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On a not so chance encounter of neurophilosophy and science studies in a sleep laboratory

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Abstract
This article was inspired by participant observation of a contemporary collaboration between empirically oriented philosophers of mind and neuroscientists. An encounter between this anthropologist of science and neurophilosophers in a Finnish sleep laboratory led to the following philosophical exploration of the intellectual space shared by neurophilosophy and science studies. Since these fields emerged in the 1970s, scholars from both sides have been visiting brain research facilities, but engaged with neuroscientists very differently and passionately fought with each other over the reduction of mind to brain. As a case in point, this article looks at the philosophical controversy over the dreaming brain. It serves as a window on the problem space opened up by the demise of positivist conceptions of science, now inhabited by both neurophilosophy and science studies. Both fields face the problem of how to bridge the gap between empirical research and conceptual work. At a time when ontological speculation has made a comeback in these areas of research, studies on how epistemic objects manifest themselves in the material culture of neuroscience could help neurophilosophers to become better materialists. In the sleep laboratory, however, the materiality of dreams continues to be elusive. In dreaming science studies and neurophilosophy encounter a phenomenon that – at least in 2015 – still invites a positivist rather than a materialist attitude.

Keywords
dreaming brain, laboratory ethnography, neurophilosophy, neuroscience, post-positivism

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Figure 1. Philosopher as test subject and participant observer in a sleep laboratory (© Nicolas Langlitz 2008)

I Strange bedfellows

In her pajamas and all wired up for the EEG recording, philosopher-to-be Jennifer Windt spent a Finnish winter night in December 2008 in the basement of the Center for Cognitive Neuroscience at the University of Turku. The German-American doctoral researcher was a member of the Volkswagen Foundation's European Platform for Life Sciences, Mind Sciences and Humanities, a funding initiative bringing together 60 young neuroscientists, psychologists and philosophers to develop transdisciplinary research collaborations, which I had joined as a participant observer from 2006 to 2010. The small group of 7 people that I followed was interested in the dreaming brain and, although spread all over central and northern Europe, it had its home base in this Finnish sleep laboratory run by the philosopher-neuroscientist Antti Revonsuo. As a camera filmed Windt in her slumbers, the rest of our group was sitting next door, occasionally glancing at the video transmission and her brain waves. Twice we had woken her up from non-REM sleep, but all she could report was hypnagogic imagery, never any dreams. After a verbal report through an intercom, Windt had to use a trackball mouse to fill in a questionnaire displayed on the screen above her bed. When she closed her eyes again, we decided to wait for her EEG to show the pattern typical of REM sleep, which in 1953 the neurophysiologists Aserinsky and Kleitman had identified as the neural correlate of dreaming. Around 4 a.m., I fell asleep myself. I was woken up from my own dreams by the excited voices of my collaborators when the electroencephalogram finally showed the characteristic combination of alpha, beta and de-synchronous waves. We hit the buzzer. After a loud alarm signal, a recorded voice – to her consternation it was my voice – asked our philosopher-subject to describe the preceding experience. 'I was fast asleep', Windt mumbled. 'I can't remember anything.' By now it was 6 a.m. Everybody was tired and disappointed. We broke off this test run for a planned experiment and went home.

This encounter between an anthropologist of science and a neurophilosopher sets the stage for the following philosophical exploration of the intellectual space shared by neurophilosophy and science studies. Originally, I had set out to study the epistemic culture of neurophilosophy ethnographically. Science studies scholars have paid much attention to neuroscience, but not to philosophy. Here was a chance both to build on the existing literature and to cover new ground. As Jennifer Windt and I visited sleep laboratories like the one in Turku, attended the annual conference of the International Association for the Study of Dreams in Montreal, and jointly interviewed several prominent dream researchers, I came to notice parallels between the trajectories of our respective fields. We both acted as participant observers of the neurosciences. It struck me as curious that both ethnographers (Latour and Woolgar, 1986[1979]; Lynch, 1985) and empirically oriented philosophers of mind (P. S. Churchland, 1986) began to visit brain research facilities in the 1970s, but have not had much to say to each other – nothing friendly, in any case. In her presidential address to the American Ethnological Society, Emily Martin (2000) even launched a major attack on neurophilosophy. To my mind, however, one of the goals of the anthropology of science is to de-parochialize disciplinary cultures the way cultural anthropology seeks to free us from ethnocentrism. Primed by their efforts to understand what the Bororo mean when they claim to be red parrots, anthropologists are inclined to accept statements such as 'I am my brain' or 'your brain dreamed that . . . ' as social facts to be interpreted in the contexts of their utterance. In the collaborative spirit of the European Platform, however, I eventually sought not just to understand differences between knowledge cultures, but to establish common intellectual ground. Despite its attempts to reduce subjective experience to neural processes, neurophilosophy continues to share the epistemic structure of the human sciences, as laid out by Michel Foucault (1973). Instead of describing neurophilosophers in a predominantly ethnographic mode, this article will situate both science studies and neurophilosophy in the problem space left behind by the demise of a positivist vision of science.

Nineteenth-century positivists such as Auguste Comte or Ernst Mach believed that the replacement of theological and metaphysical conceptions of the world by science would advance human flourishing. Their 20th-century successors, the neo-positivists of the Vienna Circle, sought to crowbar metaphysics by introducing a strict separation between observation and theory, epistemology and ontology: hypotheses either could be tested against the raw data of perception, recorded in a language independent of all theories, or they had to be eliminated alongside the unobservable entities they postulated. As Ian Hacking (1983: 41–57) pointed out, positivists are anti-realists: not the existence of things in themselves, but their sensory perception or its documentation in the form of neutral protocol sentences, would provide continuity to the progression of science by enabling researchers to compare new theories with old ones.

By contrast, post-positivist philosophers of science such as Willard Van Orman Quine (1960), Paul Feyerabend (1962), or Thomas Kuhn (1962) claimed that all observations were already laden with theory and therefore could not settle scientific disputes. At least in part, scientific change had to be explained psychologically and sociologically and whether it always amounted to progress was hotly debated. In his intellectual history of Anglo-American science studies, John Zammito (2004) argued that this hodgepodge of philosophy, history, sociology and anthropology of science

grew out of the post-positivist naturalization of epistemology. But soon studies of the material cultures of laboratories and other research sites replaced the theory-dominated view of science held by post-positivist philosophers; they did so by a focus on practice. Ethnographic and micro-historical studies showed how scientific instruments authorized new forms of social organization and natural philosophy (Shapin and Schaffer, 1985), how experimental systems shaped epistemic things (Rheinberger, 1997), and how the entities researchers experimented with were not passive objects of representation, but interacted with the instruments used to probe them and manifested themselves in response and often in resistance to their investigation (Latour, 1987; Pickering, 1995). The construction of scientific representations was no longer looked at as a purely human activity, but was equally shaped by the objects under investigation. As things themselves were thought to break through the mantle of appearances, the distinction between epistemology and ontology collapsed. Against the most fundamental philosophical impulse of positivism, metaphysics was making a comeback; a prelude to the 'ontological turn' (Henare, Holbraad and Wastell, 2007) and the 'new materialisms' (Coole and Frost, 2010) that were to follow.

Although neurophilosophy has preserved more positivist philosophemes than science studies, it also grew out of post-positivism advocating the theory-ladenness of observation, the naturalization of epistemology, and a metaphysical worldview centered on scientific realism and materialism. How these responses to the predicament of positivism relate to the ones provided by science studies will be explored in this article as we review the philosophical reception of neuroscientific dream research: from Norman Malcolm's Wittgenstein-inspired attack on the neurophysiology of dreaming to Patricia Churchland's presentation of sleep and dream research as key to eliminating the folk psychological notion of consciousness and Antti Revonsuo's dream catcher thought experiment in which the reduction of mind to brain is achieved as researchers relive their test subjects' instrumentally recorded dreams in the medium of a virtual reality environment. Far from replacing the human sciences, these reductive and eliminativist approaches continue to be organized around the 'empirico-transcendental doublet' of man as 'a being such that knowledge will be attained in him of what renders all knowledge possible', which Foucault (1973: 318) had identified as the epistemic object of the *sciences humaines*. If man's face had been drawn in sand at the edge of the sea, as Foucault imagined, neither neurophilosophy nor science studies have erased it after all those years. As human sciences, however, these fields are both confronted with the problem of how to bridge the gap between empirical research and conceptual work.

II Anti-psychologism: Is there a neurophysiology of dreaming?

If turn-of-the-century philosophers such as logician Gottlob Frege or phenomenologist Edmund Husserl had had their way, neither Jennifer Windt nor any other budding philosopher would have wasted their time in a neuroscience laboratory. And these advocates of disciplinary autonomy and purity did have their way for much of the 20th century, as the philosopher and historian of science Martin Kusch has argued in a sociological analysis of the psychologism controversy. As the new scientific psychology emerged in Germany between 1880 and 1920, a novel figure, the 'physiologist-philosopher', took

the academic stage. The most prominent representative of this movement was Wilhelm Wundt, a professor of philosophy who had built up his own psychology laboratory and conceived of his experimental work as a scientifically rigorous way of doing philosophy. The approach became so popular that, by 1913, 23% of all positions in German philosophy departments had been filled with experimental psychologists. Their success provoked opposition on the part of colleagues who wanted to protect their discipline against natural scientific incursions. The disciplinary backlash against empirical philosophers was initiated by Frege and brought to fruition by Husserl. They crafted powerful arguments against the psychologization of philosophical questions and rallied enough forces in the academy and the ministries to purge philosophy of psychologism and psychologists. This triumph geared substantial parts of academic philosophy toward the phenomenological exploration of transcendental subjectivity, the fundamental ontology of Being, and linguistic analyses of conceptual truths. Following the admittedly crude dichotomy of analytic and continental philosophy for a brief moment, we could say that through Frege's enormous influence on what – despite its German and Austrian origins – came to be known as the Anglo-American tradition and Husserl's eminent role on the European continent the desire to separate philosophical reflection from empirical research was instituted on both sides of the discipline.

Against the background of this well-established anti-psychologism, the purported discovery of physiological correlates of dreaming in 1953 did not receive a warm welcome in philosophy. In 1953, Eugene Aserinsky and Nathaniel Kleitman (1953) recorded both electroencephalograms (EEG) and electrooculograms (EOG) of sleeping subjects. They found cyclically recurrent phases of sleep characterized by rapid eye movements (REM) and pronounced brain-wave activity. When stirred from these so-called REM sleep periods, 74% of subjects could provide detailed dream reports while only 8% remembered a dream after awakenings from non-REM sleep. Aserinsky and Kleitman inferred that dreaming was a manifestation of a particular level of cortical activity normally encountered during sleep.

Four years later, William Dement and Kleitman (1957) related objective EEG and EOG recordings to subjective reports of dream content. They compared subjects' estimates of dream-duration to the length of REM periods and showed that 4 out of 5 test persons were able to determine the correct dream duration. Moreover, specific eye-movement patterns were correlated with visual imagery. In the course of 35 awakenings, most EOG measurements showed both horizontal and vertical motions and were difficult to associate with the dreamed activities. However, three reports went along with purely vertical eye movements. In all these dreams the action also took place in the vertical plane. For example, one subject 'dreamed of standing at the bottom of a tall cliff operating some sort of hoist and looking up at climbers at various levels and down at the hoist machinery'. In the single instance where the sleeper's eyes exclusively moved from left to right, the subject reported 'watching two people throwing tomatoes at each other' (ibid.: 344).

Such correlations between supposedly 'subjective' dream reports and 'objective' electrophysiological measurements showed 'that Dement and Kleitman have an erroneous picture of the concept of dreaming', claimed the ordinary language philosopher Norman Malcolm (1959: 79). Dreams, Wittgenstein's disciple argued, were nothing but

dream reports, the language-game of 'I dreamed that . . . ': 'It is senseless to suppose that [a person's] dream differed from his report of it unless this means that he might change, add to, or contradict his report. No one knows what it would mean to "verify" his report. Others use his criterion as their criterion of what his dream was' (Malcolm, 1956: 30). Ian Hacking (2002: 233) called this 'the perfect example of . . . the positivist attitude to dreams' rejecting any metaphysical speculation about the nature of dreams as dreamed at night (see also Hacking, 1975: 103–12). Adopting the verifiability principle of neo-positivism, Malcolm held that the meaning of a proposition depended on what would count as evidence for its truth and this made nonsense of correlating neurophysiological activity and dream narratives:

> This correlation might be so impressive that scientists would be tempted to adopt the occurrence of the physiological process as their criterion for the occurrence of a dream. Let us imagine that it even became the criterion in ordinary life. There would then be such a thing as proving that a man had dreamt, although on awakening he honestly reported that he had not; and the duration (three minutes, say) of the physiological process, and its time of occurrence, could be made the criterion of the duration and time of occurrence of the dream. It would even have sense to say of someone 'He is halfway through his dream!' All of this would amount to the adoption of an extremely different use of the word 'dreaming'. Its meaning would have to be taught differently, and all sorts of remarks would make sense that at present do not. (Malcolm, 1956: 29–30)

Following a particular brand of philosophical anti-psychologism, Malcolm did not take the empirical findings of Kleitman's laboratory as an incentive for conceptual change. Instead he enforced a strict division between the conceptual and the empirical, the analytic and the synthetic, as the respective domains of philosophy and the sciences. Whereas analytic propositions are necessarily true by virtue of their meaning to be determined by examining logical relationships between concepts, synthetic propositions happen to be true because of certain facts about the world that need to be validated by experience and empirical inquiry. In Malcolm's eyes, the identity of dream and dream report was an analytic truth. Dreams could not possibly be experiences during sleep because 'sleep qua sleep has no experiential content' (Malcolm, 1959: 39). Ascribing this way of talking about sleep to customary uses of English, Malcolm claimed that Dement and Kleitman's electrophysiological objectification of dreams presupposed an out-of-the-ordinary concept of dreaming. Therefore, their discoveries did not even pertain to what people usually called dreaming. For conceptual reasons alone, dreaming could not be an epistemic object of sleep research. Here the normative implications of Malcolm's ahistorical conception of ordinary language come to the fore. His insistence that 'that is how the words are used!' (Malcolm, 1956: 31) is not a socio-linguistic observation, but a dictum meaning 'This is how the words ought to be used!'

The philosopher Hilary Putnam disagreed. He questioned the discontinuity between everyday and scientific concepts of dreaming: 'Virtually a hundred per cent of all hearers will "pass" these discourses without detecting the slightest trace of linguistic oddity' (Putnam, 1975[1962]: 314–15). But, more fundamentally, Putnam defended physiologists like Dement and Kleitman by challenging Malcolm's account of meaning. Putnam

shifts the discussion from dreaming to two different scientific objects – neurological disease such as multiple sclerosis and chemical substance. That he could make this shift indicates that his argument addresses a more general question in the history and philosophy of science, but it also betrays that dreaming might not have been ideal to make his case. Putnam asked his readers to consider a disease such as multiple sclerosis, which is difficult to diagnose because the same symptoms could also be seen in other neurological diseases while only some of them might be on display in any particular case of multiple sclerosis. One of the possible etiologies already discussed in 1962 was that of unspecified infectious agents. In a *reductio ad absurdum* of Malcolm's assumption that meaning was a matter of purely conventional linguistic criteria, Putnam imagined a patient called X suffering from the typical symptoms: 'Then Malcolm's view is that, no matter what we find out later, X has multiple sclerosis because that is what we presently mean. In particular, if we later identify a virus as the cause of multiple sclerosis, and this patient's condition was not caused by that virus, he still had multiple sclerosis' (ibid.: 310). Similarly, Malcolm's account would imply that 18th-century chemists meant different chemical substances than their 20th-century colleagues as the former identified acids as sour-tasting, soluble in water and turning litmus paper red while today acids can be defined more theoretically as proton donors. Dreaming, Putnam had to admit, was a special case because 'the man on the street certainly does not assume the existence of a physical referent when he uses the word', but he does assume – *pace* Malcolm – that dreams 'take place during the night', i.e. in physical time (ibid.: 315). As a scientific realist early Putnam maintained that meaning was not just a matter of convention, but also of reference to things that are out there. As we accumulate knowledge about them, the criteria to discern multiple sclerosis, acids, or dreams change. In the case of such natural kinds, however, this does not necessarily mean that the concepts no longer refer to the same unobservable entities: 'Whether scientists at t_1 and scientists at t_2 are or are not talking about the same thing when they use a term is . . . to be ascertained by examining the relevant scientific theory (the latest one available!) and not by linguistic investigations', argued Putnam (ibid.: 311). Dement and Kleitman had not altered any meanings – they had just found out more about dreaming.

Logically, however, Putnam could not disprove Malcolm's account. If dreams were defined as dream reports, no discovery about the physiology of sleep could provide any reason to change the dream concept. Malcolm's argument was circular and its circularity made it impenetrable to empirical findings. Putnam's counter-argument was equally closed. Only if dreams were defined as experiences during sleep could research on the correlation of sleep physiology and dream content support the presupposition that dreams are experiences during sleep. Putnam (1975[1962]: 315) was well aware of this standoff: 'Perhaps there are two ''circles'' present; but one of these circles may be the circle of our usual ways of thinking and talking, while the other is just the lonely circle of an unusual philosophic position.'

Although Malcolm and Putnam disagreed so fundamentally, neither of these thinkers could base his presumptions on access to the true nature of dreams. David Bloor, one of the founders of the so-called Strong Program in science studies, concluded that their disagreement over the nature of dreaming could only be explained by social factors. Behind the metaphysical dispute over scientific realism Bloor (1983: 68) unveiled actual and

potential conflicts between experts and laypersons, which the philosophers merely rehearsed: 'Malcolm wants to stress the autonomy of commonsense language-games: no one, he says, is required to follow the example of the scientist. By contrast, what Putnam wants to stress is the authority of the scientist. This is why he slides from a description of scientific practice to a normative conclusion about its legitimacy.' But when philosophers tried to legislate, Bloor maintained, their position was groundless: 'The real duty of the analyst is not to be an advocate of the interests of this or that social reference group. It is to make clear that we have a choice and to spell out its ramifications' (ibid.: 81). Following Wittgenstein's call for an anthropology of modernity that describes, but does not evaluate, 'our conduct and our valuations just like those of the Negroes', Bloor advocated the replacement of philosophy by the sociology of knowledge (ibid.: 82 and 183).

The controversy between Malcolm and Putnam took place in the early 1960s, at a time when ordinary language philosophy was still playing strong, but new venues for empirical philosophies were emerging. The consensus against psychologism began to show its first cracks. It is no coincidence that these chapters in the entangled histories of philosophy and science attracted the attention of students of science such as Bloor and Kusch. As a field of empirical research with pronounced philosophical ambitions, science studies was confronted with some of the same epistemological obstacles and objections as the empirically oriented philosophy of mind (Kusch, 1995: 14–15). In their own ways, both domains sought to naturalize epistemology. Revealing the historically contingent social construction of philosophical arguments against psychologism paved the way for rehabilitating hybrid forms of philosophy, which combined concept work with empirical inquiry. A prehistory to the not so chance encounter of neurophilosophy and science studies in a Finnish sleep laboratory.

III The birth of neurophilosophy from the spirit of post-positivism

A separate historical trajectory starts out from the Vienna Circle, but leads into the same problem space. In the 1920s, neo-positivists aspired to support rather than to curb science. But, like a long lineage of Kantians before and the ordinary language philosophers after them, they advocated a division of labor instead of continuity between philosophy and the empirical sciences. Their claim for the autonomy and authority of philosophy was based on a series of related distinctions: between the analytic and the synthetic, the conceptual and the empirical, and the context of justification and the context of discovery. Empirical researchers explore how the world might be, but it is philosophers who decide whether their discoveries do not violate any analytic truths, whether they are conceptually sound and epistemologically justified. This partition also implies that the investigations of historians, sociologists and psychologists are confined to the context of discovery. They must not trespass into the normative domain of philosophical judgement. Facts and values have to be kept separate (Zammito, 2004: 12–14). All this was called into question in the 1950s and 1960s. While French structuralists and poststructuralists began to do away again with the separation of philosophy and the human sciences, in North America post-positivist philosophers of science, most prominently represented by Quine, Feyerabend and Kuhn, tore down the opposition of the empirical

and the conceptual underlying the positivists' understanding of science and philosophy. John Zammito has shown how this led to the emergence of science studies. But it also laid the foundations for neurophilosophy.

In the 1970s, Anglo-American philosophy of mind began to take a naturalistic turn. Among the earliest and most radical representatives of this movement were the Canadian philosophers Paul and Patricia Churchland. They met at the University of Pittsburgh where Paul Churchland was working on his PhD. But soon his future wife won a fellowship for Oxford, which still served as the stronghold of ordinary language philosophy, although it was already in decline (Forguson, 2001). Perfectly epitomized by Norman Malcolm whose publications on dreaming primarily served to dismantle Cartesian dream skepticism, this school of thought took traditional philosophical problems to be perplexities growing out of philosophers' inattention to what their words actually meant in everyday usage. In Patricia Churchland's eyes, however, this focus on conceptual analysis was too constrained: 'I'm not just interested in the anthropological question of what somebody means by free will', she discovered at Oxford. 'I'm interested in whether or not we have it! Is it real? Is it in our brains? And that meant all of a sudden I found myself shifting into the empirical domain, to try to understand what we knew, what science knew about the things I was interested in' (Churchland and Churchland, 2006: 7).

The philosopher who had prepared the epistemological ground for the Churchlands' amalgamation of philosophy and neuroscience was Quine. Quine (1951: 34) had claimed that the distinction between analytic and synthetic truths was 'an unempirical dogma of empiricists, a metaphysical article of faith'. In Paul Churchland's (1979: 46–54) rendering of Quine's more multifaceted argument, the assumption that certain sentences were analytic was based on the intuition that their truth was rooted in our understanding of the terms they contain. 'All bachelors are unmarried males' is the classical example, 'dreams are dream reports' a more controversial one. Supposedly, there is no fact of the matter beyond these definitions. But when a 17th-century alchemist explained combustion by postulating the existence of phlogiston, a fire-like element contained by inflammable materials, its conception was equally exhausted by the postulated set of assumptions about the substance, argued Paul Churchland. Denying any of these assumptions would have been inconsistent with the notion of phlogiston. Considering that 18th-century chemists could demonstrate experimentally that combustion did not amount to a loss of phlogiston, but to compounding with oxygen, it is clear that sentences defining phlogiston cannot count as necessarily true or analytic. But in Churchland's eyes it was not synthetic either because the concept of phlogiston had no other source of semantic identity than these sentences. He concluded: 'A given sentence may not admit of a denial consistent with one's current understanding of its terms, but this provides no guarantee that the sentence is even true, let alone necessarily true, let alone true solely in virtue of meanings' (ibid.: 47). He followed Quine's proposal to replace the distinction between analytic and synthetic truths by a conception of knowledge as an evolving and continuous network of beliefs in which logical and ontological assumptions are located more centrally than empirical observations. Even though peripheral statements can be revised more easily in light of recalcitrant experience, in principle everything is up for grabs – even the previously deemed analytic sentences at the very core of this fabric. By arguing against the cleavage between the purely conceptual and the factual, Quine had blurred

the boundary between speculative metaphysics and empirical research. 'For him, the philosophical undertaking wasn't something distinct from science', Paul Churchland noted (Churchland and Churchland, 2006: 7).

The most prominent consequence of Quine's rehabilitation of psychologism was that epistemology had to be naturalized. The logical positivists had opposed such a move because of its circular logic. Wanting to establish an epistemological foundation for the empirical sciences, they would have thwarted their goal by drawing from the empirical sciences in the process of their validation. But for Quine this normative endeavor was futile as it presupposed that a clear-cut distinction between empirical discoveries and their philosophical justification could be made and that neutral observation sentences could serve as the bedrock of science. In his view, however, an observation sentence was not just contingent on present sensory stimulation but also on 'stored collateral information' such as theories and languages that go into understanding the sentence (Quine, 1969: 85–6). If philosophers stopped dreaming of deducing science from always already theory-dependent observations they could also rid themselves of their scruples against circularity (ibid.: 76). The goal of Quine's naturalized epistemology was no longer to authorize but to understand how science worked – and how it worked so successfully that it arrived at theories which made verifiable predictions. 'We are after an understanding of science as an institution or process in the world, and we do not intend that understanding to be any better than the science which is its object' (ibid.: 84).

Science studies would come to provide varieties of such an understanding of science that were largely social and historical. By contrast, Quine and the Churchlands drew from the mind and life sciences rather than the social sciences to naturalize epistemology: 'Epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science', Quine wrote:

> It studies a natural phenomenon, viz., a physical human subject. This human subject is accorded a certain experimentally controlled input – certain patterns of irradiation in assorted frequencies, for instance – and in the fullness of time the subject delivers as output a description of the three-dimensional external world and its history. The relation between the meager input and the torrential output is a relation that we are prompted to study for somewhat the same reasons that always prompted epistemology; namely, in order to see how evidence relates to theory, and in what ways one's theory of nature transcends any available evidence. (Quine, 1969: 82–3)

The Churchlands followed Quine's anthropology of science focusing on the individual rather than the group, on the scientist as a biological organism rather than the social milieu and material culture enabling her research. In contrast to Quine's behaviorism, however, the neurophilosophers were already children of the cognitive revolution of the 1970s. Whereas Quine only cared about stimulus and response, they turned to brain research to see what was inside the black box translating scientific observations into underdetermined theories.

Churchland and Churchland also abandoned the linguistic turn, which Quine and his generation of analytic philosophers had taken (Keeley, 2006: 13–14). In particular, Paul Churchland (1979: 125) opposed the idea that cognitive activity occurred in an internal

language of thought. In his eyes, human beings were no 'ideal sentential automatons' representing their inputs as observational and theoretical sentences woven into webs of propositional beliefs. Networks of neurons came to replace networks of propositions in the neurophilosophical conception of *anthropos* as an 'epistemic engine'. When the Churchlands moved to the University of California at San Diego (UCSD) in 1984, they aligned themselves with the then flourishing movement of connectionism in cognitive science, which provided a theory of how mental and behavioral phenomena emerged from sub-symbolic rather than linguistic information-processing in interconnected networks of simple units. Implemented in computer simulations of neural networks, the units of these networks were modeled on nerve cells: they received input from a number of connected neurons and, depending on how they were activated or inhibited, they passed the activation on to another unit. Without any words such a system could learn to make desired distinctions as each unit weighed the inputs differently to adjust its output behavior to a set goal. This framework enabled Paul Churchland to give a neurocomputational form to some of the central doctrines of post-positivism.

The positivist vision of science had collapsed as more and more philosophers grew convinced that there was no language of raw sense data in which scientists could represent their observations in a way that was independent of the controversial claims put forward by the theories these observations were meant to test. But from Paul Churchland's naturalist vantage point, the resulting crisis of representation was no crisis at all. He reduced the theory-dependence of all perception to a brain function. A theory, conceptual framework, or scientific paradigm was no set of linguistic propositions (possibly accompanied by certain practices), but a particular configuration of a neural network. No sensory input could enter into the system without passing through a more or less well trained or maybe still inchoate filter of synaptic connections and weights: 'Theory-ladenness thus emerges not as an unwelcome and accidental blight on what would otherwise be a neutral cognitive achievement, but rather as that which makes processing activity genuinely cognitive in the first place' (P. M. Churchland, 1998: 271; see also P. M. Churchland, 1996: 277).

From the theory-ladenness of observation follows a second important doctrine of post-positivism. If there is no neutral vocabulary in which to articulate observation sentences, then there is no common measure to translate and compare successive and competing theoretical frameworks or paradigms with each other. In Churchland's neurocomputational guise, such incommensurability is based on the fact that for sheer mathematical reasons many of the billions of possible configurations of 10^8 brain neurons cannot be brought into an equivalence relation to the current configuration. But if there are no independent criteria to make reasoned empirical choices between competing theories, the rationality of scientific progress is put in jeopardy. Any framework seems as good as any other and a radical relativism would ensue. On the basis of what could we determine whether chemistry was truer than alchemy, whether a neuroscientific account of dreaming ought to replace the one provided by psychoanalysis or common sense? Although Paul Churchland embraced the notion of incommensurability, he did not share positivist concerns about its relativist consequences. In his eyes, recognizing incommensurability required the discarding of the positivist conception of science and to 'get down to the more serious business of exploring how empirical data

really steer our theoretical commitments' (Churchland and Churchland, 1998: 274). And that did not necessarily happen by choosing between commensurable theories in light of a continuously growing body of uncontroversial facts. 'Commensurability', Paul Churchland maintained, 'is just a measure of the similarity between alternative frameworks, and sometimes what the epistemic situation requires is a profoundly different perspective on the world' (ibid.: 274–5).

The profoundly different perspective advocated by Paul Churchland was in line with Feyerabend's iconoclastic rearticulation of Democritus' ancient materialism. Against the restrictions of common sense and the complacency of ordinary language, Feyerabend (1963) defended the possibility that the customary idiom in which people spoke about their inner lives and which was reinforced by powerful institutions could be succeeded by a purely physiological account of human beings. From this argument Paul Churchland derived his 'eliminative materialism' (Churchland, 1998: 257), proposing 'that our common-sense conception of psychological phenomena constitutes a radically false theory, a theory so fundamentally defective that both the principles and the ontology of that theory will eventually be displaced, rather than smoothly reduced, by completed neuroscience' (P. M. Churchland, 1993: 42).

Considering its genealogy this position should come as a surprise, both ontologically and politically. The theory-ladenness of observation led many a post-positivist to adopt an anti-realist metaphysics. If all observations are theoretically biased, why would the entities postulated by a currently held theory be any more real than those populating the preceding framework? Kuhn's *The Structure of Scientific Revolutions* (1962) suggested that the incommensurability between different paradigms made it impossible to say what was actually out there. And in Quine's (1951: 41) network of beliefs, 'Homer's gods and physical objects only differ in degree, not in kind'. Yet Churchland advocated an unexpected realism (Keeley and Krieger, 2006). In his evolutionary epistemology, learning was an adaptive behavior promoting the survival of an organism by enabling it to adjust to an environment, which was only all too real in the pressure it applied. The organism's neural network not only processed all perceptual input and thereby charged it with the synaptic equivalent of theoretical presuppositions, but it also transformed itself in this iterative process to accommodate to the given challenges. Consequently, the theory-ladenness of observation not only bore the risk of concocting a distorted image of the world, but also carried the promise of a steady enhancement of human engagement with reality (Churchland, 1979: 142–51). Expressing an sense of spiritual unity with the physical world, which I have dubbed 'mystic materialism' (Langlitz, 2012: 204–41), Churchland wrote:

> If our perceptual judgments must be laden with theory in any case, then why not have them be laden with the best theory available? Why not exchange the Neolithic legacy now in use for the conception of reality embodied in modern-era science? . . . The resulting expansion of our perceptual consciousness would be profound. Should we ever succeed in making the shift, we shall be properly at home in the physical universe for the very first time. (P. M. Churchland, 1979: 35)

Politically, Paul Churchland's proximity to Feyerabend is astonishing because this *enfant terrible* of academic philosophy and intellectual star of the counter-culture not

only sought to defend the possibility of revolutionary materialism against stifling tradition. Feyerabend (1998[1975]) was equally passionate about defending society against science, which, in his eyes, had become just as oppressive as the ideologies it had once fought. He wanted to protect science against itself, against its own authoritarian tendencies, which suffocated alternative views in the name of the one and only truth. By contrast, Churchland's eliminative materialism was precisely about the eradication of incommensurable common-sense conceptions of the mind, so-called folk psychology, in the name of neuroscience. Much of the social scientific opposition to neurophilosophy at a time when US President George H. W. Bush declared the 1990s as the 'Decade of the Brain' and European and American governments began to support the neurosciences with billions of dollars was a reaction to a radical materialism, which seemed to have lost its air of heterodoxy. This is not the place to discuss how accurate this assessment is, especially in America where politics are powerfully shaped by the fact that 90% of adults continue to believe in God and more than 80% in the survival of an immaterial soul (Shapin, 2008). Paul Churchland, in any case, confessed that, although his epistemological impulses and his heart inclined to Feyerabend, his political impulse did not veer to him, but to Kuhn who had recommended a conservative policy focusing most resources on the currently dominant scientific paradigm (Churchland and Churchland, 1998: 302–3).

Epistemologically, the structure of the Churchlands' naturalized epistemology is also not as seditious as many in the human sciences have taken it to be (see, for example, Kusch, 1997; Martin, 2000). In fact, the demolition of the Kantian distinction of the analytic and the synthetic only reaffirms the figure of 'man' as an empirico-transcendental double, simultaneously subject and object of anthropological knowledge. Man reappears in the guise of the brain, which is both the object of empirical research and the quasi-transcendental condition of all knowledge, including neuroscientific knowledge. This would assign the brain and its neurophilosophical reflections a central role in the anthropology of science.

IV Dreaming in and of a unified science of the mind–brain

Early Paul Churchland's reception of post-positivist epistemology prepared the ground for the birth of neurophilosophy, which was marked by Patricia Churchland's 1986 book of the same title. The goal was to work towards a unified science of the mind–brain bridging the gaps between neurobiology, psychology and cognitive science. The neopositivists had already dreamed of uniting all sciences around the one scientific method, which produces incorrigible observation sentences in a theory-neutral language shared across disciplines. The Churchlands were not only critics, but also heirs of positivism in that they too hoped to integrate a coherent body of knowledge by reducing less basic sciences like psychology to more basic sciences like neurobiology. Their materialist anthropology notwithstanding, the epistemology underlying neurophilosophy was quite idealistic as it conceived of these sciences as theories (although neurally implemented) rather than social institutions or material cultures. Consequently, reduction took the form of a relation between two theories. However, if these theories were not based on theoretically unbiased observation sentences, the commensurability required by any reduction

could not be presupposed, but – if at all possible – it had to be achieved in a gradual process of interdisciplinary co-evolution (P. S. Churchland, 1986: 278–95). Post-positivism had historicized the reductive epistemology, which the Churchlands had inherited from their positivist forebears.

Contemporaneous with neurophilosophy, the emerging field of consciousness studies returned consciousness to the very center of the newly merged philosophico-scientific attention. The 'taboo against consciousness' imposed by behaviorism and the linguistic turn for almost half a century had been lifted (Baars, 2003; Roepstorff, 2003). Many critics of the reduction of mind to brain pointed to consciousness as a property of mental states that could not be explained in terms of brain states. Thus consciousness was a strategic point to conquer on the way to a unified science of the mind–brain. As the royal road to its neurobiological basis, Patricia Churchland (1988) proposed sleep and dream research. But her tactics indicated that she would rather destroy than reoccupy this fiercely embattled position. In Patricia Churchland's eyes, the study of consciousness was still based on folk psychology, which took the waking state of healthy human beings as paradigmatic. She doubted whether consciousness was a natural kind and speculated that neuroscientific research would show that this 'old folk notion' had to be eliminated and that the variety of phenomena previously conflated under this label would have to be recategorized (ibid.: 302). She discussed a broad spectrum of neurological and psychiatric disorders, from blind-sight and hemi-neglect to split-brain patients and multiple personality disorder, as 'denormalizing data' – the kind of anomalies, which, in Kuhn's account, would ultimately explode the prevalent conceptual framework and lead to a scientific revolution (ibid.: 287–90).

But it was not exclusively pathological phenomena which called the paradigm of waking consciousness into question. Drawing from Allan Hobson's neuroscientific research, Churchland pointed to dreaming as a state of consciousness that regularly occurred in normal subjects while being radically different from waking. Characteristic EEG patterns, rapid eye movements and other objective measures even allowed the identification of different sleep phases in animal models where invasive techniques could be employed to determine correlations between psychological and physiological conditions. Patricia Churchland (1988: 290) was convinced: 'If there is a domain relevant to consciousness which has sufficient supporting infrastructure and surrounding theory to enable experimental discovery, it is the sleep-dream-awake cycle.'

V 'Maybe that's something that Descartes would have predicted . . . '

Only in this one article did Patricia Churchland (1988) present sleep and dreaming as keys to consciousness. To my knowledge she never followed up on her own proposal. In the face of Churchland's initial optimism regarding the supporting infrastructure, the 1980s were actually the time when sleep researchers turned away from the vagaries of dreaming to the clinical condition of sleep apnea, which could be measured without having to rely on any subjective reports and was increasingly recognized as a public-health problem. Following the new funding priorities, many dream researchers began to apply their sleep laboratory skills to this burgeoning new field (Kroker, 2007). Far away from

the hustle and bustle of American medical science, however, in a town in the south of Finland, Patricia Churchland's proposal eventually came to life.

At the University of Turku, the philosopher, psychologist and neuroscientist Antti Revonsuo (1995) took up Churchland's idea and built his own sleep laboratory to study the dreaming brain as a model of consciousness. Dreaming seemed to be the perfect state to study what he called 'the most fundamental question' of 'why there is something rather than nothing, for me?' (Revonsuo, 2006: xv). While dreaming, the central nervous system generates an experience of immersion in complex surroundings while being physiologically dissociated from sensory input and motor output, Revonsuo (1995: 42) argued. Insulated against the confounding noise of action and perception, the dreaming brain was taken as a model of 'pure consciousness'. Whereas Churchland had conceived of dreaming as one anomalous state among others, which only for pragmatic reasons seemed particularly suitable to *eliminate* the concept of consciousness, Revonsuo sought to maintain, clarify and support that concept and neuroscientific dream research appeared to be the best way of pinpointing the phenomenon experimentally (2006: 30–2).

Elsewhere I have written about the existential and political experiences motivating this preference for a model, which presents the brain as largely isolated from the rest of the body and the world – as if in a vat (Langlitz, forthcoming-a). In this article, however, the focus is not on the philosophy of mind, but the underlying philosophy of science. At the beginning of Revonsuo's quest for the neural correlates of consciousness stood a thought experiment. He imagined a neuro- or rather consciousness-imaging technology called 'dream catcher/presenter' that first captured the physiological activity of a dreaming brain. Then the device would enable scientists to relive the experiential content of their test subjects' dreams in the medium of a virtual reality environment. This science fiction scenario, prefigured by Douglas Trumbull's 1983 movie *Brainstorm*, contains a serious philosophical argument: only if neuroscientists succeed in reconstructing subjective experiences from objective data can they claim to have genuinely discovered consciousness in the brain. However, in contrast to theoretical accounts which try to close the explanatory gap by developing a set of propositions laying out causal relationships between neural and psychological processes the dream catcher provides experiential rather than theoretical access to its object. Mediated by neurophysiological measurements the mind–brain is eventually represented in the medium of another mind–brain (Revonsuo, 2006: 300–3, 344–7; for a more detailed discussion of another such 'experiential model', see Langlitz, 2012: 132-65).

Revonsuo operationalized this thought experiment in the form of a more limited laboratory experiment. To test the materialist doctrine of mind–brain identity, he sought to determine the occurrence of dreams as conscious states in EEG recordings. Neurophysiological indices telling apart dreaming from dreamless sleep within one and the same sleep phase would amount to neural correlates of consciousness. When data from serial awakenings of 9 subjects had been collected, these data were divided. Introspective reports and electroencephalographic recordings were analysed by different judges who were ignorant of which EEG sequences had led to dream reports and which ones had not. An external EEG research group used a number of statistical methods to identify the signature of the recordings that were followed by dream reports. But the accuracy of their

predictions turned out to be no better than chance. A doctoral researcher presenting these findings at a conference explained that there were 4 different explanations for this failure: 'Subjective experience is a) not in the brain, b) is in the brain, but not in the EEG, c) is in the EEG, but not in our data, or d) is in the data, but needs more complex and novel methods of analysis.' In 2008, Revonsuo conceded in a conversation:

> We still haven't found any objective sign indicating the presence or absence of consciousness in the dreaming brain. Maybe that's something that Descartes would have predicted: that you cannot objectively capture consciousness because it is this immaterial, non-spatial, and imperceptible thing . . . We haven't been able to disprove the Cartesian position . . . The dream catcher experiment is a test of the whole emergent materialist position . . . We will continue our analysis, but if we can't find anything then we have a real problem where to go.

VI How to make neurophilosophy more materialist

Although neurophilosophers are proud to have left the armchair, not many get their hands as dirty as Antti Revonsuo did. Like most laboratory ethnographers, the majority reads neuroscientific articles, converses with brain researchers and occasionally visits their laboratories. The European Platform for Life Sciences, Mind Sciences and Humanities was meant to provide an opportunity to the participating philosophers to familiarize themselves and get practically involved in the intricacies of laboratory work. The anecdote shared at the outset of this article already hinted at a fundamental problem that has haunted the neurophilosophical reception of dream research from the start: the discovery of REM sleep did not amount to identifying the neurophysiological equivalent of dream consciousness. After 60% to 93% of experimental awakenings from REM phases, subjects recall dreams (Nielsen, 2000). But, as philosopher Jennifer Windt and the rest of our group learned the hard way, this characteristic pattern of brain waves does not always coincide with hallucinatory experiences during sleep. As David Foulkes (1962) had already suggested, REM sleep and dreaming are not identical. So far, Revonsuo's more systematic search for the neural correlates of dream consciousness has not uncovered any more reliable indicators.

Philosophically, such experiences in the laboratory are worthless if they fail to be conceptualized. It is ironic that materialist philosophers of mind have been frequenting brain research sites for more than 30 years without reflecting more systematically on the material culture and practice of neuroscience, even though this was exactly what science studies scholars have been examining in parallel all along. Of course, Churchland and Churchland had read their Kuhn. *The Structure of Scientific Revolutions* had taught them 'that learning the theories peculiar to any discipline is not solely or even primarily a matter of learning a set of laws and principles: it is a matter of learning a complex social practice, of entering a specialized community with shared values and expectations', of acquiring 'the right skills of instrumental and symbolic manipulation', etc. (Churchland and Churchland, 1998: 11). Nevertheless, even as an official member of the UCSD Science Studies Program, Paul Churchland insisted, these

disciplinary theories still remained theories, 'however much they have become the implicit engine of intricate mundane practice' (ibid.: 34). True to this understanding of the sciences, neurophilosophy treats them as theoretical representations of the world. In response to the post-positivist problematization of representation, the Churchlands sought to naturalize epistemology by coming to understand 'how the mind–brain represents what it represents', including itself (P. S. Churchland, 1986: 5). Their anthropology of science was neither social nor cultural but cognitive in that it departed from the assumption that 'cognition cannot be understood as a fundamentally propositional-logical affair', but would eventually be represented in the geometrical form of state vectors in a multidimensional phase space (ibid.: 271 and 457). Since the neurocomputational reduction of scientific theories and paradigms to brain states has not yet been achieved, however, neurophilosophy continues to treat the sciences it draws from as logical systems of propositions. In its own epistemic practice, neurophilosophy ignores the material culture and social culture of brain research as if they had been sublated into the final results of neuroscientific publications. In this respect, it looks at science as a product rather than a process, as a theory rather than a practice.

In the coeval science studies literature, the Strong Program radicalized Kuhn's insight that scientific theories are shared and contested by communities. Controversies such as the one over psychologism or the dispute between Malcolm and Putnam were explained by social factors alone. This social constructionism might appear as the very antithesis of the naturalism of neurophilosophy. But, as Bloor (1999) emphasized, the Strong Program emerged in the 1970s as part of an equally naturalistic enterprise just providing a different response to the dilemmas of post-positivism. Having been trained in experimental psychology in the heyday of behaviorism, Bloor expected the sociologist of science to isolate the 'stimulus' in a 'sufficiently neutral data-language' to explain the 'response', which in the case of scientists was not a reflex but a belief or theory (ibid.: 92). Yet Bloor's positivism was broken, just as that of the Churchlands. They had all learned from Quine that theories were underdetermined by the evidence. While the Churchlands looked to the brain for answers to the epistemological question of how scanty observations ultimately led to comprehensive theories, Bloor assumed that the scientists' torrential responses to meager stimuli were mediated by society. Itself part of nature, society would fill in the blanks left open by the empirical investigation of 'non-social nature'. Bloor's naturalism did not deny the agency of research objects (e.g. to stimulate sense organs). As a matter of methodological principle, however, the sociologist had to treat both sides of a controversy symmetrically and assume that their diverging perspectives were equally determined by the contested subject matter. Shared research objects could not explain why two groups of scientists disagreed – only diverging social interests could. Bloor described himself as 'a realist (or materialist) about nature without assuming that any particular theoretical description of it is uniquely correct' (ibid.: 94). Regarding questions of ontology, Bloor remained a staunch positivist.

In contrast to this relativism of controversy studies, many laboratory ethnographies and micro-histories suggested that research objects manifest themselves more clearly in response to some inquiries rather than others (Latour, 1987). One research group manages to observe a phenomenon that does not materialize in the experiments of its competitors. The reason is not necessarily that the successful camp loaded its observations

with better theories, as Paul Churchland proposed. Instead the camp's members might have extended their senses with superior instruments (Hacking, 1983: 181–3). Where would neurophilosophy be if EEG and other functional neuroimaging technologies had not provided glimpses into the brain at work or in sleep? Science studies scholars such as Latour and Woolgar (1986[1979), Hacking (1983), Galison (1997), or Rheinberger (1997) transformed the psychological space of discovery into a space of experimental manipulation. They responded to what was widely perceived as a crisis of representation by relocating representation from the theoretical to the practical side of science. Representation was no longer regarded as a propositional account of the world but as the activity of instrumentally producing traces, images and artifacts, which enable scientists to better grasp and handle the objects of their investigation (Rheinberger, Hagner and Wahrig-Schmidt, 1997: 8–9). This work of mediation builds chains of referential associations between the empirical and the conceptual. Here it is human practices rather than neural processes that render representations possible. But, like neurophilosophy, science studies conform to the episteme of the human sciences as they conduct empirical research on the quasi-transcendental conditions of knowledge (see Foucault, 1973: 397).

Rheinberger (1992: 71) argued that the results of natural scientific research are not sentences but scientific objects about to become technological objects. The histories of molecular biology and physics are full of examples of entities investigated in one study, which, once they had been stabilized and mastered, came to serve as probes for another research object in the following study (Hacking, 1983; Rheinberger, 1997). The very materiality of this process allows for an interchangeability of object and instrument. Consequently, the ontological distinction between representation and what is being represented came to appear unstable and porous. Maybe it is no coincidence that Revonsuo, as a philosopher with extensive experimental experience, conducted a thought experiment centered on a new scientific instrument. Should his imaginary dream catcher/presenter ever be realized, it would be a wonderful instantiation of the process outlined by Rheinberger: the neurophysiological measurements from a dreaming subject are used to intervene in the brain of a second person who would get to experience the same dream. What used to be the object of research would become an instrument. Following Hacking's (1983) proposal to be anti-realist about theoretical entities and realist about entities that can be used experimentally to manipulate other entities, the workability of Revonsuo's dream catcher would decide the controversy between dream positivists such as Malcolm and dream realists like Putnam or Churchland in favor of the latter.

I would like to end this article on a simultaneously provocative and reconciliatory note. The theory-dominated neurophilosophy of the Churchlands is not materialist enough. As a philosophy of science, it would profit from attending to the naturalization of science in science studies. At the same time, the 10-year moratorium on cognitive explanations of science imposed by Latour and Woolgar (1986[1979]: 280) has long run out. Giere and Moffatt's (2003) attempt to integrate cognitive anthropology with studies of the social and material culture of the sciences could serve as one model for how to overcome the antagonism between neurophilosophy and science studies. Both fields have been struggling to build an empirical philosophy on the ruins of positivist conceptions of science. In the face of shared problems such as how to overcome the gap between empirical and conceptual work, they might be able to learn from each other (for tentative

steps in this direction, see Langlitz, forthcoming-a, forthcoming-b). Maybe they will even find a way out of the impasse of anthropological thought, the way that Foucault (1973) awaited so impatiently.

But I do not wish to close with a plea for an even more unified science. To me, the value of any empirical philosophy lies in its openness and sensitivity towards the phenomena. Revonsuo's thought experiment did not translate into a laboratory experiment pinpointing consciousness in the dreaming brain. Consciousness did not inscribe itself in the electroencephalographic recordings as he had envisaged. It is not necessarily the reality of epistemic things that shines through in their resistance. Sometimes they fail to manifest because they actually do not exist. I confess that I too dream materialist dreams. But, as far as the neuroscientific evidence goes in 2015, it is still too early for scientific realism about consciousness in dreams (lucid dreaming might be a different matter; see LaBerge *et al.*, 1981). If Revonsuo's story had a moral, it would be that in the current epistemic situation neither neurophilosophers nor science studies scholars should prematurely dismiss a positivist attitude towards the ontology of certain things. The dreaming brain, for example.

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