



International Journal of Clinical and Experimental Hypnosis

Publication details, including instructions for
authors and subscription information:

<http://www.tandfonline.com/loi/nhyp20>

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Version of record first published: 31 Jan 2008

To cite this article: Steven Jay Lynn & Scott Lilienfeld (2002): A critique of the franklin commission report: Hypnosis, Belief and suggestion, International Journal of Clinical and Experimental Hypnosis, 50:4, 369-386

To link to this article: <http://dx.doi.org/10.1080/00207140208410111>

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A CRITIQUE OF THE FRANKLIN COMMISSION REPORT: *Hypnosis, Belief, and Suggestion*

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Abstract: This article critiques the landmark Report of the Commissioners Charged by the King to Examine Animal Magnetism, now widely known as the "Franklin Report." The authors mount a defense of D'Eslon, the disciple of Mesmer who conducted the "experiments," designed by the Commissioners that debunked animal magnetism as the mechanism responsible for dramatic alterations in behavior and medical cures following the application of Mesmer's procedures. The authors identify deficiencies in the commissioners' methods, discuss difficulties inherent in drawing strong inferences from the experiments they conducted, and contend that the commissioners missed an opportunity to elucidate the manifold ways in which mesmerism mapped onto important psychological constructs and phenomena. The authors adopt a fanciful approach by couching their critique in a sympathetic response to D'Eslon, who appears to one of the authors in a dream and voices his reservations about the commissioners' efforts.

Tossing, turning, restless legs and all, I try one self-hypnotic technique after another. They usually work, mind you, but this time, and every time this week, no luck; at best, a fitful sleep. Same dream. Same team: Franklin and D'Eslon.

It is 1784. I'm somewhere in Paris, but it's not exactly the "City of Light." I'm in a dank catacomb, seated among rows of corpulent aristocrats in a *baquet*, a circular vat made of oak, raised about a foot and a half off the ground. We are chained together by a rope looped around our bodies. My thumb is pressed between my pale neighbor's thumb and index finger; the powder from his wig falls onto my arm. Bent iron rods protrude from the covering of the *baquet*, one rod to a person. The sounds of the pianoforte do little to drown the din of the nattering

Manuscript commissioned May 1, 2001; final revision received June 20, 2002.

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The *International Journal of Clinical and Experimental Hypnosis*, Vol. 50, No. 4, October 2002 369-386
DOI: 10.1177/002071402237721

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aristocrats who, as if on cue, convulse in seizures or fits of coughing as they experience "crises" induced by the forces of animal magnetism that coruscate through the iron rods of the baquet.

At first I am confused, disoriented. I have no idea why I am here, and then I remember: I am a reluctant participant in an experiment conducted by Benjamin Franklin and his colleagues. Franklin, along with Lavoisier, Guillotin, and other scientific luminaries of the time, had been commissioned by King Louis XVI to test the claims of mesmerism, to examine the effects of animal magnetism on maladies of all sorts. And in my dream, they do what they want with me; to them I am no more than an object, like the multifarious magnetized doors, cups, and trees with which I am brought into contact to test my reactions. It seems the testing will continue interminably.

The commissioners are understandably skeptical of Mesmer's claim that an invisible physical fluid—animal magnetism—flows through sentient beings and, when unbalanced or blocked, causes disease. According to Mesmer, the equilibrium of the fluid and precious health can be restored by the repeated occurrence of a crisis, typically marked by convulsions or lethargy. The spectacle of the crisis is so extraordinary that it inspires a blend of astonishment and skepticism in the commissioners, which hardens their mounting incredulity about animal magnetism. The commissioners seem no more taken with the curative powers of animal magnetism than Mesmer was with the role of exorcism in the cures of his predecessor, Father Gassner, whose gravestone inscription bears testimony to him as the most celebrated exorcist of his time.

So it comes as no surprise when, from behind large oak doors, I hear Franklin and Lavoisier, the chief architect of the experiments, snicker at D'Eslon, Mesmer's disciple, who vouches for the existence of animal magnetism and stands in his stead to perform the tests orchestrated by the commissioners. One might expect that D'Eslon would magnetize me in manic flourishes, with abandon and "passes" of his arms over my body or by pressing his fingers on my lower abdominal area. But instead, he faces me, we touch fingers, and he stares for prolonged periods into my eyes. All of this is intended to coax a response from me that will give proof to the power of magnetism. Yet it all seems pointless: The commissioners have already made up their minds. They have concluded that animal magnetism and the medical cures it is supposed to effect are hokum. They have decided that D'Eslon is at worst a fraud and at best self-deluded.

He knows exactly what the inquisitors think; their disdain for D'Eslon is evident, even under the cloak of smug professionalism. His frustration is as apparent as his passionate desire to vindicate himself and his magnetic cure; yet with each experiment, he loses ground. It seems that whether I have a crisis or react to a magnetized object in some more subtle way depends entirely on whether I believe it is magnetized. When I

am told it is magnetized, and I approach the object, I feel faint and I convulse, even when the object is not actually magnetized. And when I don't believe an object is magnetized, I never react, even though D'Eslon has magnetized the object beforehand. Poor D'Eslon, it seems that the powers of animal magnetism are "all in the mind."

Every night the dream ends the same way: D'Eslon is not vindicated. I see his pleading face mouthing the words, "Give me voice; tell the truth!" My body jolts as I awaken. Animal magnetism? No, it's just the sensation I have experienced so many times before as I have transitioned to sleep or wakefulness. I'm awake. Wide awake.

Should I try to sleep now? Who am I kidding? I'll be haunted until I finish writing the article Michael Nash commissioned for the special issue of the *International Journal of Clinical and Experimental Hypnosis*. The assignment has not been easy to complete; I've struggled these past nights, hence my nightmares. Unfinished business. Mike has asked me to critique the landmark "Report of the Commissioners Charged by the King to Examine Animal Magnetism," now widely referred to as the "Franklin Report." The report concludes that the observed effects of animal magnetism can be attributed to the effects of imagination, touching, and imitation, rather than magnetic ministrations. No wonder D'Eslon's plea disturbs my sleep.

But can I write the critical piece Mike requires, when I honor the report? Does it not summarize 18 clever experiments? Does it not occupy a cherished place in the history of science? Does it not represent one of the first "skeptical" treatises that dismantles plausible yet unfounded extraordinary claims by dint of careful research? How can I tell the truth from the point of view of D'Eslon, who plays the fool on the stage of history to Franklin's part as the artful debunker?

And yet, strangely enough, if I close my eyes, I can imagine D'Eslon on some ethereal plane, doggedly reading scholarly research, assimilating texts and journals, searching for any small vindication of Mesmer and, by extension, his own life's work. How happy D'Eslon feels when he encounters Ellenberger's (1970) assessment that "There is no doubt that the development of modern dynamic psychiatry can be traced to Mesmer's animal magnetism, and that posterity has been remarkably ungrateful to him" (p. 69).

An image comes to mind of D'Eslon, a lurker on the Internet, buoyed by John Kihlstrom's (2001) comment that "one could argue that, if the Franklin Commission's experiments were a triumph for the experimental investigation of treatment, then Mesmer's own, earlier, demonstration that he could duplicate the outcomes of Father Gassner's exorcisms with his own, purely materialistic, treatment were in their own way a triumph of natural science over supernatural theories of illness." And I can see D'Eslon's visage, so pathetic in my dream, morph into a delighted smile as he reads on:

... Let's not give too much credit to a group of men who, by virtue of adopting some version of Cartesian-Kantian dualism, didn't think that the mind could be studied scientifically, and thought that mere 'imagination' could play no role in a scientific explanation.

And it's not difficult to imagine that I can commune with the invigorated spirit of D'Eslon, his aura strong, illuminated by the knowledge he acquired since those dark days when Franklin and his ilk mounted their attack on him.

Perhaps I can give D'Eslon a voice in 2002 in the light of time and history and update his inchoate views with the benefit of 218 years of subsequent scientific thought and research. Even more important, maybe if D'Eslon can rest, then so can I. My plan is to first translate as faithfully as I can the words of D'Eslon that continue to haunt me, night and day. Then Scott Lilienfeld and I will argue that D'Eslon and Mesmer, despite their errors in methods and reasoning, were on to something important and that the Report of the Commissioners was too eager to toss out the baby with the bath water. First is the voice of D'Eslon as I hear it.

D'ESLON SPEAKS

I rose to my own defense in my paper, "Observations on the Two Reports of the Commissioners Named by the King to Investigate Animal Magnetism" (D'Eslon, 1784/1965).² I wrote then that my goal was to

... review rapidly the facts which the Commissioners set forth and the conclusions which they draw from these facts in their own words. If by these simple means I do not demonstrate the reality of the agent which they contest, I will at least prove that they have not demonstrated either its nullity or its danger. (p. 8)

Those written comments reveal my awareness of the difficulty of providing conclusive proof of the existence of animal magnetism. But I was convinced at the time I penned the defense of my procedures, and I am still convinced, that the commissioners failed to make an air-tight case against the existence of animal magnetism.

Please understand that, like Mesmer, I was the product of my times, so I attributed the unusual behaviors and cures I witnessed to a physical rather than a supernatural or mystical agent. Was my attempt to arrive at a rational explanation not progress? Is it not understandable that I might come to place my faith in the power of animal magnetism at a time when psychological theories of human behavior were not well developed, much less refined and subjected to empirical test? Today, I would have a fine menu of choices to account for the quite remarkable events and cures I observed in the more than 500 patients in my care, but in the times

² All of the quotations attributed to D'Eslon, with cited page numbers, were taken from D'Eslon (1784/1965), which was reprinted in Shor and Orne (1965). All of the quotations attributed to the Franklin Commission, with cited page numbers, were taken from Franklin et al. 1784/1996, from the *Skeptical*.

in which I lived, you can surely understand that my options were limited. What was I to think? Did not many cures and crises follow in the wake of the procedures I used to manipulate the forces of magnetism? For me, this observation constituted presumptive evidence of the powerful role of magnetic forces in instigating cures. After all, the alternative explanation would have us rely on demons, newts, or frogs as active agents.

Then, as now, I strongly reject the legitimacy of denying the existence of a universal fluid on the sole ground that it is neither visible nor tangible. Back then I wrote, "Because it is imperceptible as such, the existence of animal magnetism can be demonstrated only in the treatment of disease" (p. 9). As you might expect, I was delighted that the commissioners agreed to adopt this approach. In fact, early in their investigation, they were impressed with crises that occurred in a group and were moved to say that all patients

... submit to the magnetizer; even though they may appear to be asleep, his voice, a look, a signal pulls them out of it . . . one cannot help but acknowledge the presence of a great power which moves and controls patients and which resides in the magnetizer.

In the first moment of their surprise the Commissioners devoted themselves wholeheartedly to the examination of the patients. Yet they failed to give an account of some of these cases. (p. 9)

In fact, the commissioners excluded cases in which improvement in a particular condition could be attributed to animal magnetism. Three cases I singled out involved reduction in the swelling of the stomach of a woman who was afflicted with dropsy, increased mobility in a woman's injured arm and forearm, and diminished size of swollen glands in the neck and armpit in a 10-year-old child.

Other impressive demonstrations also never made it into the commissioners' report. In one instance, I made a pain descend from one side of a soldier's head to his stomach and side. When my foot approached his, I caused him to experience a warmth that I transposed to the other foot, besides directing cold and heat through his body at will. Or consider the woman I magnetized at Mr. Franklin's villa at Passy. She did not see me. Nonetheless, I magnetized her by reflection in a mirror, even though she had her back turned to the mirror. And, finally, there was the woman who was in a crisis and completely unconscious, with her arm held aloft, palm facing outward in a state of contraction. With a mere presentation of my index finger, I made the palm turn toward her body.

Given the suppression of exceedingly important cases, is it any wonder that the gentlemen of the Royal Society wrote:

We considered it necessary to neglect rare, unusual and marvelous cases, such as the renewal of convulsive movements merely by pointing a finger or an iron conductor through the back of a well-upholstered chair or through a door or a wall; and also sensations experienced approaching a

tree, a pond, or any body or place formerly magnetized (Report of the Royal Society of Medicine, p. 21, cited in D'Eslon, 1784/1965).

How then to prove the contested existence of an unknown agent if some observers refuse to examine its curative effects in the treatment of disease, and others its purely physical effects? (p. 11).

In my meanderings through 19th- and 20th-century research articles and the like, I became even more convinced of the vital importance of specifying *a priori* the criteria for the inclusion and exclusion of individuals in an experimental protocol. The commissioners failed to adopt or enforce this crucial methodological dictum, which convinces me of their biases, lack of objectivity, and the influence of their beliefs and expectancies on the phenomena they reported.

If I sound petulant, it is because I remember how my hopes of impressing the commissioners with manifestations of magnetic phenomena were dashed by their rejection of observation of groups of patients as a means of evaluating the effects of animal magnetism. Why did they make this arbitrary determination? In their report, they stated that

The multiplicity of effects is a first obstacle; one sees too many things at once to see things clearly. Moreover, distinguished patients who come to the treatment for their health could be bothered by the questioning; being so carefully observed could inconvenience or displease them; the Commissioners themselves would be hindered by their concern for discretion. (p. 70)

Yet, I maintain that the decision not to study animal magnetism in a group setting, the very milieu in which I had observed quite spectacular reactions, could only reduce the likelihood of documenting the effects of animal magnetism. And is there even a scintilla of evidence for the commissioners' claim that "distinguished patients" would be upset by questioning and observation? At best, this claim is speculative and based on no hard evidence that the procedures were reactive in any way. Surely, the commissioners could have easily examined the reactivity of questioning and observational procedures by conducting controlled studies of people subjected to a variety of testing conditions. I also conclude that the commissioners harbored negative attitudes concerning the display of behaviors they regarded as "indiscreet." Their stance could have easily communicated an implicit but powerful message to patients to suppress their "indiscreet" crises, as well as other curious and unusual behaviors consistent with the effects of animal magnetism.

Some of the first experiments that the commissioners conducted were "upon themselves." I can only ask, how did the commissioners' concerns about discretion affect their own responses to animal magnetism? Would the commissioners even admit to the influence of animal magnetism if it were present?

Perhaps even more relevant to my defense, how could their admonishment to "not pay too much attention to what was happening inside

themselves" (Franklin et al., 1784/1996, p. 72) not have affected their subjective experiences and powers of observation? These instructions could have established the expectancy that little or nothing of any consequence would occur or dissuaded participants from noting small changes in sensations and perceptions that could potentiate the effects of animal magnetism. Could the impressions of the commissioners and the participants tested have been very different had they been told to pay close attention to (or at least not ignore!) what was happening inside themselves? One can only wonder, insofar as the commissioners did not see fit to manipulate the instructions they conveyed to patients in this phase of the experimental proceedings.

Given the circumstances in which the tests were conducted, it is not surprising that "none of them (the Commissioners) felt a thing, or at least nothing that could be attributed to the action of magnetism" (p. 72). I wrote that "it is obvious these gentlemen made it difficult for themselves to experience sensations in sessions . . . with the amazing precaution not to pay too close attention to what was happening within them" (pp. 12-13).

Despite the commissioners' disavowal of the effects of magnetism, I maintain to this day that there were notable effects that contradict the commissioners' negative assessment of animal magnetism. As specific examples, I cite the report of a commissioner who experienced a "pain in the pit of the stomach" as a result of the great pressure applied to this part, which lasted all day and the next and was accompanied by a feeling of fatigue and uneasiness. A second felt a slight irritation of the nerves, which he is susceptible to, on the afternoon of one of the days he was touched. And a third, "endowed with a greater sensitivity, and especially an extreme instability in the nerves, felt more pain and more intense irritations" (p. 72). To these three reports, I can add the

unreported experiences of Mr. Caille of the Royal Society who at the baquet, without being touched or magnetized by anyone, felt considerable heat, first in the pit of his stomach, then throughout his whole body, followed by nausea, which he could 'avoid' only by abandoning the iron rod of the baquet. (p. 13)

The commissioners dismissed these experiences as " 'slight mishaps' that were the consequence of incessant and ordinary variations in the state of health and, consequently, foreign to magnetism, or they follow from the pressure exerted on the stomach" (p. 72). The commissioners were also quick to dismiss the well-known cure of M. le Baron de ***, who recovered fully from a grave state of illness with magnetism (after traditional treatments failed) with the following question: "But could not a natural occurrence alone have been responsible for this recovery?" First, there is nothing whatsoever unnatural about animal magnetism. Second, I do admit that a possibility of spontaneous recovery exists in such cases. However, the commissioners failed to clearly stipulate in ad-

vance of the experimental proceedings which reports or evidence would count as support for the operation of magnetic effects and which would not. This failure leaves the experiences reported open to a multitude of interpretations, including that they were, in fact, produced by animal magnetism, as I contended.

And I feel compelled to mention that there were numerous such "dismissals" in the Report, not all explained away as the product of spontaneous remission. In the early going of the tests of animal magnetism, after the commissioners decided to study patients on an individual basis, seven patients were magnetized at the home of Franklin. Of these patients, three felt some effects, whereas four felt nothing. One patient felt pain in an eyeball and tears when a thumb was brought close to his face and moved back and forth at close range for a long time. Another patient complained of a headache when a finger was directed toward an "area of prolapse," was short of breath when a finger was placed in front of her face, and, with repeated movements of a finger from high to low, had "quick movements of the head and shoulders such as one has when feeling surprise mixed with fear" (p. 73). And a third patient "... felt similar effects, but to a much lesser degree" (p. 73).

In each case, the commissioners offered an ad hoc, mundane explanation. In the first case, the commissioners wrote:

No doubt Francois Grenet felt pain in his eye and cried because the thumb was brought so close to it. And in the second case, the woman Charpentier complained that when her stomach was touched, the pressure corresponded to the prolapse; and this pressure may have produced a part of the effects this woman felt; but the Commissioners suspected that these effects had been augmented by mental circumstances.

Yet, I must respond with what I wrote so many years ago: "The commissioners might have characterized these allegedly less marked effects more accurately by mentioning that this patient felt all the directions so strongly that he could not hold himself on the chair" (p. 13). The commissioners proposed a "natural" explanation based on the fact that the patients were essentially "ignorant" commoners who reported altered experiences because "he believes it pleases us more when he says he feels effects" (p. 74).

The commissioners contrasted the experience of these "commoners" with the reports of patients chosen from "high society who could not be suspected of ulterior motives and whose intelligence would permit them to discuss their own sensations and report on them" (p. 73). The commissioners conjectured that the patients of a higher class were "... more enlightened, more able to give account of their feelings, felt nothing at all" (p. 73). Do these comments not betray a strong bias to discount the reports of individuals who experienced effects consistent with magnetism merely because of their station in society? The commissioners provided no empirical basis for their ad hoc speculation that intelligence,

motivation, and self-report tendencies were different between commoners and members of high society. Given the commissioners' stated concerns about "discretion" in "distinguished" patients, this "uncommon" group of wig-perfumed patricians would arguably be as likely to suppress behavioral manifestations of magnetism as "commoners" would be to inflate reports to please the commissioners.

And yet two of the members of the upper crust of society did report certain effects that were, like those reported by the commoners, cavalierly dismissed. One patient's report of slight warmth at the place where he usually has pain resulting from a tumor was passed off as arising from "too much attention paid to observing oneself" (p. 73). Another patient who suffered from a nervous condition was "many times on the point of falling asleep while being magnetized" and experienced some agitation and uneasiness. In response to such attempts to interpret the experience of the patients tested, I wrote, "This confident denial of influence is founded on nothing but suspicions and arbitrary assumptions!" (p. 14).

Whereas the commissioners readily discounted reports that could implicate magnetic forces, they were quick to assign credibility to "little Claude Renard . . . this delicate organization of childhood, so fickle and so sensitive!—over which magnetism had no hold. The reason and ingenuity of this child guarantees the truth of his testimony." These statements lead me to wonder whether the commissioners were childless, never told lies themselves as children, or hibernated through the difficult period of their children's adolescence to come to believe that children cannot dissemble. In short, the commissioners made sweeping generalizations about groups or classes of individuals that are entirely lacking in substance and merit based on evidence.

As the experimentation progressed, the commissioners tested subjects in such a way as to reveal the influence of the imagination on their responses. In one study when a woman was blindfolded, she no longer felt sensations in areas where she was subject to magnetic influence. However, when she could see where she was being magnetized, she felt sensations in places corresponding to where the magnetist directed his influence.

The commissioners also tested subjects who were blindfolded and were questioned in a suggestive manner so as to lead them to believe that they were magnetized when they were not, the object of these machinations being to "mislead their imaginations." Based on these experiments, the commissioners were

... convinced by facts that the imagination on its own can produce various sensations and make one feel pain . . . even a substantial amount of heat in all parts of the body, and they have concluded that for many the imagination plays a necessary role in the effects attributed to animal magnetism. (p. 75)

My response to this argument is that a definitive proof against animal magnetism should not derive from a small number of people who can be misled or misdirected under these highly manufactured conditions, when I have treated more than 500 patients with more than a modicum of success.

The same can be said with regard to experiments in which subjects' responses to trees and cups did not seem to depend so much on whether they were magnetized but on what the subjects imagined to be the case. As I stated in the paper I proffered in my defense: "I dare reproach the commissioners with a trifle too much haste when I see them pass an absolute judgment on a few unique cases, which at least merited further experimentation before being dismissed as effects of the imagination" (pp. 14-15).

I also feel compelled to comment on the superficial treatment of a number of the cases. For example, in a case in which a young man experienced a crisis before he arrived at a magnetized tree, I argued that it did not necessarily follow that the magnetized tree had no effect.

A plausible reason why the young man experienced a crisis before arriving at the magnetized tree would have occurred to the Commissioners if they had recalled my theory. . . . I had already taught them that the procedures which activate the agent, once employed on a given subject, are consummated when it pleases Nature, sometimes sooner sometimes later. I explained that whole days often pass in waiting for this moment. If the young man in question experienced a crisis before arriving at the magnetized tree, this can surely be the outcome of a development initiated perhaps in the car in which he came to me, or perhaps in the treatment of preceding days. (p. 14)

In short, I protest that the commissioners did not understand or test my theory. Or, if the distinguished gentlemen did understand it but did not agree with it, it was incumbent on them to clearly in advance of the experiment state their expectations regarding the window of action of magnetism, before rejecting the potentially delayed effects of animal magnetism.

The commissioners were so impressed with the malign powers of animal magnetism to stir the imagination that in their second report they claimed it should be prohibited. They said that faith is the product of the imagination, "faith saves," and "hope is the life of man." However, they went on to say, "but when the imagination produces convulsions, it acts through violent means; these means are almost always destructive" (p. 82). Yet is it not the case that I treated many hundreds of patients who did not seem to be harmed in any way? The commissioners did not study the aftereffects of animal magnetism on a long-term basis, nor did they compare people who were treated with animal magnetism with people who were not treated to support their premature prohibition of a potentially effective therapy. I contend that the convulsions I induced in my patients imbued them with faith that they would recover, with the hope

that they would improve, and with the motivation to vanquish their maladies, even if the cures I effected did not occur by the "gentle means" the commissioners preferred.

I have learned that since my early days of experimentation with animal magnetism, many therapies have been devised to treat sick people and those in distress. Many of these therapies, such as those that involve prolonged exposure to distressing events and aversion techniques in behavior therapy, are not particularly gentle, yet they are often effective, nevertheless. Should gentleness be a primary criterion for implementing or banning a given procedure? I think not. Instead, the effectiveness of a given treatment should be paramount, even if its mechanisms of change are not well understood. Indeed, the mechanisms of many empirically supported treatments, so much in vogue in modern times, are not fully understood, yet this fact does not preclude their use. Even procedures that seem to be as odd, amazing, or magical as animal magnetism, including eye movement desensitization and reprocessing, are worthy of study for many reasons. Not the least of these reasons is that they can further our understanding of potent nonspecific treatment effects (e.g., therapeutic alliance, expectations, motivation) that may account for the effectiveness of a wide range of seemingly diverse treatments.

And so, there you have it. The much-lauded report of the commissioners is, on close inspection, riddled with problems. The commissioners smugly rejected animal magnetism while blithely unaware of their own biases, selective reporting of data, interpretation of cases to suit their premature close-mindedness, and tendencies to make huge inferential leaps about animal magnetism based on anecdotal studies of a small number of subjects with no follow-up of the effects of animal magnetism. Moreover, the commissioners devoted far less attention to specifying whether my treatment worked and under what conditions it was helpful than to debunking my ideas about how the treatment worked.

And yet after I have said all of this, I am going to confess what was once impossible to admit: With the benefit of 2 centuries of hindsight, I can say that I was probably wrong about animal magnetism, that it wasn't an adequate explanation for what I observed. But I still believe that I was on to something that the commissioners all too readily dismissed. Even though I was probably misguided about the mechanisms by which my treatment worked, the effects of the procedures I used were no less amazing, and the cures I effected were no less real. And now that I have said my piece, I rely on you, Steve and Scott, to help me rest in peace.

WE SPEAK

Thank, you, D'Eslon. You can rest assured (as I hope I can rest after I write this) that you did well. You made a case not only for some of the limitations of the commissioners' report, their experimental methods,

and the conclusions at which they arrived, but your words bespeak your decency as a person and your openness as a scientist and a seeker of knowledge.

Your response evoked great sympathy from us, which makes it even easier for us to put together this argument on your behalf based on contemporary psychology. As you acknowledge, even though the commissioners did not make their case iron-clad, the mechanism responsible for the cures you effected was not electromagnetism. Nevertheless, the commissioners' logic was flawed in their imputation that if magnetism doesn't exist, there can be no genuine clinical benefit from your procedures.

In the Age of Enlightenment, the Commission's judgment that the effects of mesmerism were due to imagination was tantamount to concluding that they were not of much psychological interest, thereby trivializing the treatment outcomes you achieved. One of Mesmer's disciples (quoted in Binet & Faria, 1888, p. 17) asked wisely, "If the medicine of the imagination is the most efficient, why should we not make use of it?" We agree, and we'll return to this point later.

We are saddened by the thought that we might have learned so much more by this point in time had the commissioners recognized that the study of animal magnetism could shed light on the way interpersonal communications can be transformed into deeply felt subjective experiences, with vital power to relieve human suffering. Had the commissioners taken your methods seriously, then we might know more today about the determinants of profound alterations in consciousness; the role of imagination in everyday life; the link between responses to suggestion and physiological processes; the role of leading, suggestive questions in shaping experiences; and the way that beliefs and expectancies shape a myriad of subjective and behavioral responses.

Unfortunately, the commissioners regarded your theory as pseudoscientific at best and balderdash at worst and did not have the vision to more carefully examine the efficacy of your procedures. In retrospect, the theory of animal magnetism can better be viewed as protoscientific (e.g., see Moller, 1994) than pseudoscientific. By protoscience, philosophers of science mean an early groping effort to explain a natural phenomenon that presaged more successful and sophisticated efforts. Many protosciences contain a core of truth even as they are mistaken about important scientific details. Rutherford's (1911) model of the atom, which conceptualized the atom much like a miniature solar system with the nucleus at the center and electrons revolving around this nucleus in circular orbits, is a good example of a protoscientific attempt to describe atomic structure. Similarly, the theory of animal magnetism, although clearly incorrect, represented a preliminary attempt to provide a naturalistic explanation for a seemingly supernatural phenomenon.

Of the three causes the commissioners assigned to the purported effects of animal magnetism—touching, imitation, and imagination—imagination was regarded as the primary cause. Touching was thought to prime the effects of imagination. “Imitation communicates and spreads the sensations,” but it was imagination that “produces the great effects one observes with astonishment in the group treatment” (p. 82). The commissioners claimed that they “ . . . demonstrated by decisive experiments that the imagination without magnetism produces convulsions, and that magnetism without imagination produces nothing” (p. 83), and that if one cause—imagination—was sufficient to account for the effects of animal magnetism, then the concept of a magnetic fluid is “useless” (p. 82). However, the commissioners were wrong to equate their legitimate concerns about the uselessness of the concept of animal magnetism with the efficacy of the methods you employed.

We also contend that by invoking the construct of imagination to account for the purported effects of animal magnetism, the commissioners merely substituted one poorly understood concept to explain another. Indeed, as you pointed out in the document you wrote in your defense, the commissioners referred to imagination a great deal, but they did not define it.

The construct of imagination can be defined in a variety of ways including the vividness and lifelike quality of mental images (i.e., pictures in the mind); the ability to create, manipulate, and control mental images; the fluency of mental images; and the ability to translate mental images into physical sensations. The commissioners were well aware of the “action and the reaction of the physical upon the mental and of the mental upon the physical,” (p. 80), indicating that they recognized that imagined events can have physical effects. The commissioners also thought of imagination as the ability to form and retain a mental image (“ . . . she kept seeing before her that same eye intent on watching her; and she constantly carried it in her imagination for three days, whether asleep or awake” p. 78) with concomitant feelings. Yet another way that the commissioners described imagination was akin to modern conceptualizations of expectancies, which can be defined as a person’s mental representations, imaginings, or predictions related to what will occur in a given set of circumstances.

There may be a fine line between expectancies or predictions about future events and mental representations of such events. Of course, it is possible to expect that a future event will occur without vividly imagining its occurrence. And it is possible to vividly imagine an event without adjudging it to have a high probability of occurrence. However, vivid imaginings could in turn strengthen expectancies, and expectancies could strengthen imaginings. Unfortunately, the commissioners made no attempt to explicate the relationship between mental representations and expectations of specific events, much less define imagination in

general. In not providing a clear definition of how imagination was construed, the commissioners did not bring us significantly closer to the genuine underpinnings of what was then termed mesmerism and instead merely provided us with another mysterious concept that is itself in need of explanation (see Skinner, 1977, for a discussion of the dangers associated with invoking cognitive constructs that lack surplus meaning).

What is considerably less ambiguous is that the commissioners failed to appreciate the role of expectancy in the reactions to your methods—the phenomenon from which modern hypnosis evolved. If the commissioners established anything, it was that an important condition for being mesmerized was the susceptible person's belief or expectation that the appropriate stimulus conditions had been met. A clear parallel can be drawn to hypnosis in that virtually any procedure that the subject believes will induce hypnosis can be used to do so (Kirsch & Lynn, 1999).

In addition, the expression of many hypnotic phenomena is closely related to what subjects anticipate will occur during hypnosis. For example, what subjects are told about hypnosis influences whether they experience spontaneous amnesia for the hypnotic session, breach suggested amnesia when hypnosis is "deepened," display spontaneous catalepsy (i.e., their hand can be moved to any position without resistance), show an inability to resist suggested effects, and define their responses as involuntary. These findings are entirely consistent with what the commissioners could have concluded and urged the therapeutic community to exploit long ago: A wide variety of responses can be influenced by suggestive procedures that change individuals' beliefs about what is appropriate, expected, and ultimately possible to achieve in a given circumstance.

But, as you know, D'Eslon, the commissioners undervalued the importance of expectancies and the role of suggestion in treatment in general. Moreover, as you have learned in these recent years, many modern-day psychotherapists underestimate the efficacy of hypnosis as an adjunctive treatment. Like animal magnetism, many clinical practitioners view hypnosis with suspicion due to the seemingly impressive changes in consciousness and the responses that it evokes. The tendency to marginalize hypnosis is unfortunate insofar as a considerable body of evidence supports its efficacy as an adjunct to other treatments, even if, like animal magnetism, the mechanisms that undergird its effectiveness are not completely understood.

A recent special issue of the *International Journal of Clinical and Experimental Hypnosis* on the status of hypnosis as an adjunctive treatment technique evaluated the empirical support for clinical hypnosis in a number of areas based on the following methodological criteria proposed by Chambless and Hollon (1998): (a) The treatment is compared

with and found superior to a no-treatment, alternative treatment, or placebo control group in a randomized control trial, controlled single-case experiment, or equivalent time-series design; and (b) the study utilizes a treatment manual or a logical equivalent, a specific population, reliable and valid outcome measures, and appropriate data analysis. Once these criteria have been met, a treatment may be designated as (a) *efficacious* or *well-established* if it is shown to be superior to a control condition in at least two independent research settings, (b) as *possibly efficacious* if it is shown to be superior to a control condition in one study, and (c) as *efficacious and specific* if it is shown to be superior to a placebo or bona fide alternative treatment in at least two independent research settings.

You can take credit, D'Eslon, for the early treatment of pain-related conditions with hypnotic-like procedures. We hope you feel a sense of pride knowing that a meta-analysis of 18 studies revealed a moderate to large hypnoanalgesic effect, supporting the efficacy of hypnotic techniques for pain control (Montgomery, DuHamel, & Redd, 2000). In fact, hypnosis provided substantial pain relief for two thirds of the participants, who were not all highly suggestible individuals.

Although pain management is perhaps the only domain in which hypnotic procedures can be considered well-established, hypnosis has been shown to be possibly efficacious in: (a) the treatment a variety of medical conditions ranging from irritable bowel syndrome, dermatological disorders, postchemotherapy nausea, and emesis; (b) the preoperative preparation of surgical patients; (c) smoking cessation; and (d) weight reduction. In addition, Kirsch and his colleagues' (Kirsch, Montgomery, & Sapirstein, 1995) meta-analysis of 18 studies revealed that the addition of hypnosis to cognitive-behavioral treatment substantially enhanced the effectiveness of cognitive-behavior therapy (see also Schoenberger, 2000).

Finally, there are preliminary indications that hypnosis is helpful in the treatment of the aftermath of trauma and such childhood problems as enuresis, although more research is needed to firmly establish the role of hypnosis as an adjunctive treatment (Lynn, Kirsch, Barabasz, Cardena, & Patterson, 2000). For example, it is still not clear whether suggestions, independent of a hypnotic induction, are responsible for treatment gains or whether hypnosis is efficacious for pain relief and other conditions above and beyond other nonspecific interventions (e.g., relaxation). At the same time, even if hypnosis is no more effective than relaxation or a placebo condition, for example, it would indicate that nonspecific effects (e.g., therapeutic alliance, expectancies, motivation) are quite potent and psychologically important.

There has been a long tradition in psychotherapy outcome research of treating nonspecific effects as artifacts. From a purely methodological standpoint, in some cases they are. But one person's artifact is another

person's fact. From this perspective, such nonspecific effects as expectancies can be viewed as either artifacts that confound the internal validity of randomized, controlled research designs or as important effects worth harnessing in their own right. The precise manner in which expectancies and other nonspecific effects elicit responses and enhance treatment effects is not yet fully understood, underscoring the point that the effectiveness of an intervention—whether it be hypnosis or animal magnetism—does not depend on a complete understanding of why it is effective. At the same time, identifying viable treatment mechanisms, regardless of their status as specific or nonspecific, can be important in constructing treatments that harness the power of the mechanisms identified and thereby catalyzing treatment gains.

Indeed, D'Eslon, we still have much to learn about hypnosis after more than 200 years of study. Surely, minimal blame should be cast on you for failing to understand the mechanisms of the purported effects of animal magnetism when, today, various theories continue to vie for empirical attention and support. Like the notion of animal magnetism, which features one explanatory mechanism, some contemporary theories of hypnosis (e.g., neodissociation theory, Hilgard, 1986) do so as well. We continue to place our bets on models that take into account the independent contribution and interaction of multiple social and cognitive factors and appreciate the role that individual difference variables, including imaginative ability, may eventually come to play in a comprehensive model of hypnotic responsiveness.

So, D'Eslon, in taking our leave, I urge you not to despair. With the benefit of a bit of scientific and historical hindsight, you have been at least partly vindicated. True, your etiological theorizing left something to be desired, as your assertions concerning animal magnetism have not stood the test of time. But your astute clinical observations opened onto a rich and complex world of important psychological phenomena with which contemporary thinkers are still grappling, such as expectancies, suggestion, imagination, and the many nonspecific variables involved in clinical improvement. In this respect, D'Eslon, you were at least a step ahead of the commissioners, even as you may not have grasped fully the remarkably powerful implications of what you were witnessing.

Now, at last, I can get some rest, reasonably confident that I have appeased D'Eslon. But, just as I am falling asleep, a troubling thought suddenly sets in: "What if I dream of Benjamin Franklin?"

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Eine Kritik des Berichts der Untersuchungskommission: Hypnose, Glaube und Suggestion

Steven Jay Lynn und Scott Lilienfeld

Zusammenfassung: Dieser Artikel ist eine kritische Stellungnahme zu dem richtungsweisenden Bericht mit dem Titel „Bericht der Kommission, die vom König mit der Untersuchung von Tiermagnetismus beauftragt wurde“, auch „Franklin Report“ genannt. Die Verfasser verteidigen D'Eslon, den Schüler Mesmers, der die von den Kommissionsmitgliedern geplanten „Experimente“ durchführte, in denen Tiermagnetismus als unwirksam und ohne Einfluss auf dramatische Verhaltensänderungen und Heilungen nach Mesmers Verfahren entlarvt wurde. Die Autoren zeigen die Schwächen in den Methoden der Kommission auf und diskutieren die in der Natur der Sache liegenden Schwierigkeiten, aus den durchgeführten Experimenten beweiskräftige Schlüsse zu ziehen. Sie behaupten, die Kommission habe dabei eine wichtige Gelegenheit versäumt, die vielfachen Wege zu erhellen, auf denen der Mesmerismus wichtige psychologische Konstrukte und Phänomene zuordnet. Die Verfasser kleiden ihre Kritik phantasievoll in eine wohlwollend prüfende Antwort auf D'Eslons Einwände gegen das Vorgehen der Kommission, die D'Eslon einem der Autoren in einem Traum mitteilt.

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**Critique du rapport de la Commission Franklin:
Hypnose, croyance, suggestion**

Steven Jay Lynn et Scott Lilienfeld

Résumé: Cet article critique l'état des points de repère des commissaires chargés par le Roi d'examiner le magnétisme animal, travail largement connu désormais sous le nom de "rapport Franklin." Les auteurs citent une défense faite par D'Eslon, le disciple de Mesmer qui a entrepris les "expériences," désignée par les commissaires qui ont démystifié le magnétisme animal comme mécanisme responsable des changements excessifs de comportement et des traitements médicaux selon l'application des procédures de Mesmer. Les auteurs identifient des insuffisances dans les méthodes des commissaires, discutent des difficultés inhérentes à tirer des inférences fortes dans les expériences qu'ils ont entreprises, et affirment sur que les commissaires ont manqué une occasion d'élucider les voies diverses que le Mesmerisme a tracé dans les constructions psychologiques importantes et les phénomènes. Les auteurs adoptent une approche imaginaire en formulant leur critique en une réponse sympathique adressée à D'Eslon, qui apparaît à l'un des auteurs dans un rêve et exprime ses réserves au sujet des efforts des commissaires.

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**Una crítica del Informe de la Comisión Franklin:
Hipnosis, creencia, y sugestión**

Steven Jay Lynn y Scott Lilienfeld

Resumen: Este artículo critica al importante Informe de los Comisionados por el Rey para Examinar al Magnetismo Animal, ahora ampliamente conocido como el "Informe Franklin." Los autores montan una defensa de D'Eslon, el discípulo de Mesmer, quien condujo los "experimentos" diseñados por los Comisionados para desacreditar al magnetismo animal como el mecanismo responsable de las alteraciones dramáticas en el comportamiento y las curas médicas causadas por la aplicación de los procedimientos de Mesmer. Los autores identifican deficiencias en los métodos de los Comisionados, analizan las dificultades inherentes a proponer inferencias fuertes en base a los experimentos llevados a cabo, y proponen que los Comisionados perdieron una oportunidad para elucidar las maneras múltiples en que el Mesmerismo manifestaba importantes constructos y fenómenos psicológicos. Los autores adoptan un enfoque extravagante al formular su crítica como una respuesta solidaria a D'Eslon, quien se aparece a uno de los autores en un sueño en que declara sus reservas sobre los esfuerzos de los Comisionados.

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