

Reconsidering “Brain mythology”

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Abstract. This essay attempts to reconstruct the historical origin of the term “brain mythology,” which is used very often today to summarize the status of brain anatomy around 1900. It asks whether this negative term can appropriately encapsulate that status, and tries to show that it came from a special direction of psychiatry that was generally very skeptical about somatic-oriented psychiatry. The reconstruction shows that “brain mythology” was formulated by pupils of Emil Kraepelin. This essay argues that their accusations, culminating in the accusation of “brain mythology,” can be traced back to reasons of principle related to the adoption of Wundt’s “heuristic principle of parallelism,” which Kraepelin incorporated into psychiatry. This principle suggests an independent psychic causality; for that reason alone, a strict localization of mental illness was excluded, and the value of somatic psychiatry was fundamentally questionable. This paper attempts to show that the term originated within the Wundt tradition and was also the result of skepticism about reductionism. This raises the question of whether it would not be better to describe the term, alongside objective criticism, more in the sense of a polemic between different schools of thought.

Keywords: Theodor Meynert, Emil Kraepelin, Karl Jaspers, brain anatomy, brain mythology

The Historical origin of “brain mythology”

Karl Jaspers (1883–1969) was the author of the most famous variant of the accusation of “brain mythology” (1–2), which we can find in his *General Psychopathology* from 1913:

“These anatomical constructions became quite fantastic (e.g. Meynert, Wernicke) and have rightly been called ‘Brain Mythologies.’ Unrelated things were forcibly related, e.g. cortical cells were related to memory, nerve fibres to association of ideas. Such somatic constructions have no real basis. Not one specific psychic cerebral process is known which parallels a specific psychic phenomenon.” (3).

Guenther recently argued that Jaspers “criticized here the apparently direct translation process between psychological and anatomical categories” and a “simple one-to-one translation” as an “uncritical, ‘unreal’ one” (1). I think Guenther, although she had already

emphasized that “Jaspers’s critique was marked by his lager project,” misses the nuances of the debate, because what Jaspers is primarily criticizing here is the assumption of psychophysical parallelism in the sense of Mach’s “neutral monism.” Thus, I think that we can better understand Jaspers’ criticism against the background of his concerns about this principle.

Meynert’s theory is based on psychophysical parallelism, which was widely accepted by scientists at this time (for brain researchers see Edinger (4). He mentions that “the outer shape stands with the inner life like in a secret alliance” (5), and that “whether all essence appear mental or material only depends on the perspective” (6).

It was Mach who formulated the popular principle of a “complete parallelism of the psychical and physical,” which assumes that the quantity or quality of the sensations must have a correspondence in the quantity or quality of the physiological process (7, 8).

Mach gives us the following examples of his principle:

“I have a sensation of space, whether through the sensation of sight or through that of touch, or in any other way, I am obliged to assume the presence of a nerve-process in all cases the same in kind. (...) If I see figures which are the same in size and shape but differently colored, I seek, in connexion with the different color-sensations, certain identical space-sensations with their appurtenant identical nerve-processes. (...) If the seemingly limitless multiplicity of color-sensations is susceptible of being reduced, by psychological analysis (self-observation), to six elements (fundamental sensations), a like simplification may be expected for the system of nerve-processes. If our system of space-sensations appears in the character of a threefold manifoldness, its system of co-ordinated nerve-processes will likewise present itself as such.” (7).

Most likely, based on this principle, Meynert made an identification of sensations with nerve cells: “The inner state of the nerve cell is sensitiveness.” (5). On the other hand, he discovered a fibre connection between the individual nerve cells, which he called, based on the concept of association psychology, an *association-fibre*. Association-fibres connect different areas of the cortex and the “memory images” within each other. They create a “kind of network” (5) with which the individual nerve cells communicate. To do justice to the model of association psychology, he had to explain how memory and association can be realized on the basis of this network.

We can assume that Meynert’s plan was to naturalize mental phenomena in the sense of an identity theory, and in another essay I have argued that it was very close to positivist thought (70-72). It is also worth noting that Meynert helped set the foundation of the Vienna Circle (9). According to contemporary sources, Meynert was also a member of the circle that met at the Viennese inn “The Golden Duck,” (10) which played a special role in the establishment of positivism in Vienna. The importance of his research is also highlighted by Theodor Gomperz (1832–1912) in his autobiography, which provides very important insights into the prehistory of Austrian positivism (11).

F. A. Lange (1828–1875), many years before Oswald Külpe (1862–1915), the reformer of Wilhelm Wundt’s (1832–1920) psychophysical parallelism, who

used Ernst Mach’s (1838–1916) “neutral monism” to develop a psychology without a metaphysical dualism of the mental and the physical (12, 13), demanded from brain research that it show that “thinking is an activity of the brain,” not only intended for “resolving it somehow into elements” but also to demonstrate “in these elements still the correspondence of the physical and the mental, only then will this mode of regarding things be generally adopted, and great weight attributed to it in the formation of our collective theory of things.” (14). In Meynert’s theory, he saw this demand realized in the very first attempts (14). Alois Riehl’s (1844–1924) “monism” demanded a “translation of the statement of subjective experience into terms of the objective,” i.e., the finding “of physical correlates for psychical activities in the external intuition.” (15) By making this assumption, Meynert’s brain theory should have, in fact, succeeded in translating the subjective language of psychical activities into the realm of physiology. With regard to Meynert, we can read Riehl: “The fact that this translation succeeds, that physical correlates for psychical activities are to be found in external intuition, confirms the truth of critical monism.” (15).

Moritz Schlick’s (1882–1936) “two-language theory” represented a variant of psychophysical parallelism that was strongly influenced by Riehl. Later, it was one important source for Herbert Feigl’s “identity theory,” which reintroduced the mind-body problem into the modern debate (16).

For Schlick, the psychical and the physical are only “two different systems of concepts” of the same object, and are not of a metaphysical nature. In fact, this is very similar to Riehl’s conception (17). For the same reason as Riehl, Schlick was convinced that the ultimate goal was a “reduction of psychology to brain physiology” (18). For him, only a so-called *physiological psychology* could enable a theoretically complete knowledge of the mental, something which is impossible with the method of pure introspection. Introspective psychology for Schlick could never go beyond the stage of qualitative knowledge. To reach the level of objective knowledge, we must pass over to physiological psychology:

“With the aid of such a psychology, we find it possible to correlate concepts with the given subjec-

tive qualities just as we are able to correlate them with the inferred objective qualities. The subjective qualities thus become just as knowable as the objective qualities.” (18).

Ultimately, attention should be focused on brain processes in order to be able to think about the implementation of this project.

“Hence in the exact world picture of scientific knowledge, the numerically describable concepts that must be substituted for the subjective qualities are simply certain brain processes. It is to these that the analysis of the mutual dependencies inevitably leads. Even though we are immeasurably far from knowing exactly which individual processes are involved, at least the path is indicated: cerebral processes must be substituted for subjective qualities. This is the only hope we have of fully knowing the subjective qualities. (...) In sum, a definitive knowledge of qualities is possible only through the quantitative method. The life of consciousness is thus completely knowable to the extent that we succeed in transforming introspective psychology into a physiological, natural scientific psychology – ultimately into a physics of brain processes.” (18).

Finally, Schlick also discussed Meynert’s approach but considered it, on the basis of Johannes von Kries’ (1853–1928) and Becher’s critique, to be impracticable, at least because of the state of knowledge at that time (18).

Schlick shared with Kries and Becher the view that the claims of Paul Flechsig (1847–1929) and other researchers were too comprehensive (18). Kries himself had a discussion with Flechsig and accused brain researchers of attributing “psychic phenomena of the highest order”, such as the “formation of a judgement (*Urteilsbildung*)” to the interconnection between individual neurons. He saw this claim not only in Flechsig but also in Ramon Cajal (1852–1934) (19). Cajal, similarly to other brain researchers, attributed higher mental functions to the activity of neural networks. Kries asked if, on this point of view, we ultimately “attribute more to the connecting fibres than we can safely ascribe to them in the present state of knowledge” (19). On the other hand, both Kries and Schlick obviously believed that further research could shed more light onto the issue. Kries indicated this at the end of his book (19–20). Becher also seemed to

share this opinion. He answered a letter from Schlick: “[I] agree (...) with you that in spite of the criticism, a localization of residuals (*Residuen*) and associations (*Assoziationen*) in the physical is in principle not impossible” (21).

Later, for example, Sigmund Freud (1856–1939) criticized the notion that the simplicity and elementarity of psychological elements should correspond to the same simplicity and elementarity on a physiological level. In his opinion, this was an unjustified transfer from psychology to physiology (22). Guenther concluded from this that Freud had gone a step further than Meynert: “Freud criticized the localization of functions because it was not physiological *enough*. He wanted to construct a purely physiological model of the nervous system, which could give meaningful insight into mental processes (and consequently psychology) in a way that was blocked for a simple and introspective psychological account.” (1).

It should, however, be noted that Meynert’s model does represent a plausible solution to Mach’s already mentioned “principle of a complete parallelism of the psychical and physical.” The reproach to Meynert that he was still too much influenced by the concept of association psychology (which for Guenther also is the essence of Jaspers’s idea behind the term “brain mythology”) thus falls short, and cannot be readily accepted.

The advantage of this principle was that by rejecting Fechner’s inner threshold, which was important for Freud’s conception of the unconscious, it became possible to attribute mental phenomena to the corresponding brain processes (8, 23, 24). But with Freud, who himself was a follower of psychophysical parallelism, this is no longer so easy (22).

On the other hand, Guenther supports the thesis of this essay by showing that Freud, in contrast to Jaspers, has not rejected the research program which later was called “brain mythology,” and that he “engaged with and worked through the principles of the neuropsychiatric tradition.” (1). It is interesting that Oswald Bumke (1877–1950) criticized Freud precisely for being close to this tradition (25). Bumke’s evaluation of Freud’s standpoint supports not only Guenther’s thesis, but also the thesis of this paper, because we see again that this approach was generally very skeptical

about an anatomically-oriented psychology, and not only about the model of Meynert.

The term “brain mythology” became famous through Jaspers but is much older. As Bumke reports, Franz Nissl (1860–1919), Emil Kraepelin’s (1856–1926) successor in Munich, used “brain mythology” for the first time with reference to Flechsig’s lecture “Brain and Soul” (1894) (25). The term was coined at the peak of the debate. Later, Mayer (26) and Nissl’s pupils Jaspers (3) and Hans W. Gruhle (1880–1958) (27) also used it.

I argue below that we can find the roots of the discussion behind this term in the negative attitude of Wundt and his followers. They had an ambivalent relationship with brain anatomy, which we should not generalize when discussing this subject. In particular, we have to consider that in principle, they had concerns about reductionism. It was Kraepelin who incorporated Wundt’s “heuristic principle of parallelism” into psychiatry. The principle argues for an independent psychic causality; for that reason alone, a strict localization of mental illness was excluded, and the value of somatic psychiatry is fundamentally questionable (28). I will show in more detail that with respect to the contradiction to his “heuristic principle of parallelism,” Wundt himself already spoke of “figments of fantasy” and “ideas of utopian brain anatomists.” (29).

We also see this development clearly in the basis of the term “brain mythology” which, alongside objective criticism, is a question of background. Thus, it seems that it was a fundamental question – a rejection of psychological reductionism – that culminated in the term “brain mythology”. Basically, as Gundlach recently showed, there were many schools of thought at the time that formulated strong polemics against each other. Polemic expressions against different schools of thought (which, of course, could go hand in hand with justified criticism) were typical, as Gundlach emphasizes (30, 31).

The current literature suggests that “brain mythology” was a common term at the time, which, in fact, does not seem to be the case. Harrington, for example, recently argued that there was a “general consensus” in the early twentieth century to describe the speculative empirical claims of brain researchers with a somatic approach as “brain mythology.” (32). Steinberg (33), a

Flechsig specialist, on the other hand, speaks on a general level about Flechsig’s “famous-infamous (*berühmt berüchtigt*) reputation” among psychologists and clinical-empirical psychiatrists (34, 35).

“Brain mythology” in the first sense was aimed at the attempt to describe mental functions in terms of anatomical discoveries. They were criticized for their “anatomical constructions” (27) respectively because they “left the field of the purely anatomical,” (36) and not for Flechsig’s alarming attempt to legitimize his authoritarian political claims through brain anatomy (37, 38). We should not forget that Flechsig was not the only psychiatrist who wanted to combine his research with “moral implications” (33). Kraepelin, for example, also had a nationalist attitude (39) and warned about the “devastating effect of the mentally ill on the Volkskörper” (40).

Niessl, like his teacher Kraepelin, was generally very skeptical about an anatomically oriented psychology. Brain anatomy has for him basically no value for the understanding of “pure” mental illnesses: “Brain anatomy has (...) nothing to do with psychiatry.” (41). Jaspers wrote his *General Psychopathology* in Munich under Nissl, “in the shadow of Kraepelin,” because the department “was completely Kraepelinian through and through.” (42). Gruhle’s strong connection to experimental psychology has been examined in more detail by Gundlach (30). In fact, Jaspers, like Kraepelin, excluded a strict localization of mental illness and a “parallelism between mental and somatic events.” (3). If we look at his criticism of Meynert, it becomes clear that, in the same way as Wundt, he had to reject it for fundamental reasons: thus in general also scientific advances are not able to solve the problem (43, 44). For Jaspers, Meynert’s psychiatry is based on the assumption that “the structure of the psyche and the structure of the brain must coincide. This postulate has never been proven. It cannot be proven, because it is meaningless. What is heterogeneous cannot coincide, but at best one can be used as a metaphorical expression of the other.” (43).

Finally, it can also be assumed that the strong polemic against these ideas was also in part the result of a personal conflict between Flechsig and Wundt, both of whom were researching at the University of Leipzig. Flechsig pursued and radicalized Meynert’s program.

Flechsig, mainly because of his difficult nature, had many opponents among his colleagues (33, 44–49). The competition between Wundt and Flechsig at this time is well documented in letters by Wundt. As early as 1890, he reports to Kraepelin about his colleague in Leipzig: “Some of my interns from the institute visit Flechsig’s lectures on brain anatomy on a weekly basis and tell me miracles about his psychology, which he occasionally claims, in doing so he decides the most difficult questions with an enviable safety.” (50).

Wundt also later negatively emphasized Flechsig’s attacks on experimental psychology, and the direction of psychiatry of his former pupil Kraepelin, in his Rector’s speech in Leipzig. In a letter to Kraepelin at the beginning of 1895 Wundt, who was most probably in the audience (33), suspected that he “probably read” Flechsig’s “suggestive rector’s speech on brain and soul.” (51). In 1899, Wundt complained to Külpe that he did not know anybody outside Southern Germany, apart from Ziehen, “who should not be indignant at the suggestion that he should deal with anything other than brain anatomy or pathology of the nervous system.” (52). Even more impressive is what follows: “Incidentally, I look with complete peace of mind towards the moment when the Saxon government will place my institute under the direction of my colleague Flechsig. I am curious to see what he does with the instruments.” (52).

This conflict between Wundt and his pupil Kraepelin and Flechsig dates back to the 1880s, when Kraepelin and Flechsig split up in a dispute. At the beginning of his career, between 1881 and 1882, Kraepelin, with the help of a recommendation from Wundt, was employed by Flechsig as a medical assistant. The dismissal of Kraepelin in mid-1882 marked the beginning of a lifelong conflict. On the one hand, this conflict should have been reduced to Kraepelin’s idea of establishing an experimental psychological research laboratory, which would have been very costly and contradicted Flechsig’s scientific approach to psychiatry. Kraepelin already had a certain amount of skepticism about somatic psychiatry at that time, and sought early access to experimental psychology, and later also to experimental laboratory science, which slowly became established in the form of his new approach to psychiatry (53). On the other hand, Flechsig accused

Kraepelin of violating his duty.

As Steinberg and Müller have pointed out, several factors may have played a role here. Thus, Flechsig obviously felt duped by Kraepelin because he wanted to habilitate on a topic that was near to experimental psychology, and whereby he regarded Wundt as his most important contact. On the other hand, Kraepelin seems to have spent a lot of time dealing with his habilitation topic; time he should have spent on service in the clinic. On the basis of archive material, Steinberg and Müller have emphasized that this dismissal cannot be ascribed to Flechsig’s difficult character, as depicted for a long time in the literature, but that Kraepelin himself was more to blame for it (54).

Engstrom has shown that there was also a deep conflict between Ziehen and Kraepelin and his students. By embedding mental processes into the brain, Ziehen was close to the standpoint of what was called “brain mythology”:

“The dispute over the status of psychology within psychiatry and the strategies deployed to overcome resistance to it, has received relatively little attention in psychiatric historiography. But there is no doubt that the dispute was remarkably acrimonious. Ziehen considered Kraepelin’s experimental research to be ‘genuinely bad’ and a ‘caricature of Ebbinghaus’s well known work’ (...). And in turn, by the early 1920s, Ziehen’s critics were celebrating the demise of his association theory and the ‘blind, senseless ... and arbitrary’ cerebral connectivity that it preached (...). Amongst Kraepelin’s students, Ziehen had the reputation of an ‘arrogant, superficial know-it-all, completely ignorant of all psychiatry things’ (...)” (55).

Ziehen also emphasized in his review of his “Brain and Soul” that Flechsig was right to criticize the arrogance of Wundt and Kraepelin, the latter addressed with the term “author of a common psychiatric textbook,” which looked down on brain anatomy (56). There was also a benevolent review of “Brain and soul” by Carl Pelman (1838–1916) (57).

Wundt’s psychophysical parallelism

Wundt formulated his own version of psychophysical parallelism, which argued for an independent

psychic causality and corresponded to his apperception theory. Araujo recently emphasized that Wundt's psychophysical parallelism has not only an epistemological meaning but also a metaphysical meaning. He showed that it is evident in Wundt "that the empirical side of his psychophysical parallelism is initially associated with his metaphysical interests" and that the rejection of localizationism was Wundt's "theoretical intention" behind his concept of psychophysical parallelism (58). Thus, it was for fundamental reasons, that he had to speak out against brain researchers like Meynert or Flechsig, and he already had attacked his former students, Külpe and Münsterberg, for their monistic interpretation of psychophysical parallelism (59). While others spoke of "brain mythology," Wundt himself similarly spoke of "figments of fantasy" and "ideas of utopian brain anatomists":

"According to the present state of our physiological knowledge, the heuristic principle of parallelism is satisfied with the assumption of a consistent correspondence of psychic elements and physiological processes. The assumption that in the brain there are 'association fibers' that procure the so-called 'association of ideas', or that these fibres, which are connecting different centers of the brain, should take on the task of connecting thoughts, these and other ideas of utopian brain anatomists belong to the class of 'sentient atoms', cell-souls of Haeckel and similar figments of fantasy." (29).

Exponents of psychophysical parallelism like Mach, Zöllner, or Haeckel usually used the notion of animated cells (*Zellseelen*) or animated atoms (*Atomseelen*) to clarify the place of the psychical in the physical world (60–61). In Meynert's theory, every ganglion cell has the functional energy of sensitiveness (*Empfindungsfähigkeit*) (62).

Kraepelin later followed his teacher Wundt and implemented this principle into psychiatry. A strict localization of mental illness was thus excluded and the value of somatic psychiatry was fundamentally questionable (28). Fahrenberg suspects that Wundt's theory was hardly received (63), but in fact, through Kraepelin, it played an important role in psychiatry.

As shown by Araujo, we have to view Wundt's scientific psychology as part of his philosophical program, in which it finds its ultimate justification. Araujo

argues that Wundt's psychology is "part of his highest epistemic ideals, which are philosophical in their essence." (58) Therefore, we have to accept that there is a close link between his psychological work and his philosophical thinking. Wundt's standpoint has its roots in the neo-Kantian movement, especially through the influence of Eduard Zeller (1814–1908) (58).

Wundt denied that mental functions can be derived from the mechanics of the nervous substance in principle. This intention of the "utopian brain anatomists" would be, in principle, impracticable, because it was not true that psychic elements were only simple reflections of the physical connections into which we could translate them. If that were the case, one might believe that advances in physiology could eventually somehow solve current problems of this project, but for Wundt the tools were not only insufficient for the time being, but rather "the interrelations between mental processes would be incomprehensible even if the interrelations between brain processes were as clearly understood as the mechanism of a pocket watch." (29).

"It is never possible to arrive, by way of a molecular mechanics, at any sort of psychical quality or process. If, then, experience teaches us that the molecular processes within our nervous system may have psychical concomitants, we can only say that we are here in presence of a fact which lies altogether beyond the cognisance of a molecular mechanics of nerve-substance, and consequently beyond the cognisance of any strictly physiological inquiry. (...) As a matter of fact, we can no more derive the mechanics of nerve-substance from the connexions and relations of our feelings, than we can derive the latter from molecular processes (64)."

Thus, according to Wundt, Flechsig's attempt to "treat psychology as part of brain physiology" had failed completely (65, 66).

Conclusion

I showed that according to Bumke, Kraepelin's pupil Nissl, who himself was the teacher of Jaspers and Gruhle, is regarded as the founder of the term "brain mythology." Nissl himself rejected any assumption of possible functional significance, including neuron theory, which was at this time controversially discussed and

considered unverifiable (67). Meynert's scholar Edinger, who had an intensive correspondence with Nissl about this matter (68), defended it as a "heuristic hypothesis of the highest quality" (69). At the same time, he also predicted that localization would ultimately be accepted in the same way as neuron theory (68).

This modern looking approach was undoubtedly controversial at the time. Nevertheless, I argue that, in principle, the strong polemic that culminated in the expression "brain mythology" was not only limited to a possibly justified criticism, but also had principled reasoning behind it and could be attributed to the rejection of reductionism. The question therefore arises whether the term should be used across the board, or whether it would be better to understand it in terms of a discourse between different schools of thought.

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