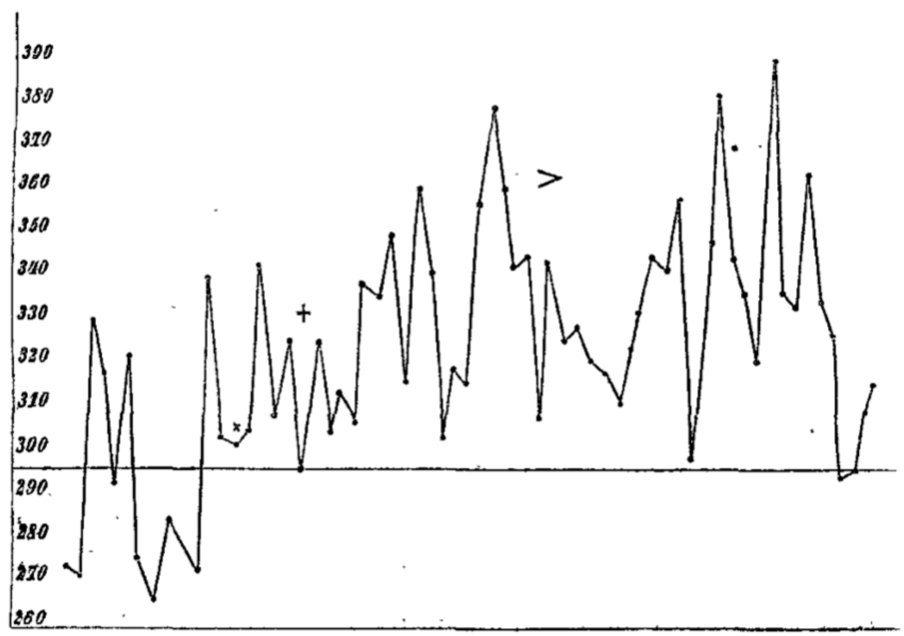


Fig. 6. Alkohol, 60 g. Wahlreaction. (E. Kraepelin, 11. XI. 82.)

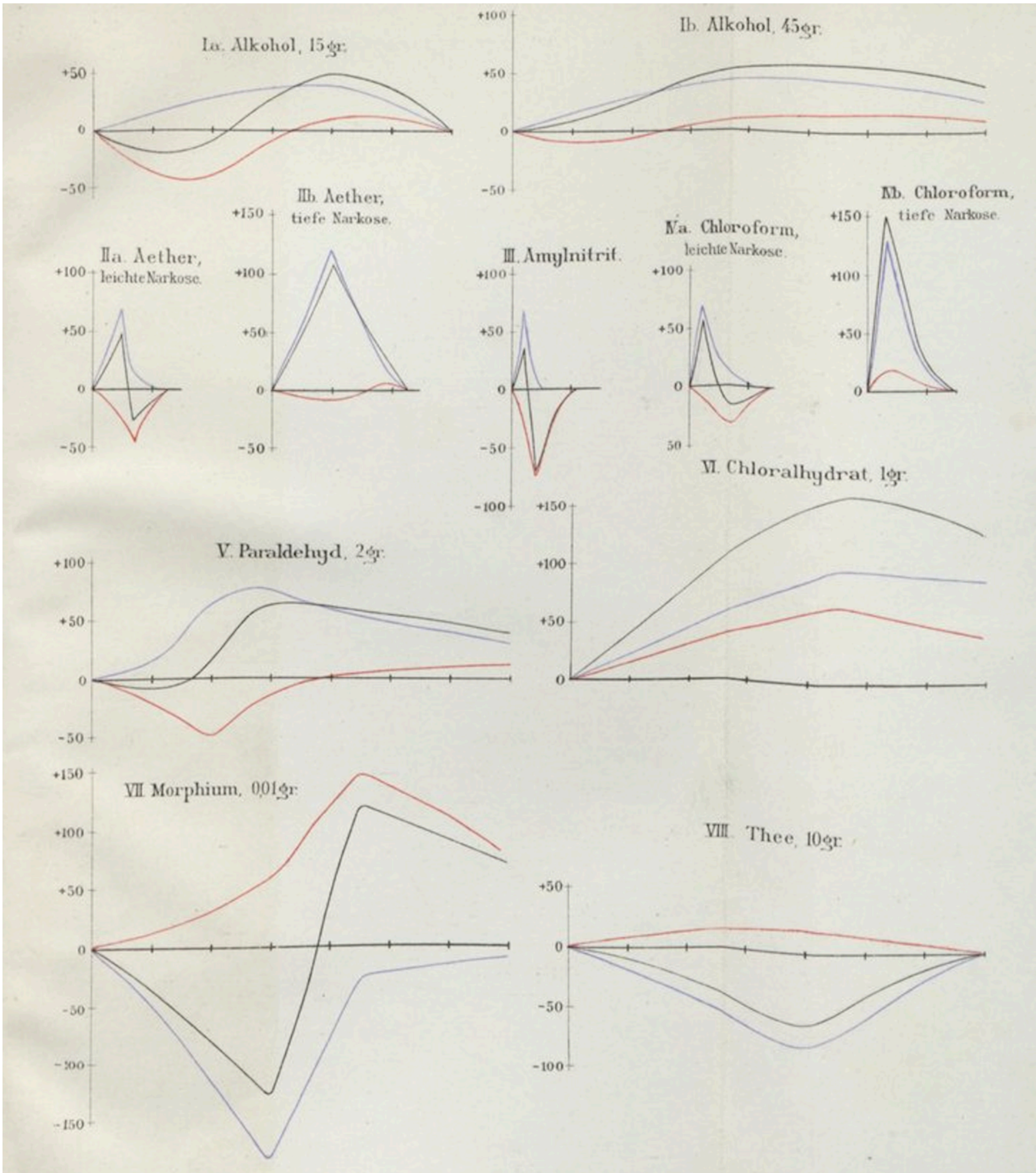


Figure 8: Collection of reaction time graphs from Kraepelin's 1892 work. The red line corresponds to motor function and the blue line corresponds to sensory/intellectual functions (Kraepelin, 1892a, unnumbered appendix)

Bibliography

Abraham, J., & Mathew, S. (2019). Merkel Cells: A Collective Review of Current Concepts. *International journal of applied & basic medical research*, 9(1), 9–13.

Adler, H. (1991). "Gustav Theodor Fechner: A German Gelehrter." In: (Kimble, G. et al), *Portraits of pioneers in psychology*. Washington, DC Hillsdale, N.J: American Psychological Association L. Erlbaum Associates.

Aldea, A.S., Allen, A. (2016). "History, critique, and freedom: the historical a priori in Husserl and Foucault." *Cont Philos Rev* 49, 1–11.

Alderwick, C. (2016). "Nature's capacities: Schelling and contemporary power-based ontologies." *Angelaki*, 21(4), 59-76.

Allik, J. and Tammiksaar, E. (2016). "Who was Emil Kraepelin and Why do we Remember him 160 Year Later?" *TRAMES*, 20(70/65), 4, 317–335.

Alturkistani, H. A., Tashkandi, F. M., & Mohammedsaleh, Z. M. (2015). "Histological Stains: A Literature Review and Case Study." *Global journal of health science*, 8(3), 72–79.

Amrein, M. and Nickelsen, K. (2008). "The Gentleman and the Rogue: The Collaboration between Charles Darwin and Carl Vogt." *Journal of the History of Biology*, 41(2), 237-266.

Anctil, M. (2015). *Dawn of the neuron: The early struggles to trace the origin of nervous systems*. Montreal: McGill-Queen's University Press.

Anderson, M. (2011). "Telling the Same Story of Nietzsche's Life." *Journal of Nietzsche Studies*, 42(1), 105–120.

Anonymous [Nature]. (1878). "Ernst Heinrich Weber." *Nature* 17, 286.

Anonymous [Nature]. (1938). "Gustav Fritsch (1838–1927)." *Nature* 141, 403–404.

Anonymous. (1721). *Pharmacopoeia Londinensis*, fourth edition.

Anonymous. (1722). *Pharmacopoeia Edinburgensis*, 2nd edition.

Anonymous. (1802). "Lob der allerneusten Philosophie" *Allgemeine Literatur-Zeitung*, no 225. 339.

Anonymous. (1866). [Review:] "On the Speedy Relief of Pain and Other Nervous Affections by Means of the Hypodermic Method." *The British and foreign medico-chirurgical review*, 37(74), 395–401.

Araujo S. (2012). "Why did Wundt abandon his early theory of the unconscious? Towards a new interpretation of Wundt's psychological project." *History of psychology*, 15(1), 33–49.

Araujo, S. (2014). "Bringing new archival sources to Wundt scholarship: The case of Wundt's assistantship with Helmholtz." *History of Psychology*, 17(1), 50–59.

Araujo, S. (2016). *Wundt and the Philosophical Foundations of Psychology: A Reappraisal*. Springer International.

Auf der Horst, C. and Labisch, A. (1999). "Heinrich Heine, der Verdacht einer Bleivergiftung und Heines Opium-Abuses." *Heine-Jahrbuch*, 38. 105-132.

Bache, F. (1980). *A System of Chemistry for the Use of Students of Medicine* (Ed. T. Thomson). Arkose Press.

Bailey, W., & Truong, L. (2001). "Opium and Empire: Some Evidence from Colonial-Era Asian Stock and Commodity Markets." *Journal of Southeast Asian Studies*, 32(2), 173–193.

Bamford, R. (2016). "The Ethos of Inquiry: Nietzsche on Experience, Naturalism, and Experimentalism." *Journal of Nietzsche Studies*, 47(1), 9-29.

Ban, T. A. (2006). "Academic psychiatry and the pharmaceutical industry." In *Progress in Neuro-Psychopharmacology and Biological Psychiatry* (Vol. 30, Issue 3, pp. 429–441). Elsevier BV.

Barnes, C. (1970). "Stage: 'Opium,' Farwell to Addiction". *The New York Times*. 58.

Beck, H. (1992). “The Social Policies of Prussian Officials: The Bureaucracy in a New Light.” *The Journal of Modern History,* 64(2), 263-298.

Behrens, P.J. (1980). “An edited translation of the first dissertation in experimental psychology by Max Friedrich at Leipzig University in Germany.” *Psychol. Res* 42, 19–38.

Benitez-Rojo, A. (1996). *The Repeating Island*. Durham: Duke University Press

Bergman, N.A. (1991). “Humphry Davy's Contribution to the Introduction of Anesthesia: A New Perspective.” *Perspectives in Biology and Medicine* 34(4), 534-541.

Bernstein, J. (1868). “Zur Theorie des Fechner'schen Gesetzes der Empfindung.” *Archiv für Anatomie, Physiologie and wissenschaftliche Medicin*, 388-393.

Bernstein, J. (1871). *Untersuchungen über den Erregungsvorgang im Nerven- und Muskelsysteme.* Heidelberg: Carl WInter's Universitätsbuchhandlung.

Berridge, V. (1999). *Opium and the People: Opiate Use and Drug Control Policy in Nineteenth and Early Twentieth Century England*. Free Association, London.

Biagioli, F. (2016). “Helmholtz’s Relationship to Kant.” In: *Space, Number, and Geometry from Helmholtz to Cassirer* (p. 1-21). *Archimedes (New Studies in the History and Philosophy of Science and Technology)*, vol 46. Springer.

Bilal, M., Edwards, B., Loukas, M., Oskouian, R. J., & Tubbs, R. S. (2017). “Johann Gaspar Spurzheim: A Life Dedicated to Phrenology.” *Cureus*, 9(5): e1295.

Bistricky, S. L. (2013). “Mill and mental phenomena: critical contributions to a science of cognition.” *Behavioral sciences* (Basel, Switzerland), 3(2), 217–231.

Boring, E. G. (1950). *A history of experimental psychology* (2nd ed.). New York, Appleton-Century-Crofts.

Bos, Gerrit. (1996). ""Balādhur" (Marking-Nut): A Popular Medieval Drug for Strengthening Memory." *Bulletin of the School of Oriental and African Studies, University of London* 59, no. 2: 229-36.

Bowler, P. J. (2005). "Revisiting the Eclipse of Darwinism." *Journal of the History of Biology* 38, no. 1: 19-32.

Brain, R. (2001). "The Ontology of the Questionnaire: Max Weber on Measurement and Mass Investigation." *Studies in history and philosophy of science*. 32. 647-84.

Breidbach, O. (2004). "Schelling and experiential science." *Sudhoffs Archiv*, 88(2), 153-74.

Brobjer, T. (2016). "Nietzsche's Reading and Knowledge of Natural Science: An Overview." In: *Nietzsche and Science*, Ed. G. Moore, T. Brodjer. Routledge. 21-50.

Brock, W. (2002). *Justus von Liebig: Chemical Gatekeeper*. Cambridge Press, Cambridge.

Broman, T. H. (2002). *The transformation of German academic medicine, 1750-1820*. Cambridge: Cambridge Univ. Press.

Brown, J. (1795). *The Elements of Medicine Or, A Translation of the Elementa Medicinae Brunonis Vol. I.* London, Johnson.

Brown, T. (1987). "Medicine in the Shadow of the Principia." *Journal of the History of Ideas*, 48(4), 629-648.

Brühl-Cramer, C. (1819). *Ueber die Trunksucht und eine rationelle Heilmethode derselben*. Berlin.

Brunton, D. (2000). "A Question of Priority: Alexander Wood, Charles Hunter and the Hypodermic Method". *Proceedings of the Royal College of Physicians of Edinburgh*. 30: 349–351.

Bucholz, Ch. F. (1800). “Versuche die Zerlegung des Opiums in seine nähere Bestandtheile betreffend; nebst einigen dahin gehörigen Bemerkungen.” *Journal der Pharmacie* 8, 1. Stück, 24– 62.

Butler, T. (1970). “The Introduction of Chloral Hydrate into Medical Practice*.” E. Bulletin of the History of Medicine*, 44(2), 168-172.

Bynum, W. F.; Porter, Roy (1988). *Brunonianism in Britain and Europe*. Welcome Trust.

Cahan, D. (1993). *Hermann von Helmholtz and the foundations of nineteenth-century science*. Berkeley: University of California Press.

Campbell, N. D. (2007). “The Man with the Syringe: Pain and Pleasure in the Experimental Situation.” In *Discovering Addiction: The Science and Politics of Substance Abuse Research* (pp. 83–112). University of Michigan Press.

Campioni, G., D’Iorio, P., Fornari, M., Fronterotta, F., Orsucci, A. (2003). *Nietzsche’s persönliche Bibliothek*. Berlin-New York. De Gruyter.

Cartwright, F. (1972). “Humphry Davy's Researches on Nitrous Oxide.” *Brit. J. Anaesth*. (1972), 44, 291-296.

Cayleff, S. (2016). *Nature's path: A History of Naturopathic Healing in America*. Baltimore, Johns Hopkins University Press.

Chvátal, A. (2017). “Discovering the structure of nerve tissue: Part 3: From Jan Evangelista Purkyně to Ludwig Mauthner,” *Journal of the History of the Neurosciences*, 26:1, 15-49.

Ciaccio, J. (2018). “Between Intoxication and Narcosis: Nietzsche’s Pharmacology of Modernity.” *Modernism/modernity*, Vol 25, 1. 115-133.

Cocteau, J. (1930). *Opium: journal d'une désintoxication*. Stock, Delamain & Boutelleau.

Coleman, W. (1999). *Biology in the nineteenth century: problems of form, function and transformation*. Cambridge University Press.

Cooke, M. (1974). “De Quincey, Coleridge, and the Formal Uses of Intoxication.” *Yale French Studies*, No. 50 [Intoxication and Literature], 26-40.

Cooper, A. (2017). “Kant and experimental philosophy.” *British Journal for the History of Philosophy*, 25:2, 265-286.

Corley, T. (1987). “Interactions between the British and American Patent Medicine Industries 1708—1914.” *Business and Economic History*, 16, 111-129.

Courtwright, D. T. (2002). *Forces of Habit: Drugs and the Making of the Modern World*. Harvard Press, Cambridge.

Courtwright, D.T. (1978). “Opiate Addiction as a Consequence of the Civil War.” *Civil War History* 24(2), 101-111.

Cowan, M. (2005). "Nichts ist so sehr zeitgemäss als Willensschwäche:” Nietzsche and the Psychology of the Will.” *Nietzsche-Studien: Internationales Jahrbuch Fur Die Nietzsche-Forschung*, 34. 48-74.

Crawford, M. J. (2019). “An Imperial Pharmacopoeia? The Pharmacopoeia Matritensis and Materia Medica in the Eighteenth-Century Spanish Atlantic World.” In: *Drugs on the Page: Pharmacopoeias and Healing Knowledge in the Early Modern Atlantic World*, Ed, M. Crawford and J. Gabriel. United States: University of Pittsburgh Press, 63-81.

Cullen, W. (1789). *Materia Medica Vol. 1*, Edinburgh.

Dahlkvist, T. (2014). “Nietzsche and Medicine.” In *Handbuch Nietzsche und die Wissenschaften* (Heit, H., & Heller, L. (Eds.).. Berlin/Boston: De Gruyter, 138-154.

Dalzell, T. (2011). “Freud and Kraepelin.” In: *Freud's Schreber between Psychiatry and Psychoanalysis*. London, Karnac/Routledge. 115-147.

Danziger K. (1980). “Wundt’s Theory of Behavior and Volition.” In: Rieber R.W. (eds) *Wilhelm Wundt and the Making of a Scientific Psychology*. Springer, Boston, MA.

Darmon, I. (2011). “No “new spirit”? Max Weber’s account of the dynamic of contemporary capitalism through “pure adaptation” and the shaping of adequate subjects.” *Max Weber Studies*, 11(2), 193–216.

Daston, L. and Galison, P. (2007). *Objectivity*. New York, Zone Books.

Daston, L., & Galison, P. (1992). “The Image of Objectivity.” *Representations*, 40, 81–128.

de la Mettrie, J. (1748*). L'homme machine*. De l'imp. d'Elie Luzac, fils.

de Marneffe, D. (1991). “Looking and Listening: The Construction of Clinical Knowledge in Charcot and Freud.” *Signs*, 17(1), 71–111.

De Quincey, T. (1885). *Confessions of an English Opium-eater* (Originally 1821). New York: John B. Alden.

Decker, H. (2007). “How Kraepelinian was Kraepelin? How Kraepelinian are the neo-Kraepelinians? - from Emil Kraepelin to DSM-III.” *History of Psychiatry*, SAGE Publications, 18 (3),337-360.

Dehio H. (1887). *Untersuchungen über den Einfluss des Coffeins und Thees auf der Dauer einfacher psychischer Vorgänge*. Dorpat: Universität Dorpat.

Deming, S. (2011). *The Economic Importance of Indian Opium and Trade with China on Britain's Economy, 1843-1890*. Economics Working Paper, [online] 25(1)

Derks, H. (2012). “Tea for Opium Vice Versa.” In *History of the Opium Problem: The Assault on the East, ca. 1600-1950* (pp. 49-86). Leiden; Boston: Brill.

Derosne, C.-L. (1803). “Mémoire sur l’opium.” *Ann.Chim*.45: 257-285

Derosne, C.-L. (1804). "Ueber das Opium", in: *Journal der Pharmacie für Aerzte, Apotheker und Chemisten*, volume XII, pp.223-53. Leipzig: S.L. Crusius, 1804.

Descartes, René, and Donald A. Cress. (1993). *Meditations on first philosophy : in which the existence of God and the distinction of the soul from the body are demonstrated*. Indianapolis: Hackett Publishing Company.

Diamond, S. (2001). “Wundt before Leipzig,” In: (ed. R. Rieber and D. Robinson): *Wilhelm Wundt in History: The Making of a Scientific Psychology*. New York: Springer Science. 3-63.

Dierig, S. (2006). *Wissenschaft in der Machinenstadt: Emil Du Bois-Reymond und seine Laboratorien in Berlin*. Göttingen: Valstein Verlag.

Dietl, M. and Vintschgau. M. (1877). “Das Verhalten der physiologischen Reactionszeit unter dem Einfluss von Morphium, Caffée und Wein.” *Pflügers Archiv* 26, 316-408.

Digby, A. (1999). The evolution of British general practice, 1850–1948. Oxford University Press, Oxford.

Dilthey, W. (1901/1927). "Friedrich der Große und die deutsche Aufklärung." In: *Studien zur Geschichte des Deutschen Geistes*. Springer, 83-200.

Domanski C. W. (2004). “A biographical note on Max Friedrich (1856-1887), Wundt's first PhD student in experimental psychology.” *Journal of the history of the behavioral sciences*, 40(3), 311–317.

Donders, F. C. (1868). “On the speed of mental processes.” Translated by W. G. Koster, 1969. *Acta Psychologica* 30: 412-431.

Donnellan, B. (1982). “Friedrich Nietzsche and Paul Rée: Cooperation and Conflict.” *Journal of the History of Ideas*, 43(4), 595-612.

Driesch, H. (1922/2016). *Geschichte des Vitalismus*. Salzwasser-Verlag.

du Bois-Reymond, E. (1847/1927). *Zwei große Naturforscher des 19. Jahrhunderts: Ein Briefwechsel zwischen Emil du Bois-Reymond und Carl Ludwig* (eds. Du Bois-Reymond and Paul Diepgen). Leipzig: Barth, 47–49.

du Bois-Reymond, E. (1848). *Untersuchungen über thierische Elektricität, Erster Band*. Verlag von G. Reimer. Berlin.

du Bois-Reymond, E. (1849). *Untersuchungen über thierische Elektricität, Zweiter Band*. Verlag von G. Reimer. Berlin.

du Bois-Reymond, E. (1875/1886). *Rede, Erste Folge*. Verlag von Veit und Comp. Leipzig.

du Bois-Reymond, E. (1887). *Rede, Zweite Folge*. Verlag von Veit und Comp. Leipzig.

Dehio, H. (1887). Untersuchungen über den Einfluss des Coffeïns und Thees auf die Dauer einfacher psychischer Vorgänge. Laakmann.

Dyde, S. (2015). “Cullen, a cautionary tale.” *Medical history*, 59(2), 222–240.

Earles, M. (1961). "Studies in the development of experimental pharmacology in the eighteenth and early nineteenth centuries." PhD diss., University of London.

Ebert, A., & Bär, K. J. (2010). “Emil Kraepelin: A pioneer of scientific understanding of psychiatry and psychopharmacology.” *Indian journal of psychiatry*, 52(2), 191–192.

Echenberg, M. (2017). *Humboldt's Mexico: In the Footsteps of the Illustrious German Scientific Traveller*. Montreal; Kingston; London; Chicago: McGill-Queen's University Press.

Eder, A. (1889). *Aerztlicher Bericht der Private-Heilanstalt des Dr. Albin Eder von dem Jahre 1888*. Vienna: Ueberreuter.

Ehrhardt, W. (2006). “Goethe und Auguste Böhmer. War sie vielleicht Goethes natürliche Tochter?” In: Steffen Dietzsch und Gian Franco Frigo (Hrsg.): *Vernunft und Glauben. Ein philosophischer Dialog der Moderne mit dem Christentum*, 277-294. Akademie Verlag.

Elkana, Y. (1970). “Helmholtz’ “Kraft”: An Illustration of Concepts in Flux.” *Historical Studies in the Physical Sciences*, 2, 263–298.

Emden, C. (2016). “Metaphor, Perception, and Consciousness: Nietzsche on Rhetoric and Neurophysiology.” In: *Nietzsche and Science*, Ed. G. Moore, T. Brodjer, 91-110. Routledge.

Engmann, B., & Steinberg, H. (2017). “Die Dorpater Zeit von Emil Kraepelin – Hinterließ dieser Aufenthalt Spuren in der russischen und sowjetischen Psychiatrie?” *Fortschritte der Neurologie-Psychiatrie*, 85(11), 675–682.

Engstrom, E. J. (1991). Emil Kraepelin: psychiatry and public affairs in Wilhelmine Germany. History of Psychiatry, 2(6), 111–132.

Engstrom, E. J. (2003). *Clinical psychiatry in imperial Germany: A history of psychiatric practice*. Ithaca, N.Y: Cornell University Press.

Engstrom, E. J. (2015). “On attitudes toward philosophy and psychology in German psychiatry, 1867-1917.” In K. S. Kendler & J. Parnas (Eds.), *Philosophical issues in psychiatry III: The nature and sources of historical change*, pp. 149–164.

Engstrom, E. J. (2016). “Tempering Madness: Emil Kraepelin’s Research on Affective Disorders.” In *Osiris* (Vol. 31, Issue 1, pp. 163–180). University of Chicago Press.

Engstrom, E. J., & Kendler, K. S. (2015). “Emil Kraepelin: Icon and Reality.” *The American journal of psychiatry*, 172(12), 1190–1196.

Exner, S. (1873). “Experimentelle Untersuchung der einfachsten psychischen Processe, Erster Abhandlung.” *Pflüger, Arch*. 7, 601-688.

Exner, S. (1875). “Experimentelle Untersuchung der einfachsten psychischen Processe, Dritter Abhandlung.” *Pflüger, Arch.* 11, 403–432.

Fahrenberg, J. (2016). "Leibniz' Einfluss auf Wundts Psychologie und Philosophie". *Psychologische Rundschau*. 67 (4): 276.

Fancher, R. and Rutherford, A. (2016). "The Sensing and Perceiving Mind: From Kant Through the Gestalt Psychologists”. In: *Pioneers of Psychology*. Norton and Company.

Fattorusso, E., and Taglialatela-Scafati, O. (2008). *Modern alkaloids: structure, isolation, synthesis and biology*. Weinheim: Wiley-VCH.

Fechner, G. (1855). *Über die physikalische und philosophische Atomlehre*. Leipzig: Hermann Mendelssohn.

Fechner, G. (1861). *Ueber die Seelenfrage. Ein Gang durch die sichtbare Welt, um die unsichtbare zu Finden*, Leipzig: Amelang.

Fechner, G. (1876). *Vorschule der Aesthetik, Erster Teil*. Leipzig: Druck und Verlag von Breitkopf und Härtel

Fechner, G. (1879). *Briefwechsel Fechner-Wundt, Wundt-Nachlaß*, Universitätsarchiv Leipzig.

Fechner, G. (1888). *Elemente der Psychophysik, Erster Theil* (Second edition, First edition 1860). Leipzig, Druck und Verlag von Breitkopf und Härtel.

Fechner, G. (1889). *Elemente der Psychophysik, Zweiter Theil* (Second edition, first edition 1860). Leipzig, Druck und Verlag von Breitkopf und Härtel.

Fechner, G. (1901). *Zend-Avesta oder über die Dinge des Himmels und des Jenseits*: *Vom Standpunkt der Naturbetrachtung* (Zweite Auflage, First edition published 1851). Leipzig: Leopold Voß.

Fechner, G. (ed.) (1826). *Repertorium der organischen Chemie: in sechs Bänden, Band 1,Teil 1*. Leipzig, Voß.

Fechner, G. (ed.). (1834*). Das Hauslexikon. Vollständiges Handbuch praktischer Lebenskentnisse für alle Stände. Erster Band*. Leipzig. Breitkopf und Härtel.

Fechner, G. (ed.). (1841). *Das Hauslexikon. Vollständiges Handbuch praktischer Lebenskentnisse für alle Stände. Zweiter Band. Zweite Auflage*. Leipzig. Breitkopf und Härtel.

Feest, U. and Sturm, T. (2011). "What (Good) Is Historical Epistemology? Editors' Introduction." *Erkenntnis (1975*-) 75, no. 3: 285-302.

Filley, C. M. (2001). *The behavioral neurology of white matter*. Oxford: Oxford University Press.

Finger, S. (2000). *Minds behind the brain: A history of the pioneers and their discoveries*. Oxford: Oxford University Press.

Finger, S., & Piccolino, M. (2011). *The Shocking History of Electric Fishes: From Ancient Epochs to the Birth of Modern Neurophysiology*. New York: Oxford University Press.

Finger, S., & Wade, N. J. (2002). "The Neuroscience of Helmholtz and the Theories of Johannes Müller Part 1: Nerve Cell Structure, Vitalism, and the Nerve Impulse." *Journal of the History of the Neurosciences*, 11(2), 136–155.

Finger, S., Piccolino, M., & Stahnisch, F. W. (2013). "Alexander von Humboldt: Galvanism, Animal Electricity, and Self-Experimentation Part 2: The Electric Eel, Animal

Electricity, and Later Years.” *Journal of the History of the Neurosciences*, 22(4), 327–352.

Finkelstein, G. (1996). *Emil Du Bois-Reymond: The Making of a Liberal German Scientist (1818-1851)*. PhD Diss. Princeton University.

Finkelstein, G. (2006). “Emil du Bois-Reymond vs Ludimar Hermann.” *Comptes Rendus Biologies*, 329(5–6), 340–347.

Finkelstein, G. (2013). *Emil du Bois-Reymond: Neuroscience, Self, and Society in Nineteenth-Century Germany.* Cambridge, Massachusetts: The MIT Press

Fischer, E. P. (1998). *Byk Gulden: Forschergeist und Unternehmermut*. München/ Zürich, Piper.

Fishman, RS. (2010). “Darwin and Helmholtz on Imperfections of the Eye.” *Arch Ophthalmol*. 128(9):1209–1211.

Floyer, J. (1687). *Pharmako-Basanos: or the Touchstone of Medicines*, London.

Forel, A. (1935). *Rückblick auf mein Leben*. Europa-Verlag, Zürich.

Foucault, M. (1990). *The history of sexuality: Volume 1*. New York: Vintage.

Foye, W.,Williams, D. and Lemke, T. (2002). *Foye's Principles of Medicinal Chemistry*. Lippincott Williams & Wilkins.

Freckelton, I., SC. (2012). “The Deaths of King Ludwig II of Bavaria and of his Psychiatrist, Professor von Gudden: Warnings from the Nineteenth Century.” In *Psychiatry, Psychology and Law* (Vol. 19, Issue 1, pp. 1–10). Informa UK Limited.

Freud, S. (1882). “Über den Bau der Nervenfaser und Nervenzellen beim Flußkrebs,” *Sitzungber. Akad. Wiss. Wien* 85: 9-46.

Freud, S. (1884a). “Die Struktur der Elemente des Nervensytem*.” Jahrbücher für Psychiatrie*, Vol. 5, 221-229.

Freud, S. (1884b). “Ein Fall von Hirnblutung mit indirekten basalen Herdsymptomen bei Skorbut.” *Wien. Med. Wschr*. 34:244-246, 276-279.

Freud, S. (1885a). *Über Coca*. Wien, Moritz Perles.

Freud, S. (1885b). “Beitrag zur Kenntnis der Cocawirkung.” Seperatabdruck von: *Wiener Med. Wochenschrift* 35, 5. 1-8.

Freud, S. (1885c). 'Ueber die Allgemeinwirkung des Cocains'. *Zeitschrift für Therapie mit Einbeziehung der Elektro- und Hydroherapie*, 3. 49-51.

Freud, S. (1885d). “Ein Fall von Muskelatrophie mit ausgebreiteten Sensibilitätsstörungen (Syringomyelie).” *Wien. med. Wschr*. 35: 389-392, 425-429.

Freud, S. (1886). “Akute multiple Neuritis der spinalen und Hirnnerven.” *Wien. Med. Wschr*. 36:168-172

Freud, S. (1891). *Zur Auffassung der Aphasien*. Leipzig und Wein: F. Deuticke.

Freud, S. (1895). *Entwurf einer Psychologie*. Unpublished Manuscripts. Library of Congress.

Freud, S. (1899). *Die Traumdeutung*. Leipzig und Wein: F. Deuticke.

Friedrich, C. (1998). “Der Fabrikant und sein Berater: Heinrich Emanuel Merck (1794-1855) und Johann Bartholomäus Trommsdorff (1770-1837).” *Pharm. Ind*. 60, 6, 508-511.

Friedrich, C. (n.d.). “German Pharmacopoeias.” ISHPWG Germany.

Friedrich, M. (1883). “Über die Apperzeptionsdauer bei einfachen und zusammengesetzten Vorstellungen.” *Philos Stud* 1:39–77

Fritsch, G and Hitzig, E. (1870). “Über die elektrische Erregbarkeit des Großhirns.” *Archiv für Anatomie, Physiologie und Wissenschaftliche Medicine*: 300–332.

Fuhrmans, H. (1962). *F. W. J. Schelling Briefe und Dokumente*. Bonn: H Bouvier.

Fullinwider, S. (1991). “Darwin Faces Kant: A Study in Nineteenth-Century Physiology.” *The British Journal for the History of Science*,24(1), 21-44.

Fye, W. B. (1999). “Rudolf Albrecht von Koelliker.” *Clin Cardiol*, 22: 376-377.

Gaedcke, F. (1855). “Ueber das Erythroxylin, dargestellt aus den Blättern des in Südamerika cultivirten Strauches Erythroxylon Coca Lam.” *Archiv der Pharmazie*. 132 (2): 141–150.

Gambaratto, A. (2018a). “Lorenz Oken (1779–1851): Naturphilosophie and the reform of natural history.” *The British Journal for the History of Science*, 51(3), 329–340.

Gambarotto, A. (2018b). “Vital Forces, Teleology and Organization.” In *History, Philosophy and Theory of the Life Sciences*. Springer International Publishing.

Gao, M. (2020). "Founding Its Empire on Spells of Pleasure": Brunonian Excitability, the Invigorated English Opium-Eater, and De Quincey's "China Question". *Literature and Medicine*; 38(1):1-25.

Garrett, B. (2003). “Vitalism and Teleology in the Natural Philosophy of Nehemiah Grew (1641-1712).” *The British Journal for the History of Science*, vol. 36, no. 1, pp. 63–81.

Gerabek, W.E. (2001). “Lorenz Oken und die Medizin der Romantik.” In: Breidbach O., Fliedner HJ., Ries K. (eds) *Lorenz Oken (1779–1851).* Verlag Hermann Böhlaus Nachfolger Weimar, Stuttgart.

Gescheider, G. (2015). *Psychophysics: The Fundamentals*. Routledge Press.

Geschwind, N. (1970). “The Organization of Language and the Brain.” *Science*, 170(3961), 940-944.

Gibson, W. (1970). “The Bio-Medical Pursuits of Christopher Wren.” *Medical History*, 14(4), 331-341.

Gladstone E. (1993). “Johann Sigismund Elsholtz: Clysmatica Nova (1665): Elsholtz' Neglected Work on Intravenous Injection: Part IV.” *Cal West Med*;39(3):190-193.

Glynn, I. (2013). *Elegance in science: The beauty of simplicity*. Oxford: Oxford University Press.

Goethe, J. W. von (1811/1989). *Goethes Werke (Aus meinem Leben: Dichtung und Wahrheit, Dritte Teil, 11. Band).* Hamburger Ausgabe, Munich: Beck.

Golinski, J. (2011). “Humphry Davy: The Experimental Self.” *Eighteenth-Century Studies* Vol. 45, No. 1, The Disorder of Things, pp. 15-28.

Golomb, J. (2015). “Introductory Essay: Nietzsche’s New Psychology.” In: *Nietzsche and Depth Psychology*, Ed. J. Golomb, R. Lehrer, W. Santaniello. SUNY. 149-169.

Gordon, C. (1998). “Canguilhem: life, health and death.” *Economy and Society*, 27(2–3), 182–189.

Grant, I. H. (2008). *Philosophies of nature after Schelling*. London: Continuum International Pub. Group.

Greenblatt, S. H. (1991). “The Development of Modern Neurological Thinking in the 1860s.” *Perspectives in Biology and Medicine* 35(1), 129-139.

Griffith, R. D., Abyaneh, M.-A. Y., Falto-Aizpurua, L., & Nouri, K. (2014). “Demystifying Merkel.” *JAMA Dermatology*, 150(8), 814.

Gross, C. G. (2007). “The Discovery of Motor Cortex and its Background.” *Journal of the History of the Neurosciences*, 16(3), 320–331.

Grundfest, H. (1963). “The Different Careers of Gustav Fritsch (1838-1927).” *Journal of the History of Medicine and Allied Sciences*, 18(2), 125-129.

Grzybowski, A. & Żołnierz, J. (2020). “Sigmund Freud (1856–1939).” *Journal of Neurology*. 268

Guareschi, I. (1896). *Einführung in das Stadium der Alkaloids mit besonderer Berücksichtigung der vegetabilischen Alkaloids und der Ptomaine*, Gaertners Verlagsbuchhandlung, Handlung, Berlin.

Gudden, B. V. (1875). "Ueber ein neues Microtom." *Archiv für Psychiatrie und Nervenkrankheiten*, 5, 229-234.

Guenther, K. (2015). *Localization and its discontents: A genealogy of psychoanalysis and the neurodisciplines*. Chicago: Chicago University Press.

Gwyn, J. (2001). "The Halifax Naval Yard and Mast Contractors." *The Northern Mariner/Le marin du nord*, XI, No. 4 (October 2001), 1-25.

Haaz, I. (2002). "Les conceptions du corps chez Ribot et Nietzsche." *À partrir des Fragments posthumes de Nietzsche, de la Revue philosophique de la France et de l'étranger et de la Recher- che-Nietzsche*. Paris.

Hachtmann, R. (1997). *Berlin 1848: eine Politik- und Gesellschaftsgeschichte der Revolution*. Bonn: J.H.W. Dietz.

Hacking, I. (2002). *Historical Ontology*. Cambridge: Harvard University Press.

Hacking, I. (2009). "La Mettrie's Soul: Vertigo, Fever, Massacre, and The Natural History". *CBMH*, Vol 26:1, 179-202.

Hagner, M. (2012). "The Electrical Excitability of the Brain: Toward the Emergence of an Experiment." *Journal of the History of the Neurosciences*, 21:3, 237-249.

Hajdu, Steven I. (2002). "A note from history: Introduction of the cell theory". *Annals of Clinical and Laboratory Science*. 32 (1): 98–100.

Hakosalo, H. (2006). “The brain under the knife: serial sectioning and the development of late nineteenth-century neuroanatomy.” *Studies in history and philosophy of biological and biomedical sciences*, 37(2), 172–202.

Hay, M. (1882/1883). "Vegetable Alkaloids, and the Methods of their Separation." *The Pharmaceutical Journal and Transactions*, 3rd ser., XIII: 719-21.

Healy, D. (2008). *Mania: A Short History of Bipolar Disorder*. United States: Johns Hopkins University Press.

Healy, D. (2009). *The Creation of Psychopharmacology*. United Kingdom: Harvard University Press.

Heckers, S., & Kendler, K. S. (2020). “The evolution of Kraepelin's nosological principles.” *World psychiatry: official journal of the World Psychiatric Association* (WPA), 19(3), 381–388.

Heidelberger, M. (2003). “The Mind-Body Problem in the Origin of Logical Empiricism.” In C. Klohr (Trans.), *Logical Empiricism* (pp. 233–262). University of Pittsburgh Press.

Heidelberger, M. (2004). *Nature From Within: Gustav Theodor Fechner And His Psychophysical Worldview*. University of Pittsburgh Press.

Heine, H. (1834/87). *Zur Geschichte der Religion und Philosophie in Deutschland*. Verlag von Otto Hendel.

Heine, H. (1972). *Werke und Briefe in zehn Bänden*. Band 5, Berlin und Weimar. 1972.

Helmholtz, H. V., Bois-Reymond, E. D., Kirsten, C., Hörz, H., & Wollgast, S. (1986). *Dokumente einer Freundschaft: Briefwechsel zwischen Hermann von Helmholtz und Emil du Bois-Reymond, 1846-1894*. Berlin: Akademie-Verlag.

Helmholtz, H. von (1850). "Vorläufiger Bericht über die Fortpflanzungs-Geschwindigkeit der Nervenreizung." *Archiv für Anatomie, Physiologie und wissenschaftliche Medicin*.

Helmholtz, H. von (1867). *Handbuch der physiologischen Optik*, Leipzig: Leopold Voss.

Hennis, W. (1991). "The pitiless "sobriety of judgement": Max Weber between Carl Menger and Gustav von Schmoller — the academic politics of value freedom." *History of the Human Sciences*, 4(1), 27–59.

Hennock, E. P. (2007). *The origin of the welfare state in England and Germany, 1850-1914: Social policies compared*. Cambridge: Cambridge University Press.

Herbart, J. (1850). "Lehrbuch zur Psychologie." In: Hartenstein (ed.), *Herbarts Sämmtliche Werke* (Bande 5, 3–187). Leipzig: Voss.

Hergenhahn, B. & Henley, T. (2014). *An introduction to the history of psychology*. Belmont, CA, USA: Wadsworth Cengage Learning.

Hildebrandt, H. (1993). Der psychologische Versuch in der Psychiatrie: Was wurde aus Kraepelins (1895) Programm?. In *Psychologie und Geschichte* (Vols. 5, Issue 1/2). Verlag C. W. Leske + Budrich GmbH.

Hippius, H. (2008). *The University Department of Psychiatry in Munich: from Kraepelin and his predecessors to molecular psychiatry*. Heidelberg: Springer.

Hitzig, E. (1874). *Physiologische und klinische Untersuchungen über das Gehirn, Erster Teil*. Berlin, Verlag von August Hirschwald.

Hlade, J. (2021). "Reconsidering "Brain Mythology."" *Medicina Historica*, Vol. 5, N. 1. 1-9.

Hoff, P. (1992). "Emil Kraepelin and Philosophy: The Implicit Philosophical Assumptions of Kraepelinian Psychiatry." In: Spitzer M., Uehlein F., Schwartz M.A., Mundt C. (eds) *Phenomenology, Language & Schizophrenia*. Springer, New York, NY.

Hoff, P. (1992b). “Psychiatrie und Psychologie—Bemerkungen zum Hintergrund des Kraepelinschen Wissenschaftsverständnisses.” In: Oldigs-Kerber J, Leonhard JP (eds) *Pharmakopsychologie— Experimentelle und klinische Aspekte*. Fischer, Stuttgart, 25–43.

Hoff, P. (1994). *Emil Kraepelin und die Psychiatrie als klinische Wissenschaft. Ein Beitrag zum Selbsverständnis psychiatrischer Forschung* [Monographien aus dem Gesamtgebiet der Psychiatrie 73]. Springer, Berlin Heidelberg New York.

Hoff, P. (2015). “The Kraepelinian tradition.” *Dialogues in clinical neuroscience*, 17(1), 31–41.

Hoffmann, C. (2006). *Unter Beobachtung–Naturforschung in der Zeit der Sinnesapparate*. Wallstein Verlag, Göttingen.

Hofmann, A. W. v. (1876). “The Life-work of Liebig:” In *Experimental and Philosophic Chemistry; with Allusions to His Influence on the Development of the Collateral Sciences and of the Useful Arts ; a Discourse Delivered to the Fellows of the Chemical Society of London in the Theatre of the Royal Institution of Great Britain, on March the 18th, 1875*; the Faraday Lecture for 1875. United Kingdom: Macmillan.

Holmes, P. (2002). “Galen of Pergamon: A Sketch of an Original Eclectic and lntegrative Practitioner, and His System of Medicine”. *Journal of the American Herbalists Guild*.

Holub, R. (2015). “The Birth of Psychoanalysis from the Spirit of Enmity: Nietzsche, Rée, and Psychology in the Nineteenth Century.” In: *Nietzsche and Depth Psychology*, Ed. J. Golomb, R. Lehrer, W. Santaniello. SUNY. 149-169.

Hoppe, B. (2007). "Robin Keen: The Life and Work of Friedrich Wöhler (1800–1882)". *Isis*, 98 (1): 195–196.

Hoquet, T. (2018). *Revisiting the Origin of Species, the Other Darwins*. Routledge.

Housman, B., Bellary, S. S., Walters, A., Mirzayan, N., Shane Tubbs, R., & Loukas, M. (2012). “Moritz Heinrich Romberg (1795-1873): Early founder of neurology.” *Clinical Anatomy*, 27(2), 147–149.

Hubbard, J. A., & Binder, D. K. (2016). “History of Astrocytes.” In *Astrocytes and Epilepsy* (pp. 1–38). Elsevier.

Hunter, C. (1865). *On the speedy relief of pain and other nervous activities by means of the hypodermic method*. London, Churchill and Sons.

Huxtable, R. and Schwarz, S. (2001). “The Isolation of Morphine—First Principles in Science and Ethics.” *Molecular Interventions* 1(4):189-91.

Jacobi, E. (1965). *Der Neurologie Moritz Heinrich Romberg 1795–1873*. Inaugural dissertation medicine. Faculty University, Zurich.

Jacobson, M. (1991). *Developmental Neurobiology*. Boston: Springer.

James, K., & Withington, P. (2022). “Introduction to Intoxicants and Early Modern European Globalization.” *The Historical Journal*, 65(1), 1-11.

Jaspers K. (1913). *Allgemeine Psychopathologie. Ein Leitfaden für Studierende, Ärzte und Psychologen*. Berlin: Springer.

Jenkins, A. (2007). “Alexander von Humboldt's "Kosmos" and the Beginnings of Ecocriticism.” *Interdisciplinary Studies in Literature and Environment*, 14(2), 89-105.

Jeston, J., & Dick, P. (1823). “No IV. English Opium.” *Transactions of the Society, Instituted at London, for the Encouragement of Arts, Manufactures, and Commerce*, 41, 17-31.

Jones, E. (1953). *Life and Work, Vol. I, The Young Freud, 1856-1900*, London.

Jones, R. (1994). “The Positive Science of Ethics in France: German Influences on "De la division du travail social".” *Sociological Forum*, 9(1), 37-57.

Jurna, I. (2003). "Sertürner und Morphin--eine historische Vignette." *Der Schmerz* 17, no. 4: 280-83.

Kane, H. (1881). *Drugs that Enslave: The Opium, Morphine, Chloral, and Hashisch Habits*. Philadelphia, Presley Blakiston.

Kant, I. (1781). *Kritik der reinen Vernunft*. Leipzig: F. Meiner, 1919.

Kant, I. (1784/1999). "Was ist Aufklärung?" In *Was ist Aufklärung:Ausgewählte Kleine Schriften* (ed. Cassirer, E., Brandt, H. D.,). Hamburg: Meiner.

Kant, I. (1787). *Metaphysische Anfangsgründe der Naturwissenschaft*. Riga, Johann F. Hartknoch.

Kant, I. (1790). "Kritik der Urteilskraft." In: *Akademieausgabe von Immanuel Kants Gesammelten Werken Bände und Verknüpfungen zu den Inhaltsverzeichnissen Band V: Kritik der praktischen Vernunft, Kritik der Urteilskraft*. 165-487.

Kant, I. (1797). "Metaphysik der Sitten, in zwei Teilen." In: *Akademieausgabe von Immanuel Kants Gesammelten Werken Bände und Verknüpfungen zu den Inhaltsverzeichnissen Band VI: Die Religion innerhalb der Grenzen der blossen Vernunft, Die Metaphysik der Sitten.* 203-473.

Kant, I. (1798). "Anthropologie in pragmatischer Hinsicht." In: *Akademieausgabe von Immanuel Kants Gesammelten Werken Bände und Verknüpfungen zu den Inhaltsverzeichnissen Band VII: Der Streit der Fakultäten, Anthropologie in pragmatischer Hinsicht*. 117-335.

Kant, I. (1922). *Briefwechse*l. In Kant 1900–, Vols. 10–13.

Kapoor, L. D. (1995). *Opium Poppy: Botany, Chemistry, and Pharmacology*. New York: Food Products Press.

Karch, S. (2006). *A Brief History of Cocaine*, Second Ed. Taylor and Francis.

Karenberg, A. (1990) “Lehrbuch der Nervenkrankheiten des Menschen” von Moritz Heinrich Romberg (1840). *Therapiewoche* 40:3407–3410.

Kelly, A. (2012). *The Descent of Darwin: The Popularization of Darwinism in Germany, 1860-1914.* UNC Press Books.

Kennedy, J. (1985). *Coca exotica: the illustrated story of cocaine*. Fairleigh Dickinson University Press.

Kennedy, P. (1987). *The Rise and Fall of the Great Powers: Economic Change and Military Conflict from 1500 to 2000* (1st ed.). New York: Random House.

Kielhorn, F.-W. (1996). “The history of alcoholism: Brühl-Cramer’s concepts and observations.” *Addiction*, 91(1), 121–128.

Kitchen, M. (2011). *A History of Modern Germany: 1800 to the Present*. John Wiley & Sons.

Klein, U. and Lefèvre, W. (2007). *Materials in eighteenth-century science: a historical ontology.* Cambridge, MA: MIT Press.

Klempe, S.H. (2021). “The Importance of Leibniz for Wundt.” *Hu Arenas* 4, 20–31.

Klinge, M. (2004). “Teachers.” In W. Rüegg (Ed.), *A History of the University in Europe: Volume 3, Universities in the Nineteenth and Early Twentieth Centuries (1800–1945)* (pp. 123-160). Cambridge University Press.

Klockgether-Radke, A. P. (2002). "F. W. Sertürner und die Entdeckung des Morphins – 200 Jahre Schmerztherapie mit Opioiden -." *Ains · Anästhesiologie · Intensivmedizin · Notfallmedizin · Schmerztherapie Anasthesiol Intensivmed Notfallmed* 37.5: 244-49.

Knabe, P.-E. (1978). “Die Rezeption der französischen Aufklärung in den «Göttingischen Gelehrten Anzeigen» (1739-1779)”, Vittorio Klostermann, Frankfurt am Main.

Knight, R.J.B. (1986). "New England Forests and British Seapower: Albion Revised", *American Neptune*, 46, 221-9

Koehler, P. (2012). “Eduard Hitzig's Experiences in the Franco-Prussian War (1870–1871): The Case of Joseph Masseau.” *Journal of the History of the Neurosciences*, 21:3, 250-262.

Kölliker, A. (1852). *Mikroskopische Anatomie, oder, Gewebelehre des Menschen. v. 2, 1852-54, Specielle Gewebelehre*. Leipzig, Verlag von Wilhelm Engelmann.

Kölliker, A. (1859). *Handbuch der Gewebelehre des Menschen*. Leipzig, Verlag von Wilhelm Engelmann.

Kölliker, A. (1880). *Grundriss der Entwicklungsgeschichte des Menschen und Der höheren Thiere*. Leipzig, Verlag von Wilhelm Engelmann

Kölliker, R. A. (1889). *Handbuch der Gewebelehre des Menschens*. Leipzig: W. Engelmann.

Kolmer, W. and Lauber, H. (2013). *Haut Und Sinnesorgane: Auge*. Springer-Verlag.

Königsberger, L. (1906). *Hermann von Helmholtz*. New York: Dover Publications.

Kraepelin, E. (1880*). Die Abschaffung des Strafmasses - Ein Vorschlag zur Reform der heutigen Strafrechtspflege*. Stuttgart: Enke.

Kraepelin, E. (1881a). Brief an Wilhelm Wundt, München, 1. Aug. 1881. Wundtbriefwechsel, https://home.uni-leipzig.de/wundtbriefe/viewer.htm

Kraepelin, E. (1881b). “Ueber den Einfluss acuter Krankheiten auf die Entstehung von Geisteskrankheiten.” *Archiv für Psychiatrie und Nervenkrankheiten*, Bd. 11, 1881, S. 137-183, S. 295-350, S. 649-677, Band 12, 1882, S. 65-121, S. 287-356.

Kraepelin, E. (1881c). “Ueber psychische Zeitmessung.” *Schmidt's Jahrbücher der gesamten Medizin* 196: 205-213.

Kraepelin, E. (1882a). “Ueber die Dauer einfacher psychischer Vorgänge.” *Biologisches Centralblatt*, Bd. 1, 1881/82, S. 654-672, S. 721-733, S. 751-766.

Kraepelin, E. (1883a). “Ueber die Einwirkung einiger medicamentöser Stoffe auf die Dauer einfacher psychicher Vorgänge, Erste Abteilung, Ueber die Einwirkung von Amylnitrit, Aethyläther und Chloroform.” *Philosophische Studien*, 1, 417-462.

Kraepelin, E. (1883b). “Ueber die Einwirkung einiger medicamentöser Stoffe auf die Dauer einfacher psychicher Vorgänge, Zweite Abteilung, Ueber die Einwirkung von Aethylalkohol.” *Philosophische Studien*, 1, 573-605.

Kraepelin, E. (1883c). *Compendium der Psychiatrie: Zum Gebrauche für Studirende und Aerzte*. Leipzig: A. Abel.

Kraepelin, E. (1885). [Review of:] Paul Rée, "Die Entstehung des Gewissens". *Literarisches Centralblatt*. 1697-1698.

Kraepelin, E. (1886a). [Review of:] Paul Rée, "Die Illusion der Willensfreiheit. Ihre Ursachen und ihre Folgen". *Literarisches Centralblatt*. 41.

Kraepelin, E. (1886b). “Schlaflosigkeit und deren Behandlung durch die neueren Schlafmittel.” *Jahresberichte der Gesellschaft für Natur- und Heilkunde in Dresden*, Jg. 1885/86, S. 153-155.

Kraepelin, E. (1887). *Psychiatrie: Ein kurzes Lehrbuch für Studirende und Aerzte*. Zweite, gänzlich umgearbeitete Auflage. Leipzig: Abel.

Kraepelin, E. (1888). “Psychologische Forschungsmethoden.” *Humboldt - Monatsschrift für die gesamten Naturwissenschaften*, 7, 12-14.

Kraepelin, E. (1892a). *Ueber die Beeinflussung einfacher psychischer Vorgänge durch einige Arzneimittel*. Jena, Fischer.

Kraepelin, E. (1892b). "Ueber die centrale Wirkung einiger Arzneimittel." *Archiv für Psychiatrie und Nervenkrankheiten*, Bd. 24, 1892, S. 641-642; *Neurologisches Centralblatt*, Bd. 11, 420-421

Kraepelin, E. (1893). *Psychiatrie: Ein kurzes Lehrbuch für Studirende und Aerzte*. Vierte, vollständig umgearbeitete Auflage. Leipzig: Abel, 1893.

Kraepelin, E. (1894). "Ueber geistige Arbeit." *Neue Heidelberger Jahrbücher*, Jg. 4, Heidelberg, 31-52.

Kraepelin, E. (1895). Der psychologische Versuch in der Psychiatrie. Psychol Arbeiten 1: 1–91.

Kraepelin, E. (1899). "Neuere Untersuchungen über die psychischen Wirkungen des Alkohols." *Internationale Monatsschrift zur Bekämpfung der Trinksitten*, Jg. 9, 321-332; *Münchener Medicinische Wochenschrift*, Jg. 46,1365-1369.

Kraepelin, E. (1903). *Ueber Geistige Arbeit*. Zweite. Jena: Gustav Fischer.

Kraepelin, E. (1906a). *Über Sprachstörungen im Traume*. Engelmann Verlag, Leipzig[vgl. 1910].

Kraepelin, E. (1906b). "Das Verbrechen als soziale Krankheit." *Monatsschrift für Kriminalpsychologie und Strafrechtsreform*, Bd. 3. 257-279.

Kraepelin, E. (1906c). "Der Alkoholismus in München." *Münchener Medizinische Wochenschrift*, Jg. 53, 737-741.

Kraepelin, E. (1983). *Lebenserinnerungen* (Hippius, H., Peters, G. & Ploog, D. Ed.). Berlin, Heidelberg: Springer Berlin Heidelberg Imprint Springer.

Kraepelin, E. and Hoch, A. (1896). "Ueber die Wirkung der Theebestandtheile auf körperliche und geistige Arbeit." [Emil Kraepelin (Hg.):] *Psychologische Arbeiten*, Bd. 1, 378-488.

Kraepelin, E. and Oseretzkowsky, A. (1901). "Ueber die Beeinflussung der Muskelleistung durch verschiedene Arbeitsbedingungen." [Emil Kraepelin (Hg.):] *Psychologische Arbeiten*, Bd. 3, 1901, 587-690.

Kremers, E. and Sonnedecker, G. (1986). *Kremers and Urdang's History of Pharmacy*. Madison, WI: Amer. Inst. History of Pharmacy.

Kuhn, T. (2012). *The Structure of Scientific Revolutions: 50th Anniversary Edition*, foreword by Ian Hacking. United Kingdom: University of Chicago Press.

Lampl, H. (1987). "Ex Oblivione: Das Féré-Palimpseste: Noten zur Beziehung Friedrich Nietzsche-Charles Féré (1857–1907)." In: *Nietzsche-Studien* 15. 225 – 264.

Laudan, L. (1981). "The Sources of Modern Methodology: Two Models of Change." In *Science and Hypothesis* (pp. 6–19). Springer Netherlands.

Lazarsfeld, P. F., & Oberschall, A. R. (1965). "Max Weber and Empirical Social Research." *American Sociological Review*, 30(2), 185–199.

Lehrer, R. (2015). "Nietzsche and Psychology." In: *Nietzsche and Depth Psychology*, Ed. J. Golomb, R. Lehrer, W. Santaniello. SUNY. 149-169.

Lenoir, T. (1982). *The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology*. Dordrecht and Boston: D. Reidel.

Lentacker, A. (2019). "The Codex Nationalized: Naming People and Things in the Wake of a Revolution." In: *Drugs on the Page: Pharmacopoeias and Healing Knowledge in the Early Modern Atlantic World*, Ed, M. Crawford and J. Gabriel. United States: University of Pittsburgh Press, 222-240.

Lesch, J. (1981). "Conceptual Change in an Empirical Science: The Discovery of the First Alkaloids*." Historical Studies in the Physical Sciences* 11, no. 2: 305-328.

Levinger, M. (1998). “Kant and the Origins of Prussian Constitutionalism.” *History of Political Thought*, 19(2), 241-263.

Levinstein, E. (1877). Die Morphiumsucht; eine Monographie nach eignen Beobachtungen. Hirschwald, Berlin.

Lewontin, R. C. (2003). *Biology as Ideology.* Toronto, Anansi.

Lichtenfels, R. and Fröhlich, R. (1851). “Ueber den Puls als ein Symptom, sowie als numerisches Maass der physiologischen Arzeneiwirkung,” *Sitzungsber. d. Wein*, VI 824–849.

Liebig, F. (1842). *Die organische Chemie in ihrer Anwendung auf Physiologie und Pathologie*. Braunschweig, Vieweg und Sohn.

Liebig, J. (1831). “Ueber einen neuen Apparat zur Analyse organischer Körper, und über die Zusammensetzung einiger organischen Substanzen.” *Ann. Phys.*, 97: 1-43.

Liebig, J. (1832). "Ueber die Verbindungen, welche durch Einwirkung des Chlors auf Alkohol, Aether, ölbildendes Gas und Essiggeist entstehen." *Annalen der Chemie* 1: 182-230.

Liebig, J. (1832*). Annalen der Pharmacie. Band* I. Lemgo and Heidelberg, Meyerschen Hof-Buchandlung and Winterschon Universitäts-Buchhandlung.

Liebig, J. v. (1843). *Die Thier-Chemie oder die organische Chemie in ihrer Anwendung auf Physiologie und Pathologie*. Braunschweig, Vieweg und Sohn.

Liebig, J. v. (1874). *Reden und Abhandlungen*. Leipzig, Heidelburg.

Liebig, J. v., and Wöhler, F. (1838). “Untersuchungen über die Nature der Harnsäure.” *Annalen der Pharmacie*. XXVI. 241-336.

Liebreich, O. (1869). *Das Chloralhydrat: ein neues Hypnoticum und Anaestheticum und dessen Anwendung in der Medicin; eine Arzneimittel-Untersuchung*. Berlin: Müller.

Liebscher, M. (2014). "Nietzsche und die Psychologie." In: *Handbuch Nietzsche und die Wissenschaft*, ed. H Heit, L. Heller. De Gruyter, Berlin. 362-378.

Link, S. (1994). "Rediscovering the Past: Gustav Fechner and Signal Detection Theory." *Psychological Science*, 5(6), 335-340.

Link, S., Murray, D., & Ross, H. (2009). "The wide impact of Gustav Fechner's thought-provoking writings." *The American Journal of Psychology*, 122(3), 405-410.

Lipman, T. (1967). "Vitalism and Reductionism in Liebig's Physiological Thought." *Isis*, 58(2), 167-185.

Lohff, B. (1978). "Johannes Müllers Rezeption der Zellenlehre in seinem 'Handbuch der Physiologie des Menschen.'" *Medizinhistorisches Journal* 13: 247-58.

López-Muñoz, F., Boya, J., & Alamo, C. (2006). "Neuron theory, the cornerstone of neuroscience, on the centenary of the Nobel Prize award to Santiago Ramón y Cajal." *Brain Research Bulletin*, 70(4–6), 391–405.

Louis, E. D. (2002). "Erb and Westphal: Simultaneous Discovery of the Deep Tendon Reflexes." *Seminars in Neurology*, 22(4), 385–390.

Löw, R. (1977). *Pfianzenchemie zwischen Lavoisier und Liebig*. Straubing and Munich.

Ludlow, L and Alvarez-Salvat, R. (2000). "Fechner: The Man in the Mask." *Popular Measurement,* 5-6.

Lytle, A. (1906). "Materia Medica, Pharmacy, Therapeutics." *The American Journal of Nursing*, 6(4), 217-224.

Macht, D. I., & Isaacs, S. (1917). "Action of some opium alkaloids on the psychological reaction time." *Psychobiology*, 1(1), 19–32.

Maehle, A. (1995). “Pharmacological experimentation with opium in the eighteenth century.” In R. Porter & M. Teich (Authors), *Drugs and Narcotics in History* (pp. 52-76). Cambridge: Cambridge University Press.

Maehle, A. (1999). *Drugs on trial: experimental pharmacology and therapeutic innovation in the eighteenth century*. Amsterdam: Rodopi.

Magner, L. (2002). *A history of the life sciences*. Boca Raton: CRC Press.

Marriott, J. (2010). *Pharmaceutical compounding and dispensing*. London: Pharmaceutical Press.

Marx, O. (1970). “Nineteenth-Century Medical Psychology: Theoretical Problems in the Work of Griesinger, Meynert, and Wernicke.” *Isis*, 61(3), 355-370.

Marx, O. (1990). “German romantic psychiatry: Part 1.” *History of Psychiatry*, 1(4), 351–380.

McNeely, I. (2002). "Medicine on a grand scale". London: Wellcome Trust Centre for the History of Medicine at UCL.

Meischner-Metge, A. (2010). “Gustav Theodor Fechner: Life and work in the mirror of his diary.” *History of Psychology*, 13(4), 411–423.

Meltzer, S. (1897). “Emil Du Bois-Reymond.” *Science*, 5(110), 217-219.

Melville, H. (1892). *Moby Dick*. United Kingdom: Dana Estes & Company.

Mendel, E. (1878). “Ueber den Verlauf der Fasern des Bindearms.” Paper presented at the Berliner medicinisch-psychologische Gesellschaft, 7 January 1878. Abstract in: *Berliner Klinische Wochenschrift*, 15, 402–403.

Mendelsohn, E. (1965). "Physical Models and Physiological COncepts: Explanation in Nineteenth Century Biology." *British Journal for the History of Science*., 2:203.

Merkel, F. (1875). “Tastzellen und Tastkörperchen bei den Hausthieren und beim Menschen.” *Archiv für mikroskopische Anatomie*, 11: 636-652.

Meulders, M. (2010). "5: Helmholtz and the Understanding of Nature". In Laurence Garey (ed.). *Helmholtz: From Enlightenment to Neuroscience*. MIT Press.

Meynert, T. (1865). "Bau und Funktion des Gehirns und Rückenmarks mit Beziehung auf deren Erkrankungen.” Habilitation.

Meynert, T. (1867). “Der Bau der Gross-Hirnrinde und seine örtlichen Verschiedenheiten, nebst einem pathologisch-anatomischen Corollarium, I.” *Vierteljahrschrift für Psychiatrie*, 1 (77–93), 198-217.

Meynert, T. (1872). “Vom Gehirne der Säugethiere.” In: *Handbuch der Lehre von den Geweben des Menschen und der Thiere, Zweiter Teil*. Leipzig, Verlag von Wilhelm Engelmann, 694-808.

Meynert, T. (1882). “Ueber die Gefühle.” *Jahrb. Psychiat*. 3: 165–76.

Meynert, T. (1884). *Psychiatrie: Klinik der Erkrankungen des Vorderhirns begründet auf dessen Bau, Leistungen und Ernährung*. Austria: Braumüller.

Miller, J. A., Sabshin, M., Gedo, J. E., Pollock, G. H., Sadow, L., & Schlessinger, N. (1969). “Some Aspects of Charcot’s Influence on Freud.” *Journal of the American Psychoanalytic Association*, 17(2), 608–623.

Miller, R. and Dennison, J. (2015). *An outline of psychiatry in clinical lectures: The lectures of Carl Wernicke*. Springer.

Millett, D. (1998). “Illustrating a Revolution: An Unrecognized Contribution to the 'Golden Era' of Cerebral Localization.” *Notes and Records of the Royal Society of London*, 52(2), 283-305.

Monakow, C. von. (1970). *Mein Leben—vita mea.* (A.W. Gulser, & Erwin H. Ackerknecht, Eds.). Bern, Stuttgart, & Wien: Huber.

Monro, A. (1771). *Essays and Observations, Vol. 3*, Edinburgh.

Moon, J. (2021). "The Circle of Life: Christopher Wren and the First Intravenous Anesthetic." *Anesthesiology*, 135, 530.

Moran, B.T. (1999). [Review:] "Leben, Arbeit und Umwelt des Arztes: Johann Daniel Major (1634-1693), Eine Biographie aus dem 17. Jahrhundert, mit neuen Erkenntnissen." *Bulletin of the History of Medicine* 73(4), 701-702.

Morrison, R. (1997). "Opium-Eaters and Magazine Wars: De Quincey and Coleridge in 1821." *Victorian Periodicals Review*, Vol. 30, No. 1. 27-40.

Mullen, P. (1977). "The Romantic as Scientist: Lorenz Oken." *Studies in Romanticism*, 16(3), 381-399.

Müller, J. (1826). *Zur vergleichenden Physiologie des Gesichtssinnes des Menschen und der Thiere, Einleitung*. Leipzig.

Müller, J. (2006). *Der Deutsche Bund 1815–1866*. München: Oldenbourg Verlag, p. 46–47

Müller, J. P. (1834). *Handbuch der Physiologie des Menschen, für Vorlesungen, Band 1*. Coblenz, Hölscher.

Müller, J. P. (1837). *Handbuch der Physiologie des Menschen, für Vorlesungen, Band 2*. Coblenz, Hölscher.

Müller, J. P. (1840). *Handbuch der Physiologie des Menschen, für Vorlesungen, Band 2*. Coblenz, Hölscher.

Ndubaku, U., & de Bellard, M. E. (2008). "Glial cells: old cells with new twists." *Acta histochemica*, 110(3), 182–195.

Neubauer, J. (1967). “Dr. John Brown 1735-88) and Early German Romanticism*.” Journal of the History of Ideas*, 28(3), 367-382.

Niemann, A. (1860). "Über eine neue organische Base in den Cocablättern." *Arch. Pharm. Band* 153, Heft 2 u. 3, 129–155, 291–308.

Nietzsche, F. (1975). *Digitale Kritische Gesamtausgabe Werke und Briefe* (P. D’Iorio, Ed.). Digitization of: Nietzsche Werke. Kritische Gesamtausgabe, Berlin/New York, de Gruyter, 1967- and Nietzsche Briefwechsel. Kritische Gesamtausgabe, Berlin/New York, de Gruyter, 1975 (G. Colli and M. Montinari, Ed). http://www.nietzschesource.org/.

Nordbruch, Claus H. R. (1996). *Über die Pflicht: Eine Analyse des Werkes von Siegfried Lenz*. Hildesheim.

Novalis. & Higgins, D. (1800/1988). *Hymns to the night*. Kingston, N.Y: McPherson

Oken, L. (1805). *Die Zeugung*. Goebhardt, Würzburg.

Oken, L. (1810). *Lehrbuch der Naturphilosophie. Dritter Teil, erstes and zweites Stück*. Friedrich Frommann, Jena.

Oken, L. (1843). *Lehrbuch der Naturphilosophie*. F. Schulthess.

Oken, L., Bach, T., Breidbach, O., & Engelhardt, D. (2007). *Gesammelte Werke*. Weimar: Verlag Hermann Böhlaus Nachfolger.

Ostrander, G. (1956). “The Colonial Molasses Trade.” *Agricultural History*, 30(2), 77-84.

Otis, L. (2007). *Müller’s Lab*. Oxford Press, New York.

Panhuysen, G. (1998). “The Relationship Between Somatic and Psychic Processes: Lessons from Freud’s Project.” *Annals of the New York Academy of Sciences*, 843(1 Neuroscience), 20–42.

Pantalony, D. (2009). “Hermann von Helmholtz and the Sensations of Tone.” In: *Altered Sensations. Archimedes (New Studies in the History of Science and Technology*), vol 24. Springer, Dordrecht.

Paracelsus. (1538/1965). “Die dritte Defension wegen des Schreibens der neuen Rezepte, Septem Defensiones 1538.” *Werke Bd. 2*, Darmstad.

Partington, J.R. (1962). “Chemistry in Germany.” In: *A History of Chemistry*. Palgrave, London.

Patil, P. (2012). *Discoveries in Pharmacological Sciences*. Singapore: World Scientific.

Paul, H. W. (2001). *Bacchic medicine: Wine and alcohol therapies from Napoleon to the French paradox*. Amsterdam: Rodopi.

Pearce, J. (2005). "Romberg and His Sign". *European Neurology* 53, 210-213.

Pemberton, H. (1746). *Pharmacopoeia Londinensis,* Dispensary of the Royal College of Physicians, London.

Pendergrast, M. (2010). *Uncommon Grounds: The History of Coffee and How It Transformed Our World.* United States: Basic Books.

Petty, R. (2019). “Pain-Killer: A 19th Century Global Patent Medicine and the Beginnings of Modern Brand Marketing.” *Journal of Macromarketing*, Vol 39, 3. 287-303.

Phillipson, J. (2012). *Chemistry and Biology of Isoquinoline Alkaloids*. Springer-Verlag Berlin.

Pichot, P. (2013). *Psychiatry The State of the Art: Volume 8 History of Psychiatry, National Schools, Education, and Transcultural Psychiatry*. Springer.

Pillmann, F., & Marneros, A. (2001). “Carl Wernicke: Wirkung und Nachwirkung. Fortschritte Der Neurologie.” *Psychiatrie*, 69(10), 488–494

Pincus, S. (2012). “Rethinking Mercantilism: Political Economy, the British Empire, and the Atlantic World in the Seventeenth and Eighteenth Centuries.” *The William and Mary Quarterly*, 69(1), 3-34.

Plitt, G.L. (1870). “Aus Schellings Leben.” In *Briefen. Erster Band*, *Zweiter Band*. Leipzig.

Porter, R. (1999). *The Greatest Benefit to Mankind: a Medical History of Humanity*. W.W. Norton.

Porter, T. M. (2010). *Karl Pearson: The Scientific Life in a Statistical Age*. Princeton University Press.

Powell, T. (1988). “Kant's Fourth Paralogism.” *Philosophy and Phenomenological Research*, vol. 48, no. 3, pp. 389–414.

Presner, T. (2007). *Mobile Modernity: Germans, Jews, Trains*. New York: Columbia University Press,.

Rachlin, H. (2005). “What Müller's Law of Specific Nerve Energies Says about the Mind.” *Behavior and Philosophy*, 33, 41-54.

Radkau, J. (2009). *Max Weber: A Biography*. Polity Press.

Redwood, T. (1847). *Gray's Supplement to the Pharmacopoeia (rewrite and expansion on original text by S. Gray).* London, Longman; Simpkin; Sherwoord; John Churchill; Henry Bohn; and Renshaw.

Reicheneder, J. (1988). “Cocaineuphorie und Naturwissenschaft.” *Sudhoffs Archiv* 72, 2. 170-184.

Reinbacher, R. (1998). *Leben, Arbeit und Umwelt des Arztes Johann Daniel Major (1634-1693): eine Biographie aus dem 17. Jahrhundert, mit neuen Erkenntnissen*. Linsengericht, Kroeber.

Rheinberger, H.-J. (1997). *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube*. Stanford University Press.

Rheinberger, H.-J. (2005). “Reassessing the Historical Epistemology of Georges Canguilhem.” In: *Continental Philosophy of Science* (Ed. G. Gutting). Blackwell Publishing.

Risse, G. (1976). “Schelling, "Naturphilosophie" and John Brown’s System of Medicine.” *Bulletin of the History of Medicine*, 50(3), 321-334.

Risse, G. (2003). *Medicine in Society: Historical Essays*. Edited by Andrew Wear, Cambridge University Press.

Ritter, J. (1798). *Beweis, daß ein beständiger Galvanismus den Lebensprozeß in dem Thierreich begleite*. Weimar, Verlage des Industrie-Comptoirs.

Ritvo, L. (1990). *Darwin's influence on Freud: A tale of two sciences*. New Haven: Yale University Press.

Roberto, M. (2007). "Ørsted, Ritter and magnetochemistry", in: *Hans Christian Ørsted and the Romantic Legacy in Science: Ideas, Disciplines, Practices*, eds. R.M. Brain, R. S. Cohen & O. Knudsen (*Boston Studies in the Philosophy of Science*, vol. 241), New York: Springer, pp. 339-385.

Robinson, D. (2001). “Reaction-time Experiments in Wundt’s Institute and Beyond,” In: (ed. R. Rieber and D. Robinson): *Wilhelm Wundt in History: The Making of a Scientific Psychology.* New York: Springer Science.

Rocca, J. (2007). “William Cullen (1710–1790) and Robert Whytt (1714–1766) on the Nervous System” 85-98. In: Whitaker, H. A., Smith, C., & Finger, S. (Eds.). *Brain, mind and medicine: Essays in eighteenth-century neuroscience* (2007th ed.). New York, NY: Springer.

Rocke, A. (1993). *The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistry*. UC Press: Berkeley.

Roeckelein, J. E. (1998). *Dictionary of theories, laws, and concepts in psychology*. Westport, Conn: Greenwood Press.

Rogers, M. R. (2003). *Newtonianism for the ladies and other uneducated souls*. New York (N.Y.) Lang.

Rokitanskys-Tilscher, U. (2018). "Im fruchtbaren Spannungsfeld von Natur- und Geisteswissenschaften--eine erste Studie zur Prägung der Persönlichkeit Carl Freiherr von Rokitanskys aus familienbiografischer Sicht." In D. C. Angetter and P. Weindling (Ed.), *Strukturen und Netzwerke: Medizin und Wissenschaft in Wien 1848–1955* (pp. 331-362). V&R Unipress.

Röschlaub, A. (1798). *Untersuchungen über Pathogenie oder Einleitung in die medizinische Theorie, Band I*. Andreä.

Röschlaub, A. (1799). Untitled ("An J. G. Fichte"). *Magazin zur Vervollkommnung der theoretischen und praktischen Heilkunde*. 1st edition, Band 2. xi-xvi.

Roth, H. L. (2002). "Finding Language in the Matter of the Brain: Origins of the Clinical Aphasia Examination." *Seminars in Neurology*, 22(4), 335–348.

Rothschuh, K. (1973). *History of Physiology*, edited and translated by Guenter B. Risse. Huntington, NY: Robert E. Krieger.

Rüegg, W. (2004). "Themes." In: *A History of the University in Europe: Volume 3, Universities in the Nineteenth and Early Twentieth Centuries (1800–1945)* (W. Rüegg (Ed.) Cambridge University Press. 3-25.

Rumore, P. (2014). “Kant's Understanding of the Enlightenment with Reference to his Refutation of Materialism”. *Inicio*, 1.

Rynd, F. (1845). “Neuralgia - introduction of fluid to the nerve.” *Dublin Med Press*. 13:167-168.

Sarikcioglu, L., & Arican, R. Y. (2006). “Wilhelm Heinrich Erb (1840-1921) and his contributions to neuroscience.” *Journal of Neurology, Neurosurgery & Psychiatry*, 78(7), 732–732.

Schelling, F. W. (1798). “Von der Weltseele.” In: *Sämtliche Werke von F. W. Schelling 1797-1798, Zweiter Band*. 1857, Stuttgart and Augsburg. Gottascher Verlag. 345-583.

Schelling, F. W. (1799a). “Erster Entwurf eines Systems der Naturphilosophie.” In: *Sämtliche Werke von F. W. Schelling 1799-1800, Dritter Band.* 1858, Stuttgart and Augsburg. Gottascher Verlag. 1-269.

Schelling, F. W. (1799b). “Einige Bemerkungen aus Gelegenheit einer Rezension Brown'scher Schriften in der A. L. Z.”. *Magazin zur Vervollkommnung der theoretischen und praktischen Heilkunde*. 1st Edition, Band 2. 255-262.

Scherf, C. (1799). *Dispensatorium Lippiacum, genio moderno accommodatum auctoritate Collegii Medici*. Vol II, 2nd edition.

Schiebinger, L. (2003). “Human Experimentation in the Eighteenth Century: Natural Boundaries and Valid testing.” *The Moral Authority of Nature*. Chicago: University of Chicago Press, pp. 384-408.

Schiffter, R. (2007). “Lebensbilder von Romberg.” *Fortschr Neurol Psychiatr* 75:160–167

Schleich, C. L. (1936). *Those Were Good Days* (B. Miall, trans.). University of Minnesota Press.

Schmitz, R. (1985). “Friedrich Wilhelm Sertürner and the Discovery of Morphine.” *Pharmacy in History*, vol. 27, no. 2, pp. 61–74.

Schultz, M. (2008). “Rudolf Virchow.” *Emerging Infectious Diseases*, 14(9), 1480–1481.

Scott, H. M. (1990). *Enlightened absolutism: Reform and reformers in later eighteenth-century Europe.* Houndmills, Basingstoke, Hampshire: Macmillan.

Seebacher, F. (2006). “The Case of Ernst Wilhelm Brücke versus Joseph Hyrtl –The Viennese Medical School Quarrel Concerning Scientific and Political Traditions.” In: Brigitte Hoppe (ed.), *Controversies and Disputes in Life Sciences in the 19th and 20th Centuries. Algorismus: Studien zur Geschichte der Mathematik und der Naturwissenschaften* 56, pp. 35–54.

Seitelberger, F. (1997). "Theodor Meynert (1833-1892), pioneer and visionary of brain research." *Journal of the History of the Neurosciences* 6(3):264-274.

Sertürner, F. (1805). “Säure im Opium.” *Journal der Pharmacie*, xiv, 33-57.

Sertürner, F. (1806) "Darstellung der reinen Mohnsäure (Opiumsäure) nebst einer chemischen Untersuchung des Opiums mit vorzüglicher Hinsicht auf einen darin neu entdeckten Stoff und die dahin gehörigen Bemerkungen", in: *Journal der Pharmacie*, volume XIV, pp.47-93. Leipzig: S.L. Crusins,

Sertürner, F. (1811). "Ueber das Opium und dessen krystallisirbare Substanz", in: *Journal der Pharmacie*, volume XX, pp.99-103. Leipzig F.C.W. Vogel.

Sertürner, F. (1817). “Ueber das Morphium, eine neue salzfähige Grundlage, und die Mekonsäure, als Hauptbestandtheile des Opiums.” *Annalen der Physik*, 55(1), 56–92.

Seyfarth, E.-A. (2006). “Julius Bernstein (1839-1917): pioneer neurobiologist and biophysicist.” *Biol Cybern* 94: 2-8.

Sheehan, J. (1971). “Liberalism and the City in Nineteenth-Century Germany.” *Past & Present*, (51), 116-137.

Shell, S. M. (2013). “Kant as “vitalist”: the “principium of life” in Anthropologie Friedländer.” In A. Cohen (Ed.), *Kant’s Lectures on Anthropology* (pp. 151–171). Cambridge University Press.

Shepherd, GM. (1991). *Foundations of the neuron doctrine*. Oxford University Press.

Shorter, E. (1997). *A History of Psychiatry*. New York: John Wiley & Sons.

Shrady, G. and Stedman, T. (1885). *Medical Record*. Vol. 28. Wood.

Siemerling, E. (1890). “Dr. Carl Westphal.” *Journal of Mental Science*, 36(153), 312-313.

Simpson, D. (2005). “Phrenology and the Neurosciences: Contributions of F. J. Gall and J. G. Spurzheim.” *ANZ Journal of Surgery*, 75(6), 475–482.

Sloan, P. (1986). “Darwin, Vital Matter, and the Transformism of Species.” *Journal of the History of Biology*, 19(3), 369-445.

Sloane, H. (1721). *Pharmacopoeia Londinensis, 4th edition*.

Smith, D. (2008). “Hail Mariani: The Transformation of Vin Mariani From Medicine to Food in American Culture, 1886-1910.” *Social History of Alcohol and Drugs*, Vol. 23, 1. 42-57.

Snelders, H. (1970). “Romanticism and Naturphilosophie and the Inorganic Natural Sciences 1797-1840: An Introductory Survey.” *Studies in Romanticism*, 9(3), 193-215.

Snelders, H. (1983). “Sertürner: opium en 'Naturphilosophie'.” Bu*ll Cercle Benelux Hist Pharm.* Sep;(65):1-4.

Snelders, S., Kaplan, C., & Pieters, T. (2006). “On Cannabis, Chloral Hydrate, and Career Cycles of Psychotropic Drugs in Medicine.” *Bulletin of the History of Medicine*, 80(1), 95-114.

Springer, A. (2002). “Kokain, Freud und die Psychoanalyse.” *Suchttherapie* 3 (1), 18-23.

Squire, L. R. (2003). *Fundamental neuroscience*. Amsterdam: Academic Press.

Steinberg H., Müller U. (2005). “Emil Kraepelin 1882/83 in Leipzig und seine frühen pharmakopsychologischen Arbeiten im Licht der aktuellen Forschung.” In: Angermeyer M.C., Steinberg H. (eds) *200 Jahre Psychiatrie an der Universität Leipzig*. Springer, Berlin, Heidelberg.

Steinberg, H., & Angermeyer, M. C. (2001). “Emil Kraepelin's years at Dorpat as professor of psychiatry in nineteenth-century Russia.” *History of psychiatry*, 12(47 Pt 3), 297–327.

Steinberg, H., Himmerich, H. (2013). “Emil Kraepelin's habilitation and his thesis: a pioneer work for modern systematic reviews, psychoimmunological research and categories of psychiatric diseases.” *The world journal of biological psychiatry : the official journal of the World Federation of Societies of Biological Psychiatry*, 14(4), 248–257.

Steinkamp, F. (2002). “Schelling's "Clara": Editors' Obscurity.” *The Journal of English and Germanic Philology*, 101(4), 478-496.

Steudel, J. (1963). *Le Physiologiste Johannes Müller*. Paris: Université de Paris.

Strickland, S. (1998). “The ideology of self-knowledge and the practice of self-experimentation.” *Eighteenth-Century Studies*, vol. 31, no. 4, 453-471.

Strohl, J. (1935). “Okens Stellung zu Paracelsus und zur Geschichte der Naturwissenschaften überhaupt.” In: *Verhandlungen der Schweizerischen Gesellschaft für Geschichte der Medizin und der Naturwissenschaften*. Jg. 116, S. 395–397.

Stuckrad, K. (2014). *The Scientification of Religion: An Historical Study of Discursive Change, 1800-2000.* Boston: De Gruyter.

Sturm, T. (2009). “Die Kritik an der empirischen Psychologie.” In: *Kant und die Wissenschaften vom Menschen.* Brill. 183–260.

Tannahill, R. (1973). *Food in History*. Stein and Day.

Tasaki, I. (2014). *Physiology and Electrochemistry of Nerve Fibres*. Saint Louis: Elsevier Science.

Tattersall, I. (1995). *The fossil trail: How we know what we think we know about human evolution.* New York: Oxford University Press.

Timmermann, J. (2006). "Kantian Duties to the Self, Explained and Defended." *Philosophy,* 81(3), 505-530.

Titchener, E. (1921). "Wilhelm Wundt." *The American Journal of Psychology*, 32(2), 161-178.

Trede, K. (2007). "150 years of Freud-Kraepelin dualism." *The Psychiatric quarterly*, 78(3), 237–240.

Tsouyopoulos, N. (1982). *Andreas Röschlaub und die romantische Medizin: die philosophischen Grundlagen der modernen Medizin*. Stuttgart: G. Fischer.

Tsouyopoulos, N. (1988). "The Influence of John Brown's Ideas in Germany". *Medical History. Supplement* No. 8: 63–74.

Tsouyopoulos, N. (1990). "Doctors contra clysters and feudalism: the consequences of a Romantic Revolution Romanticism and the Sciences." In A. Cunningham (Ed.) *Romanticism and the Sciences* (p 101-119). Cambridge University Press.

Turner, R. (1993). "Vision Studies in Germany: Helmholtz versus Hering." *Osiris*, 8, 80-103.

Urdang, G. (1946). "Pharmacopoeias as Witnesses of World History*." Journal of the History of Medicine and Allied Sciences*,1(1), 46-70

Van den Berg, H. (2009). "Kant's conception of proper science." *Synthese*, 183(1), 7–26.

Van Wyhe, J. (2002). "The Authority of Human Nature: The "Schädellehre" of Franz Joseph Gall." *The British Journal for the History of Science*, 35(1), 17- 42.

Van Wyhe, J. (2002). The Authority of Human Nature: The "Schädellehre" of Franz Joseph Gall. The British Journal for the History of Science, 35(1), 17-42.

Vanzo A. (2012). “Kant on Experiment.” In: Maclaurin J. (eds) *Rationis Defensor. Studies in History and Philosophy of Science*, vol 28. Springer, Dordrecht. 75-96.

Vartanian, A. (1983). “La Mettrie and Diderot Revisited: An Intertextual Encounter.” *Diderot Studies*, 21, 155-197.

Verplaetse, J. (2009). *Localizing the moral sense: Neuroscience and the search for the cerebral seat of morality, 1800-1930*. Dordrecht: Springer.

Vickers, N. (1997). “Coleridge, Thomas Beddoes and Brunonian medicine.” *European Romantic Review*, 8:1, 47-94.

Vigilantius, J. (1997). “Part IV - Kant on the metaphysics of morals: Vigilantius's lecture notes.” In: *Lectures on Ethics* (Ed. P. Heath and J. Schneewind). Cambridge University Press. 249-452.

Vintschgau, M. v., Hönigschmied, J. (1875). “Versuche über die Reactionszeit einer Geschmacksempfindung.” *Pflüger Archiv*, 10. 1-48.

Virchow, R. (1848). *Mittheilungen über die in Oberschlesien herrschende Typhus-Epidemie*. Berlin, Verlag von G. Reimer.

Virchow, R. (1858). *Die Cellularpathologie in ihrer Begründung auf physiologische und pathologische Gewebelehre*. Berlin, Verlag von August Hirschwald.

Virchow, R. (1860). *Handbuch der speciellen Pathologie und Therapie*. Erlangen, Verlag von Ferdinand Enke.

Vogt, K. (1855). *Köhlergluabe und Wissenschaft*, Gießen: Ricker.

Volhard, J. (1898). *Justus v. Liebig: Vortrag in der Hauptversammlung des Vereins deutscher Chemiker*. Springer, Berling and Heidelberg.

vom Scheidt, J. (1973). "Sigmund Freud and cocaine." *Psyche: Zeitschrift für Psychoanalyse und ihre Anwendungen*, 27(5), 385–430.

von Engelhardt, D. (1994). "Paracelsus im Urteil der Naturforschung und Medizin der Romantik." *NTM* N.S. 2, 97–116.

Vucinich, A. (1988). *Darwin in Russian Thought.* Berkeley: University of California Press.

Wackenroder, H. (1853). "Chemische Prüfung der Coca-Blätter." *Archiv der Pharmacie* LXXV: 24–27.

Walter, E., & Scott, M. (2017). "The life and work of Rudolf Virchow 1821-1902: Cell theory, thrombosis and the sausage duel." *Journal of the Intensive Care Society*, 18(3), 234–235.

Warren, M. E. (1992). "Max Weber's Nietzschean conception of power." *History of the Human Sciences*, 5(3), 19–37.

Wasianski, E. (1804). *Immanuel Kant in seinen letzten Lebensjahren, Dritter Band*. Nicolovious, Königsberg.

Wassmann, C. (2009). "Physiological optics, cognition and emotion: a novel look at the early work of Wilhelm Wundt." *J Hist Med Allied Sci*. Apr;64(2):213-49

Watson, P. (2010). *The German Genius: Europe's Third Renaissance, the Second Scientific Revolution and the Twentieth Century*. Simon and Schuster.

Weatherby, L. (2016). *Transplanting the Metaphysical Organ: German Romanticism between Leibniz and Marx*. Fordham University Press.

Weber, M. (1917 [1994]). *Wissenschaft als Beruf 1917/1919/ Politik als Beruf, 1919 Studienausgabe*. Germany: J.C.B. Mohr (Paul Siebeck).

Weber, M. (1995). ‘Zur Psychophysik der industriellen Arbeit’, in W. Schluchter with S. Frommer (eds), *Max Weber Gesamtausgabe (Band 11): Zur Psychophysik der industriellen Arbeit. Schriften und Reden, 1908– 1912*. 162–380.

Weber, M. (2016). “Die protestantische Ethik und der Geist des Kapitalismus,” in *Max Weber Gesamtausgabe (Band 18): Die protestantische Ethik und der Geist des Kapitalismus / Die protestantischen Sekten und der Geist des Kapitalismus Schriften 1904–1920*, (ed. H. Baier et al). Tübingen: J.C.B. Mohr. 101-548.

Weber, M. (2018). “Die „Objektivität“ sozialwissenschaftlicher und sozialpolitischer Erkenntnis”, in *Max Weber Gesamtausgabe (Band 7): Zur Logik und Methodik der Sozialwissenschaften Schriften 1900–1907*, (ed. H. Baier et al). J.C.B. Mohr. 135-234.

Weikart, R. (2016). *From Darwin to Hitler: Evolutionary Ethics, Eugenics and Racism in Germany.* Palgrave Macmillan.

Weindling, P. (1993). *Health, Race and German Politics Between National Unification and Nazism, 1870-1945*. Cambridge University Press.

Wellman, K. A. (1992). *La Mettrie: Medicine, Philosophy, and Enlightenment*. Duke University.

Werner, P., & Holmes, F. (2002). “Justus Liebig and the Plant Physiologists.” *Journal of the History of Biology*, 35(3), 421-441.

Wernicke, C. (1874). *Der aphasische Symptomencomplex: Eine psychologische Studie auf anatomischer Basis*. Cohn.

Wernicke, C. (1896). *Grundriss der Psychiatrie in klinischen Vorlesungen*. Thieme.

Wernicke, K. (1900). *Grundriss der Psychiatrie in klinischen Vorlesungen*. Thieme.

Westphal, C. F. O. (1875). “Ueber einige durch mechanische Einwirkung auf Sehnen und Muskeln hervorgebrachte Bewegungs-Erscheinungen (Knie-, Fussphänomen).” *Archiv für Psychiatrie und Nervenkrankheiten*, Berlin, 5: 803–834.

Westphal, C. F. O. (1883). “Über eine dem Bilde der cerebrospinalen grauen Degeneration ähnliche Erkrankung des centralen Nervensystems ohne anatomischen Befund, nebst einigen Bemerkungen über paradoxe Contraction.” *Archiv für Psychiatrie und Nervenkrankheiten*, 14: 87–134.

Wiesing, U. (1989). *Der Tod der Auguste Böhmer. Chronik eines medizinischen Skandals, seine Hintergründe und seine historische Bedeutung [Auguste Böhner's death. Chronicle of a medical scandal, its background and its historical significance*]. “History and philosophy of the life sciences,” 11(2), 275–295.

Wilhelmy, P. (1989). *Der Berliner Salon im 19. Jahrhundert*. Walter de Gruyter.

Winfield, R. (2011). Hegel’s Solution to the Mind-Body Problem. In A Companion to Hegel (pp. 225–242). Wiley-Blackwell.

Wolpert, L. (1995). “Evolution of the Cell Theory.” *Philosophical Transactions: Biological Sciences*, 349(1329), 227-233.

Wontorra, M. (2004). *Wilhelm Wundt (1832-1920) und die Anfänge der experimentellen Psychologie*. Jubiläumsausgabe zur 125-Jahr-Feier seiner Institutsgründung. Universität Leipzig, Institut für Allgemeine Psychologie, Lehrstuhl Prof. Dr. E. Schröger.

Wood, P. (1996) [1974]. *Black Majority: Negroes in Colonial South Carolina from 1670 through the Stono Rebellion*. Norton.

Woycke, J. (1992). “Patent Medicines in Imperial Germany.” *CBMH/BCHM* Vol. 9, 41-56.

Wundt, W. (1857). “Ueber die Elasticität der thierischen Gewebe.” *Heidelberger Jahrbücher der Literatur*, 50 (16), 249-250.

Wundt, W. (1858a). *Die Lehre von der Muskelbewegung. Nach eigenen Untersuchungen bearbeitet*. Friedrich Vieweg und Sohn.

Wundt, W. (1858b). “Ueber das Gesetz der Zuckungen und die Modification der Erregbarkeit durch geschlossene Ketten.” *Archiv für physiologische Heilkunde*, 2, 354-400.

Wundt, W. (1862). *Beiträge zur Theorie der Sinneswahrnehmung*. Winter'sche.

Wundt, W. (1874a). *Grundzüge de physiologischen Psychologie*. Engelmann.

Wundt, W. (1874b). “Ueber die Aufgabe der Philosophie in der Gegenwart.” *Akademische Antrittsrede in Zürich*. Leipzig.

Wundt, W. (1876). *Ueber den Einfluss der Philosophie auf die Erfahrungswissenschaften*. Leipzig.

Wundt, W. (1909). *Grundriss der Psychologie*. 9th ed. Engelmann.

Wundt, W. (2013). *Erlebtes und Erkanntes*. Dogma Publishing.

Yakira, E. (2010). “The Anachronism of Parallelism: A Chapter in Historical Irony.” *Iyyun: The Jerusalem Philosophical Quarterly / 2010, עיון: רבעון פילוסופי, נ"ט*, p 99–114.

Zieger, S. (2005). “How Far am I Responsible?: Women and Morphinomania in Late-Nineteenth-Century Britain.” *Victorian Studies* 48(1), 59-81.

Zimmer, H.-G. (2006). “Profiles in Cardiology Johannes Müller.” *Clin. Cardiol*. 29, 327–328.

Zwirn, S. (2015). “Butterflies of the Soul: Cajal's Neuron Theory and Art.” *The Journal of Aesthetic Education*, 49(4), 105-119.