What Naturalists Always Knew about Freedom: A Case Study in Narrative Sources of "Scientific Facts"

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1. TWO POPULAR SUMMARIES

Perhaps the central debate in German-speaking popular science over the last years was on brain research and its possible impact on our account of the human being, with freedom of will as the central issue. But there is more going on than just a renaissance of the old philosophical determinism/indeterminism debate, discussions are also extending to possible consequences for our conceptions of responsibility, guilt, crime and penal law.

Among the main participants in the debate are Wolf Singer, Wolfgang Prinz, and especially Gerhard Roth, the latter presumably having the most numerous readership among a wider audience due to some paperbacks in a high-class book series. All of them defend a clearly naturalist, determinist account of man. Slogans like "We don't do what we want, but we want what we do" have become a sort of naturalist *mantra*, and the protests of philosophers from almost all kinds of schools have only lead to slightly more diplomatic theses so far. The tension between such claims and common sense is usually handled by various sorts of conventionalism, epiphenomenalism or fictionalism about concepts like self, authorship and responsibility. In Roth's case, the naturalist account is combined with a sort of radical constructivism from the beginning. It is not my task to comment on the stability of such a philosophical blend, since much has been said on that by others.

One of the curious features of the debate is that the freedom issue is partly regarded as an a priori one, partly as an empirical one by the same people. On the one hand you can read theses like the following:

> In order to find out that we are determined we would not need the Libet experiments. The idea of a free human will is in principle incompatible with scientific reasoning. Science presupposes that everything that happens has its causes and that one can find these causes. For me it is not understandable that someone who does empirical science can believe that free, i.e. non-determined action is conceivable. (Prinz 2004, 22)[1]

On the other hand the same people put together ample empirical material that they consider to be evidence for determinism, and the reference to empirical findings is surely the decisive argument for the public reception and credit of these claims. Among these findings are of course the Libet experiments (in their refined form conducted by Haggard & Eimer),[2] results of the social psychologists Wegner & Wheatley, results of Brasil-Neto, Pascual-Leone et al. on actions under magnetic stimulation of the brain, and the early stimulation experiments of Penfield & Rasmussen and Delgado on open brains of conscious patients since the 1930s. Here is a popular nutshell-summary of these findings by Gerhard Roth:

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[1] "Um festzustellen, dass wir determiniert sind, bräuchten wir die Libet-Experimente nicht. Die Idee eines freien menschlichen Willens ist mit wissenschaftlichen Überlegungen prinzipiell nicht zu vereinbaren. Wissenschaft geht davon aus, dass alles, was geschieht, seine Ursachen hat und dass man diese Ursachen finden kann. Für mich ist unverständlich, dass jemand, der empirische Wissenschaft betreibt, glauben kann, dass freies, also nichtdeterminiertes Handeln denkbar ist." (All translations W. L.)

[2] Not only for the sake of brevity, I will not address the Libet/Haggard/Eimer experiments in this paper. Recent experiments by Herrmann et al. (2005) considerably reduced their relevance. These experiments confirm the suggestion spelled out by numerous interprets in the past that the readiness potential is not more than an unspecific expectation activity of the brain, and not a determination of the action. By combining the Libet-experiment with choice-reaction task, Herrmann et al. convincingly show that the readiness potential is already present before exposition to the relevant information, i.e. at a time when the willing process cannot even have *begun*.

> Test persons can subliminally (e.g. via masked stimuli) by experimental tricks, hypnosis or brain stimulation be caused to actions of which they later claim that they *willed* them (Penfield and Rasmussen, 1950; Wegner, 2002; Roth, 2003). (Roth 2004, 15; similar Roth 2006, 10)[3]

Dozens of similar summaries can be found in literature. And as they stand, they seem to provide a massive empirical backing for determinism. Even our strong feeling of authorship and control can be proven to be an illusion, so we are told, but authorship and control is traditionally regarded as one of the conditions for an ontologically respectable conception of freedom. Summaries like that find a broad audience, they are taken for granted by many people including philosophers, scientists from various disciplines, science journalists and science politicians. Sometimes such summaries even get a bit face-lifted, consciously or unconsciously. An example is the following passage from GEO, a popular science magazine with thousands of readers. In an otherwise very careful, critical and balanced article on the consequences of neuroscience, the German neuroscientist Franz Mechsner reports the state of research as follows:

> In his book *Das Gehirn und seine Wirklichkeit* Gerhard Roth, professor of brain research at the University of Bremen, describes experiments which are illustrative in this point. The experiments were carried out on patients whose skulls had to be opened for medical reasons. If certain cortex areas of the brain (which is insensitive to pain) were stimulated by electrodes, e.g. an arm could be raised. Asked for the reason of their movement, the patients regularly [regelmäßig] claim to have willed them. Stimuli in deeper structures like the thalamus also caused movements. But the patients perceived them as unintentional or even against their will. (Mechsner 2003, 81, my italics)[4]

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[3] "Man kann Versuchspersonen unterschwellig (z.B. über maskierte Reize) durch experimentelle Tricks, Hypnose oder Hirnstimulation zu Handlungen veranlassen, von denen sie später behaupten, sie hätten sie *gewollt* (Penfield and Rasmussen, 1950; Wegner, 2002; Roth, 2003)."

[4] Gerhard Roth, Professor für Hirnforschung an der Universität Bremen, beschreibt in seinem Buch *Das Gehirn und seine Wirklichkeit* Experimente, die hierzu Aufschluss geben.
"Vorgenommen wurden sie an Patienten, deren Schädel aus medizinischen Gründen geöffnet werden musste: Reizte man bei ihnen mit Elektroden am (schmerzunempfindlichen) Gehirn gewisse motorische Cortex-Areale, konnte sich

The problem here is not only that we get the wrong impression that these experiments were carried out just recently at the University of Bremen. Rather, the problem is the newly inserted word "regularly". This really leaves nothing to desire for the naturalist: It seems now that we have easily repeatable experiments with strict correlations. Mechsner's rendering of the matter is not an exception; in numerous similar texts it is suggested as commonplace that actions (i.e. behavior with an accompanying "inner side" like intentions, plans, explanations etc.) could be triggered by external stimulation of the brain.

Critical readers might become suspicious here. Beyond medico-technical problems, should it really so easily be possible to cause people to movements which they report as willed? Would not the test persons at least become suspicious after a certain number of rounds? If experts who really conduct experiments in empirical brain research are being asked about such findings, they usually answer like "… never heard. Of course you can cause various sorts of spasms, tremors, seizures, emotional outbursts, inhibitions, even movements of limbs by stimulation, but never *actions*. Test persons always report that these effects somehow came from outside, for example that they can't resist to a strange desire to move the arm, but in any case that these movements are not willed by them."[5]

2. SCOPE, CONSTRAINTS AND DISCLAIMERS

The thesis of my paper is that these seemingly robust empirical claims, as they are boasted by Roth and others, are flatly wrong. According to my investigation in the history of the alleged research, there are no empirical results showing that full-blown actions (i.e. behavior with an accompanying phenomenological appearance like intentions, a feeling of

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etwa ein Arm heben. Nach dem Grund ihrer Bewegung gefragt, behaupteten die Betroffenen regelmäßig, sie gewollt zu haben. Reize in tiefer liegenden Strukturen wie dem Thalamus lösten ebenfalls Bewegungen aus. Doch die Patienten empfanden sie als unbeabsichtigt oder sogar gegen ihren Willen zustande gekommen."

[5] For a summary of the literature see, e.g., Halgren and Chauvel 1993. Nothing of the material summarized here points to the direction of a stimulation of something like actions.

control and authorship) can be caused by brain stimulation and similar techniques as described by Roth and others.

This of course raises the question how such bold claims can emerge almost *ex nihilo* in the literature. I intend to show that this piece of neuromythology was created by a mixture of sloppy citations, confidence to hearsay, over-interpretations, confabulations, slight mistranslations, and confusions of probabilistic and strict correlations. Over the years, these mistakes seem to have established a narrative tradition dense enough to substitute empirical findings. Some naturalists obviously have always known what empirical research could only convey.

In order not to be misunderstood, some constraints and disclaimers on my agenda seem appropriate.

Firstly, the scope of this paper is in fact tiny—it is not more than a case-study. My question is only whether this particular, aforementioned claim that full-blown actions with the feeling of authorship can be caused by external stimulation is empirically warranted. Though my answer here will be to the negative; I do of course not doubt that there is a mass of evidence that actions, decisions and perceptions can be influenced and biased in countless ways.

Secondly, my claim is a purely factual one, not an "in principle" one. I only show that the purported results from the past do not prove what they are supposed to prove. I do not exclude that someone at some time could perhaps really design an experiment where it is plausible that full-blown actions can be triggered.

Thirdly, I do not aim at defending any particular account of human freedom, especially not an incompatibilist one. I just scrutinize the empirical backing of some claims.

Lastly, I do not want to promote any postmodernist ideas (of science as a whole as narrative, etc.). When talking about narrative, I mean it in the straightforward, all-day sense and not in the sense of Lyotard and others. But I found no better word to label the astonishing development that will henceforth be described.

3. DISENTANGLING PROBABILISTIC AND STRICT CORRELATIONS

Let us start with a look at one of the more elaborate and detailed summaries that Gerhard Roth offers about earlier research:

> Electrical stimulations of the cortex were amply conducted by the Canadian neurologist Wilder Penfield since the 1930s. […—here follows a closer description of the epilepsy patients, W. L.]. Stimulation in points of the somatosensory cortex directly in front of the central fissure lead—depending on the place—to a tingling in certain parts of the body, stimulation of the primary motor cortex to spasms of particular muscles or muscle groups, stimulation of the premotor and supplementary motor cortex to complete movements of limbs (Penfield 1958). The patients reported they could not resist these movements, they perceived them as "forced upon them". Conversely, under stimulation of certain areas in these premotor areas they were not able to execute movements they *wanted* to execute, i.e. cortex stimulation lead to an inhibition. In a number of cases, however, stimulation of a cortex area near the foot of the central fissure at the border to the Sylvic fissure reliably lead to the will resp. desire to move the left resp. right hand or the left or right foot (Penfield and Rasmussen 1950).
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> The Spanish neurologist José Delgado reported that under similar conditions as in Penfield stimulation of the rostral part of the so-called internal capsule [i.e., …] lead to movements of the patient which he ascribed to himself. Similarly, by transcranial magnetic stimulation (TMS) the neurologist Brasil-Neto could cause finger movements which the test person described as "willed" (both results cited after Wegner 2002). (Roth 2003, 515f)[6]

[6] "Elektrische Reizungen der Hirnrinde wurden extensiv vom kanadischen Neurologen Wilder Penfield seit den dreißiger Jahren des vorigen Jahrhunderts durchgeführt [... – hier folgt eine nähere Beschreibung der Epilepsiepatienten]. Eine punktuelle Reizung des somatosensorischen Cortex direkt vor der Zentralfurche führte je nach Ort zu einem Kribbeln in bestimmten Körperteilen, eine Reizung des primären motorischen Cortex zu Zuckungen einzelner Muskeln oder Muskelgruppen, eine Reizung des prämotorischen und supplementärmotorischen Cortex zu kompletten Bewegungen von Gliedmaßen (Penfield, 1958). Die Patienten berichteten dabei, sie könnten diesen Bewegungen nicht widerstehen, sie kämen ihnen ‚aufgezwungen' vor. Umgekehrt waren sie bei Reizungen bestimmter Areale in diesen prämotorischen Arealen nicht in der Lage, Bewegungen auszuführen, die sie ausführen wollten, d.h. die Cortexstimulation übte eine Hemmung aus. Bei einer

We see that Roth refers to three groups of findings (by Penfield & Rasmussen, Delgado, Brasil-Neto), and as a bundle they apparently make a strong case for the possibility to stimulate full-blown actions. All of them sound like strict correlations between stimulation and action. But a closer look reveals that they are not all of that same kind: the last-mentioned experiment by Brasil-Neto, Pascual-Leone and others (Brasil-Neto et. al. 1992) only conveyed a weak probabilistic correlation. The experiment ran as follows: Test persons were instructed to arbitrarily move either the left or right finger. When their motor cortex was stimulated by magnetic pulses on the left or right hemisphere, they moved the opposite finger somewhat more frequently, although they subjectively believed in a free choice. This probabilistic dependence was only present when the movement took place within 200 milliseconds after the pulse, it disappeared at later movements. Hence Roth's description that "Brasil-Neto could cause finger movements which the test person described as 'willed'" is wrong: the general order to move came from the researchers, only the time of movement was at the test-person's choice, just some property of the movements was probabilistically influenced by the stimulation to a small extent. No actions were caused at all, and the feeling of control was only deceived in respect of the probability of left and right.

A similar comment applies to the experiments of Daniel Wegner and Thalia Wheatley (Wegner & Wheatley 1999), two social psychologists whose results are also often used by Roth and others (although not here in this particular summary). The point here is again a purported illusion of control, but the test-persons' feeling of control was only deceived about the percentage of their share in the common action of two people. The design

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Reihe von Patienten führte jedoch die Stimulation eines Cortexareals am Fuß der Zentralfurche im Übergang zur Sylvischen Furche zuverlässig zum Willen bzw. Bedürfnis, die linke bzw. rechte Hand oder den linken oder den rechten Fuß zu bewegen." (Penfield und Rasmussen, 1950)

"Der spanische Neurologe José Delgado berichtete, dass unter ähnlichen Bedingungen wie bei Penfield die Stimulation des rostralen Anteils der so genannten internen Kapsel (d.h. ...) zu Bewegungen des Patienten führte, die er sich selbst zuschrieb. Ähnlich konnte mithilfe der Transkranialen Magnetstimulation (TMS) der Neurologe Brasil-Neto Fingerbewegungen auslösen, die die Versuchsperson als ‚gewollt' beschrieb (beide Befunde zitiert nach Wegner, 2002.)"

of the experiment is somewhat complicated: Two test-persons operating something like a two-handed computer mouse were instructed to draw approximate circles on a screen which was full with pictures of objects. Every half minute they had to bring the cursor to a stop without communicating about the place to stop. Afterwards, the persons had to judge on a percentage scale whether they had rather *intended* or just *allowed* the stop just here. As a modest distraction, the test persons heard unconnected words via headphones. In fact, one of the test persons was a confederate of the researchers. Between un-manipulated rounds, this person got the headphone command to move the cursor to a certain picture following a count-down. Hence, the stops in these rounds were primarily the effect of the confederate. Nevertheless, the real test person perceived these stops as effects of "his" action at an unduly high percentage. The percentage was especially high when the noun corresponding to the stopping-place object had recently been heard via headphone. Hence, the experiment shows that one can induce illusions about control and authorship which are—at least gradually—incorrect.[7]

No doubt, both results are interesting, but they are not groundbreaking news. That people can be manipulated in their freely chosen actions by chemical, linguistic and other means, that they can even be gradually deceived about their authorship, all that has been familiar since millennia, and whole industries live from that. (Wegner & Wheatley admit that low-budget variants of such experiments can be carried out with a bowl of salted peanuts beside your TV chair). But the results discussed so far cannot be described as cases where test persons are determined to perform actions which they wrongly attribute to themselves. The experiments by

[7] Wegner and Wheatley (loc. cit. 488f.) themselves admit some methodological problems concerning the experiment. The number of successful manipulated rounds is rather low (27-40 responses from 51 participants were valid at each of the four time-points checked, and only eight participants had valid responses across all four trials). The reason is that it was sometimes difficult or impossible to move the cursor to the desired stopping-place. One might also worry whether test persons after a number of manipulated rounds do not become suspicious about a possible bias. Another problem may lurk behind the fact that the manipulated rounds were inserted after a number of rounds where the stopping-decision was completely left to the real test persons. This might cause a general over-estimation of their personal share in the common action.

Brasil-Neto, Pascual-Leone *et. al.* and Wegner & Wheatley do not provide the empirical basis for the bold claims in the summaries mentioned at the beginning.

4. A MORE PROMISING EMPIRICAL BASIS? PENFIELD'S & RASMUSSEN'S "OPEN HEAD" EXPERIMENTS AND DELGADO'S PATIENT

Let us consequently turn to the other two results invoked by Roth: the old findings by the pioneers of neuroscience Penfield & and Rasmussen and by Delgado, dating back to the 1930s to 70s, when experiments at the open skull with conscious patients faced less bioethical worries than today. Penfield and Rasmussen (1950) found out that electrical stimulation of certain points of the cortex lead to various forms of tingling, spasms, emotions, movements or a felt strange desire in the limb to move. But the patients always described these effects and desires as coming from the outside, or as being forced upon them. Here are the two most interesting cases:

> CASE 7. […] A further unexpected response was that at [point] 23, on the border of the fissure of Sylvius. When this point was being stimulated, she said she felt as though she wanted to move her left hand. To verify this sensation, the operator tried to "trick" the patient by warning her that he was stimulating when he did not so. This produced no such desire. He then warned her similarly when he did stimulate. She then reported the same desire to move her left hand. […]
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> CASE 8. […] When H. was stimulated, he hesitated; then he said, "My hand wants to tremble a little." He referred to his right hand (ipsilateral). The hand did tremble and continued a little time after stimulation was withdrawn, but he stopped the trembling voluntarily.
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> [From the explanation to Fig. 68 on Case 8]: Stimulation at [point] H produced desire to move right hand. (W. Penfield / T. Rasmussen, *The Cerebral Cortex of Man* (1950), 120-122)

Notice the constructions "she felt as though she wanted to move her left hand" and "she reports the desire to move her left hand"; we shall come back to them later on. It is more than clear that "reporting a desire" to

move a limb is not the same as "having the intention or the wish" to move it. What Penfield and Rasmussen caused by stimulation is obviously not an action in the described, full-blown sense, but rather a strange feeling as if one's limb wanted to move.

The other source indirectly cited by Roth are the electrode experiments by the Hispano-American neurologist José M. R. Delgado,[8] also dating back to the 1950s to 70s. Delgado first summarizes a mass of experiments yielding similar results to Penfield and Rasmussen: externally stimulated tinglings and other feelings, tremblings, movements, inhibitions of movements and the like (114f). What follows then is a little note on one patient, whose case is the starting point for an astonishing example of scientific hearsay, as we shall see. Here is Delgado's original text from his book *Physical Control of the Mind. Toward a Psychocivilized Society (1969):*

> In contrast to these effects, electric stimulation of the brain may evoke more elaborate responses. For example, in one of our patients, electrical stimulation of the rostral part of the internal capsule produced head turning and slow displacement of the body to either side with a well-oriented and apparently normal sequence, as if the patient were looking for something. This stimulation was repeated six times on two different days with comparable results. The interesting fact was that the patient considered the evoked activity spontaneous and always offered a reasonable explanation for it. When asked "What are you doing?" the answers were, "I am looking for my slippers," "I heard a noise," "I am restless," and "I was looking under the bed." (Delgado, 115f.)

Notice that Delgado himself gave a very cautious and unspectacular interpretation of these observations and their relevance. He comments on the scene as follows:

> In this case it was difficult to ascertain whether the stimulation had evoked a movement which the patient tried to justify, or if a hallucination had been elicited which subsequently induced the patient to move and to explore the surroundings. (loc. cit. 116)

In private correspondence (March and April 2007) Delgado told me that he still considered these attempts of an interpretation as correct, and showed a

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[8] On Delgado's life and works see Horgan 2005.

preference for the first one: the stimulation evoked a movement which the patient could not integrate, and the patient tried to give some *ex-post*-explanation for it. This phenomenon was repeatable, but (as the text in his book had already indicated) the content of these explanations differed between the rounds.[9] That means, Delgado's patient was apparently a case of the familiar phenomenon of rationalization and not a case of an external stimulation of an action.

As Delgado confirmed to me in private communication (10th April, 2007), the note in the 1969 book is the only appearance of this patient in his numerous publications.[10] This provides further evidence that the case of

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[9] "Repetition of ESB [=electrical stimulation of the brain, W. L.] showed that the evoked behavior was reliable but the patient gave different explanations for the movement which was not in his usual repertoire. He did not say that he had initiated the movement for a purpose: he tried to explain it 'after the fact.'" (J. M. R. Delgado, personal communication, 10th April, 2007).

[10] The bibliography of the book lists 21 articles with Delgado as principal author, and 10 with him as a co-author. I retrieved and checked all these 21 articles (and some additional ones with potentially relevant titles), but none of them documents Delgado's patient or similar cases. All these articles just cover medical and technical aspects of electrode implantation and stimulation, or lengthy rows of experiments with monkeys and cats, or they provide data about the various sorts of stimulation effects we already know. As an illustration I summarize the content of the six articles with the most promising titles: *Behavioral Changes During Intracerebral Electrical Stimulation* (Higgins, Mahl, Delgado and Hamlin 1956) reports *déjà vu* phenomena and various changes in perception and verbal and bodily behavior which took place when the brain of an 11-year-old psychomotor epileptic with previous lobotomy was stimulated. These forms of behavior seem rather complex (yet highly irrational), but the boy provides no case similar to our patient. *Emotional Behavior in Animals and Humans* (Delgado 1960) reports stimulated changes in verbal and emotional behavior, movements and *déjà vu* phenomena, but nothing like stimulated actions. *Effect of Brain Stimulation on Task-Free Situations* (Delgado 1963, listed as "in press") reports experiments with Rhesus monkeys. *Psychological Responses in the Human to Intracerebral Electrical Stimulation* (Mahl, Rothenberg, Delgado and Hamlin 1964) reports how stimulation lead to linguistic and ideational effects in one patient with intractable psychomotor epilepsy. *Free Behavior and Brain Stimulation* (Delgado 1964) is a 100 pages summary about stimulation experiments with monkeys; the wording "free behavior" just refers to the fact that the stimulation could now be done via radio-control and not—as previously—with wires that restrained the free mobility of the animals; *Intracerebral Radio Stimulation and Recording in Completely Free Patients*

his patient had by far not the importance that was ascribed to him in the subsequent narrative chain. If there had really been something like a stimulation of a free action, such a sensational result would surely have deserved an appropriate publication.[11]

5. FROM MOLE-HILLS TO MOUNTAINS: HOW NARRATIVE INFLATION WORKS5.1. The evidence so far

Let me summarize what we have found out as the empirical basis for the claims in question: There is

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(Delgado et al. 1968) reports the application of this new technology (i.e. radio-controlled stimulation and EEG recording) for the clinical treatment of four psychomotor epileptics. Assaulting behavior reminiscent of earlier outbursts could be elicited by stimulation of the amygdala, but there is no evidence for the stimulation of "actions" either.

[11] It is also illuminating to compare the later career of our passage from *Physical Control of the Mind. Toward a Psychocivilized Society*" with its original place and character. The full text of this book is available on the internet and can easily be retrieved via search-engines, but it is rewarding to hold a paper copy of it really in one's hand and to inspect it. It turns out as a paperback for a wider audience from the 1960s multi-disciplinary book series *World Perspectives* (other volumes in the series were, e.g., Jacques Maritain's *Approaches to God*, Werner Heisenberg's *Across the Frontiers* and Ivan Illich's *Deschooling Society*). The book contains a popular overview of contemporary brain research, especially under the respect of the possibilities of controlling and influencing socially problematic behaviour. In retrospect, we might perhaps not share Delgado's unbroken optimism in this point today, some commentators even ascribed a somewhat evangelical tone to the book (see Horgan 2005 and the critical literature mentioned there), but in any case it is an interesting document of its time, the history of neuroscience and its public perception. Although designed for a wider audience, the book also contains an extensive bibliography of approx. 240 research papers, some of them with titles which are *prima facie* promising for our issue (see my footnote 10). This appearance may perhaps have lead Wegner to overestimate the importance of the aforementioned case in his book *The Illusion of Conscious Will* (see chapter 5.2 below). We may speculate that Wegner was confident that a proper documentation of the patient could easily be found in one of Delgado's 31 listed papers. In Gerhard Roth's text again, where Delgado is only indirectly cited via Wegner's book, all of this prehistory is completely concealed. From Roth's text alone, the reader gets the impression of a robust, well-documented state of research.

(1) fairly good evidence for some slight and gradual deceptions about control and authorship, which however cannot be described as external determinations to actions; moreover, we have
(2) good evidence for replicable stimulations to movements and desires to move which, however, are perceived as "forced upon" by the patients. And we have
(3) one single case of a seeming stimulation of an action which is not considered as very relevant by the researcher himself. The case is not documented in research papers but only mentioned in a book for a wider audience.

I know of no other evidence which could be interpreted as the external stimulation of an action. How can the way from this poor empirical basis to the bold claims cited at the beginning be reconstructed? How can one make a mountain from a mole-hill?

5.2. Wegner's creation of the "feeling of doing"

A hub of the recent debate is Daniel Wegner's 2002 book *The illusion of conscious will*. Wegner collects and evaluates a variety of arguments which seem to point against free will. As Roth himself admits, this book is also his source on Delgado and Brasil-Neto.

Here is Wegner's report on Delgado's patient. Having summarized Penfield's research, Wegner comments and proceeds as follows:

> […] The movements Penfield stimulated in the brain were smooth movements involving coordinated sequences of the operation of multiple muscles, which looked to have the character of voluntary actions, at least from the outside (Penfield and Welch 1951; Porter and Lemon 1993). They just didn't feel consciously willed to the patient who did them. In this case, then, the stimulation appears not to have yielded any experience of conscious will and instead merely prompted the occurrence of voluntary-appearing actions.
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> Penfield's remarkable set of observations are strikingly in counterpoint, though, with those of another brain stimulation researcher, José Delgado (1969). Delgado's techniques also stimulated the brain to produce movement, but in that case movement that was accompanied by a feeling of doing. Delgado (1969) reported,

> In one of our patients, electrical stimulation of the rostral part of the internal capsule produced head turning and slow displacement of the body to either side with a well-oriented and apparently normal sequence, as if the patient were looking for something. This stimulation was repeated six times on two different days with comparable results. The interesting fact was that the patient considered the evoked activity spontaneous and always offered a reasonable explanation for it. When asked "What are you doing?" the answers were, "I am looking for my slippers," "I heard a noise," "I am restless," and "I was looking under the bed." (Delgado, 115-116)

Wegner continues his comment as follows:

> This observation suggests, at first glance, that there is indeed a part of the brain that yields consciously willed action when it is electrically stimulated. However, the patient's quick inventions of purposes sound suspiciously like confabulations, convenient stories made up to fit the moment. The development of an experience of will may even have arisen in this case from the stimulation of a whole action-producing scenario in the person's experience. In Delgado's words, "In this case it was difficult to ascertain whether the stimulation had evoked a movement which the patient tried to justify, or if an hallucination had been elicited which subsequently induced the patient to move and to explore the surroundings (1969, 116). (Wegner 2002, 45-47)

Wegner's rendering of Penfield's and Delgado's findings is basically correct, and especially it reflects Delgado's cautious interpretation of the behavior of his patient. This interpretation is not only repeated in a literal quotation, it is even underlined by Wegner's subsequent commentary. (Wegner's summary of Brasil-Neto's magnetic stimulation experiments—which I skip here for brevity—is also correct.) Problematic, however, is Wegner's introductory remark on Delgado which may direct the readers into a certain way of looking at things. Firstly, it is misleading to say that there is a "striking counterpoint" between Penfield and Delgado (this is not the case according to Wegner's own subsequent interpretation!), and secondly, the announcement that here we have a "movement that was accompanied by a feeling of doing" is a biased interpretation not warranted by Delgado's original text. At most one could perhaps say that the patient gave *ex-post*-rationalizations of his movements, or—to modify Wegner's words—he made "movements followed by a feeling of having done."

The most important thing that has changed by Wegner's compilation is the context: As its title suggests, Wegner's book is something like a list of pro-determinist arguments, and it connects arguments of very different kinds. For instance, it is Wegner who creates the bundle Penfield/Delgado/Brasil-Neto that will uncritically be taken on by Roth. The hasty reader of Wegner's text may overlook the difference, e.g., between probabilistic and strict dependencies, and in effect the empirical case for action-stimulation may look much stronger than it actually is. However, we should not blame Wegner for that. The critical reader can still keep things apart—*if he wants*.

A last problematic point, yet one of minor importance, is Wegner's lifting of *Physical Control of the Mind* into the rank of an empirical source-book. Reading Wegner, even the critical reader may now confidently believe that Delgado's patient—may he be important or not—is at least a well-documented case. We shall see that all these problematic points will reappear in Roth's account of the issue.

5.3. Roth's creation of the "will to move"

The next and crucial step of obfuscation is done by Roth himself. Let us first compare Penfield & Rasmussen 1950 with Roth 2003. Remember the constructions "she felt as though she wanted to move her left hand" and "she reports the desire to move her left hand" by Penfield and Rasmussen. In his own rendering of these results, Roth inserts two words which completely change the meaning (italics W. L.):

> In a number of cases, however, stimulation of a cortex area near the foot of the central fissure at the border to the Sylvic fissure reliably lead to the *will resp.* desire [zum *Willen bzw.* Bedürfnis] to move the left resp. right hand or the left or right foot (Penfield and Rasmussen 1950). (Roth 2003, for the German original see footnote 6)

As we said before, "reporting a desire to move" is clearly not the same as "having the will to move", but Roth's mistranslation turns the meaning of the text in that direction. A similar observation can be made concerning Roth's use of Delgado's patient. Changing the overall message of the text into its opposite is especially easy here, namely by simply cutting away Delgado's and Wegner's skeptical postscripts. Roth also changes the

construction so that the fact that it is only *one* patient is concealed: "the patient" now appears as an abstraction (the patient in general!), and not as a reference to one particular person as in Delgado's text above. (Roth's construction "in a number of cases …" shortly before may further foster this wrong impression). And finally, if the description of Brasil-Neto's probabilistic results (false as it is anyway!) is placed immediately after the incomplete description of Delgado's patient, the reader gets the completely false impression that magnetic transcranial stimulation works as reliably as electric stimulation of the brain.

5.4. A synopsis of the textual changes

To get a synopsis of the textual changes, let us finally have a second look at Roth's core text about the empirical findings backing his claim, this time equipped with more background knowledge. The reader is invited to read the text twice, once as it stands for itself, and once including my comments (in <*italics*> and reduced type size) which mark the places where the earlier textual tradition underwent important changes.

> "In a number of cases, however, stimulation of a cortex area near the foot of the central fissure at the border to the Sylvic fissure reliably lead to the will resp. desire <*mistranslation, unwarranted insertion of "will resp."!*> to move the left resp. right hand or the left or right foot (Penfield and Rasmussen 1950).
>
> The Spanish neurologist José Delgado reported that under similar conditions as in Penfield stimulation of the rostral part of the so-called internal capsule [i.e., …] lead to movements of the <*"the" suggests generality!*> patient which he ascribed to himself. <*Delgado's & Wegner's skeptical postscripts on the patient are omitted!*> Similarly <*conceals the difference between strict and probabilistic correlations!*>, by transcranial magnetic stimulation the neurologist Brasil-Neto could cause finger movements which the test person described as "willed" (both results cited after Wegner 2002)." (Roth 2003, 516; for the German original see footnote 6.)

This strikingly inadequate use of the actual evidence might perhaps raise the suspicion of deliberate manipulation. But we should be hesitant with such a verdict; a massively biased look at evidence known from second hand, based on a firm conviction what data could only be expected, is probably the better explanation.

6. CONCLUSION

We may conclude that Roth's claim that actions (in the full-blown, phenomenologically rich sense) can be triggered by external stimulation, is not warranted, at least not by the evidence he refers to. This might suggest a more general lesson. At the beginning I mentioned the question whether the determinism problem is an *a priori* matter or can be solved on empirical grounds. I deliberately left this question open at that point. But a partial answer can be given in any case: it is surely not fruitful to treat it with *false* empirical premises. *

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