

Catalepsy Tests: Their Implications for a Science of Hypnotism

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■ *Catalepsy and tests of its presence owe their importance in the study and use of hypnotic phenomena to the proposition that catalepsy is a major sign of hypnosis. It is one among a dozen or so such signs. They all have certain fundamental weaknesses that vitiate their usefulness and that the study of catalepsy brings into relief: Vagueness, ambiguity and lack of precise empirically definable parameters are among major problems. These problems are examined in detail in the case of catalepsy. They are additionally shown to reflect an equally, if not greater problem: the general inadequacy of the nomenclature of hypnotism. Without a relatively precise nomenclature being developed there can be little hope for the existence of a true science of hypnosis; but the greater danger is that terms like „hypnosis“ and „hypnotism“ will become so diffused in meaning as to become meaningless.*

Why talk about catalepsy tests? There are a number of reasons why one might do so. Mine, today, is that it provides one of the better ways to objectify certain general problems that I believe go to the very core of the scientific foundations of hypnotism. But before I get into my main topic, I think it may be a good idea if I clarify how I shall use the terms „hypnosis“, „trance“, and „hypnotism.“ First, although it is possible to make certain distinctions between trances, in general, and hypnosis, in particular (Weitzenhoffer, 1989), here I shall be using these two terms interchangeably. Both these terms will be strictly used in reference to a presumed state or condition of being. On the other hand the term „hypnotism“ will be reserved to designate that branch of knowledge concerned with the production, uses and nature of hypnotic phenomena, hence of hypnosis in particular; in brief, what one might also call the science of hypnosis. Elsewhere (Weitzenhoffer, 1989) I have defined hypnotism as the study and use of suggestion with and without the presence of hypnosis. This remains an alternate definition.

I do not know how many of you use so-called „catalepsy“ tests, and if you do, how often. These tests have been widely used in the past in one form or another. My impres-

sion is that they continue to be quite popular. According to an admittedly limited survey I did about two and a half years ago, 74% of my respondents stated they used one of several such tests: 70 percent said they used an „arm catalepsy test“ and about 32 percent said they also used an „eyelid catalepsy test.“ The popularity of some arm catalepsy tests may additionally be indicated by the fact that one of the few illustrations used in a recent French documentary (Harrois-Monin, 1991) was that of a subject showing an arm catalepsy of the Erickson kind. Out of four illustrations to be found in a recent work by a leading French Ericksonian (Malarewicz, 1990), three depict an Ericksonian arm catalepsy. One recent Canadian author (Kerouac, 1989), a well known Ericksonian therapist, writing on Ericksonian metaphors has shown his enthusiasm for this test by using, not one, but six illustrations, each with a patient showing an Ericksonian arm catalepsy! And, of course, as is well known, it was a favorite test for Erickson.

Insofar as twentieth century hypnotism is concerned, a very common form of the test has been to ask a presumably hypnotized subject to extend an arm and to then explicitly suggest the arm is becoming so rigid as to be unbendable. The subject is then challenged to bend it. This is usually simply called the „arm catalepsy test.“ There has also been a test said of „eye catalepsy.“ Having closed his eyes, the subject is told that his eyelids are stuck, glued, tightly closed or, even more simply, that he cannot open his eyes. Again the subject is usually challenged to do so. Of those who responded to my questionnaire indicating they tested for „catalepsy“, 65% followed this practice. But these two tests are clearly nothing more than tests of suggestibility. They are to be found, in fact, as items in many so-called scales of hypnotic susceptibility, of hypnotizability, and of depth of hypnosis, which are all based on the direct measurement of suggestibility. Why this reference to „catalepsy“ here? Would it not be more appropriate to refer to them respectively as tests of „suggested arm rigidity“ and „suggested inability to open the eyes“ or something else of that order? A variant of the above arm rigidity test has been merely to position the arm as desired and to state or suggest it will remain thus. Erickson frequently did this, using both direct and indirect suggestions, and went on from there to the production of a hypnotic state, that is, when one had not already appeared.

In addition to the above tests, there exists a third one introduced or, as we shall see, real-ly re-introduced, by Erickson around the 1960s. This consists usually of the hypnotist taking hold of the subject's wrist and, without saying a word, moving it and the attached hand and forearm to some arbitrary position, usually slightly forward and at chest level. Having done this, the hypnotist lets go of the wrist, again not saying anything. Greater details of how the test is done can be found in several works (Erickson and Rossi, 1981; Weitzenhoffer, 1989; Malarewicz, 1990). If the test is successful, the hand and arm remain

in position an indefinite amount of time, that is, unless the hypnotist intervenes to change the situation. With some historical justification, Erickson spoke of this test as a „catalepsy“ test. I shall specifically refer to it as the „Erickson“ or „Ericksonian arm catalepsy test“ and, also, as the „postured arm test“, which may be the better designation for it.

Of my respondents indicating they tested for „catalepsy“, only 35% wrote of using this Ericksonian test. I was somewhat surprised by this low figure, however, my sampling was intended to be a random one that made no effort to specially select Ericksonian hypnotherapists. Had I focused on the latter I expect I would have ended with a much higher figure.

I have just described Erickson's test, in a somewhat abbreviated manner, as it is usually seen done. There is, however, more to this test than meets the eye, at least in Erickson's version of it. As the latter explained it to me in the late 1950s, for the test to be valid one must also find present something he called „waxy flexibility.“ The hypnotist can know this is the case only through certain tactile, proprioceptive and kinesthetic sensations that he experiences as he moves the limb (Weitzenhoffer, 1989). Erickson's test then comprises two features: the objective evidence of the arm and hand remaining immobile in a given position, and the subjective evidence of the presence of „waxy flexibility.“ Erickson was none too specific about how long the member would remain in position. I received the impression from him that it would do so as long as the subject remained hypnotized or in trance, or the hypnotist did not do something else to alter the situation. Erickson explained to me that he believed the hypnotic or trance state was uniquely and intrinsically associated with a peculiar, systemic, global neuro-muscular condition of well-balanced tonicity which was demonstrated as present by the test just described. I do not recall him specifically using at the time the term „catalepsy“ for the condition itself, but he appears to have done so at a later date. In any event, the presence of this special neuro-muscular condition was supposed to be indicative of the presence of hypnosis or trance, as Erickson preferred to say.

The origin of modern tests of so-called catalepsy, and the use of the term „catalepsy“ in this context, may possibly go back to Braid. In his seminal book (Braid 1843) he remarks that it was the observed inability of magnetized subjects to open their eyes, once they had closed of their own, that had convinced him of the reality of magnetic somnambulism. In turn, he was able to observe the same effect in his hypnotized subjects. Apparently this did not involve the use of any direct verbal suggestion or challenges. This report by Braid may have given later hypnotists the idea to suggest the effect. A foundation for both arm catalepsy tests that have been described can perhaps also to be found in Braid's book. Having described the subject as having closed his eyes, and having waited 10 or 15 seconds, he goes on to explain that „...by gently elevating the arms and legs, it will be found that the

patient has a disposition to retain them in that position." But, Braid adds, if this does not occur, one should then softly ask the patient to extend his limbs and to maintain them thus "...and the limbs, in process of time, will become quite rigid and involuntarily fixed." He makes no mention of waxy flexibility or of catalepsy proper, but does use further on the term "cataleptiform" in this connection. However, by 1853 he was definitely freely using the word "catalepsy" in this context.

The term "catalepsy", actually its Greek equivalent, is said to have been introduced by Asclepiad of Bithynia in 100 B.C. (Linnaeus, 1877). By around 1773, when Mesmer came unto the scene with his notion of animal magnetism, the term already denoted a widely recognized but poorly understood disorder. Mesmer and later magnetizers apparently never identified or even associated it with the magnetic crisis nor with magnetic somnambulism. Such occurrences of catalepsy, as then conceived and as occurred in the course of magnetizations, were considered to be independently due to a coexisting pathological condition (de Puységur, 1784; Cullerrie, 1886).

I do not know for certain when catalepsy was first associated with hysteria and by whom. According to Richer (1885) this association was already in existence in 1844. This is not to say that all cases of catalepsy were being viewed as being hysterical in nature, but rather it came to be accepted that there existed a hysterical form of catalepsy (Richer 1881, 1885) in addition to one which was of some other nature and to which I shall refer as "catalepsy proper."

Charcot may not have been the first to note a close similarity between some occurrences of hypnosis and naturally occurring catalepsy, hysterical or otherwise, but he was clearly highly instrumental in promoting the idea that a form of hypnosis existed that bore a great deal of resemblance to it. So much so, that it seemed reasonable to him, as well as others, to speak of this form of hypnosis as an "induced" catalepsy." In an often referred to paper Charcot summed up in 1882 the essential features of natural and induced catalepsy as follows: "The cataleptic subject is immobile ... The eyes are open, unmovable, blinking is absent ... The limbs and all parts of the body often keep, for a very long time, the positions, the postures, no matter how difficult to maintain, that have been communicated to them. They appear to be of a great lightness when they are lifted or displaced, and no resistance is felt when they are flexed or extended" (Charcot, 1882).

Here then we find anew a portion of Braid's description of the results of passively moving the subject's limbs. There seems to be disagreement regarding the eyes, but this need not be of concern to us at this time. More to the point is what Charcot next says, "Flexibilitas cerea and that which is called the "stiffness of the painter's mannequin" does not exist. The tendon reflexes are abolished" (ibid.).

Now, as you will recall, Erickson had told me that waxy flexibility was a feature of what he referred to as "arm catalepsy." In order to understand what Erickson meant by waxy flexibility, I asked him to show me some cases of it. What he demonstrated, and which I have recently tried to describe elsewhere (Weitzenhoffer 1989), was essentially what Charcot describes in the first quotation and which, according to him, and contrary to what Erickson said, does not involve waxy flexibility! Since objectively and subjectively the two descriptions agree, the disagreement must have its roots in the definitions of waxy flexibility respectively used by Charcot and Erickson.

I have tried to further clarify this issue of what "waxy flexibility" is, or should be, because references to it come up frequently in the literature, either alone or as a synonym for "catalepsy"; when the signs of hypnosis or trance are discussed. I have not been able to find for certain who first spoke of it and hence how it was first described or defined. However, Wernicke (1900), a recognized authority on the subject and related ones, does say: "Waxy flexibility means that every passive movement meets a moderate constant resistance in all joints involved."

But he then goes on to remark that it is associated with the same kind of postural perseveration that we have been discussing in the case of catalepsy! There is not necessarily a contradiction here as may seem. One should remember that Charcot is speaking of a symptom found present when a specific disorder, called "catalepsy", is also present. Wernicke, on the other hand, is writing about a symptom found in not one, but in a number of other conditions, catatonias being one. We may thus have here two distinct symptoms which, at the objective level, have a common feature that we might call postural perseveration, but which differ significantly at the diagnostician's subjective level. That is, postural perseveration may occur with and without waxy flexibility accompanying it. Failure to recognize this may be what led some writers to speak of catalepsy and waxy flexibility interchangeably.

But there is more. According to Wernicke there exists another condition rather like waxy flexibility but in which he precises "the joint resistance is missing." He proposes that one should refer to it as a "pseudo waxy flexibility." He does not use the term "pseudo" in this connection, but the German expression he uses seems best translated thus. Had Charcot written at a later date and had Erickson been familiar with Wernicke's writings they both might then have said that it is pseudo waxy flexibility that is characteristic of "catalepsy", but clearly in a rather different context.

As if things were not getting complicated enough, Charcot goes on also to point out that, in the case of what he called "induced somnambulism", there is an effect that might also be said to be "cataleptic": When touched in certain ways, the limbs of somnambules, he

says, develop a certain rigidity such that they can be placed in various positions that are maintained. However, in this case, according to Charcot, the person passively moving the limbs of the subject experiences a resistance at the level of the joints which is not experienced in the case of catalepsy, whether natural or induced. Could this be Wernicke's waxy flexibility? Charcot does indeed specifically refer here to „waxy flexibility“ being present. Charcot felt strongly one should distinguish this cataleptic-like effect from that seen with induced and natural catalepsy by speaking here of a „cataleptoid rigidity“ or of a „pseudo-cataleptic“ rigidity. Inasmuch as induced somnambulism was Charcot's hypnotic form most identifiable with twentieth century hypnosis, the idea for the modern test of suggested arm rigidity may also have originated here.

In my experience with the use of Erickson's postured arm test, I must report that, beside having felt with some subjects the lack of resistance, the lightness of the limb, etc, as described by Erickson and by Charcot, I have also often experienced a certain degree of resistance and rigidity when positioning the arm and hand of other subjects. In still other instances, there has been an initial resistance and rigidity which presently disappeared as manipulation of the arm went on. A number of persons have communicated to me having had similar experiences. In brief, both type of responses reported by Charcot have been observed in modern times with presumably hypnotized subjects. I am not sure what this all means, but one thing is apparent: what may appear to be a straightforward test may not be so by a long shot.

Charcot's description of the symptoms of catalepsy, natural and induced, is to be found repeated over and over, often practically word for word, by many of his contemporaries and associates such as Pierre Janet, Binet, Fere, Gilles de la Tourette, Paul Richer, Paul Magnin, Grasset, Barth and many others. But in contrast to Charcot, all begin their description by specifically stating that the most salient feature of catalepsy is the immobility of the patient or subject. True„immobility“ is the first feature Charcot mentions in his description of induced catalepsy. This could indicate that he too sees it as the most salient feature, but this is not the impression I get from his overall discussion. I feel it is the postural perseveration which, of course, by its very nature, must include some immobility, that he sees as the more important. There are also scattered specific references in this older literature to the absence of fatigue as the main symptomatic feature. With regard to the duration of the imposed posture there is little agreement. Binet and Fere (1888) claim a maximum of only 10 to 15 minutes. Gilles de la Tourette (1889) also writes of a duration of only 15 to 25 minutes. On the other hand Pierre Janet (1889) states it varies from 20 minutes to several hours and Barth (1886) writes that the postures can last „indefinitely!“ Beside what Erickson had to say to me about the characteristics of „catalepsy“ in the

context of modern inductions of hypnosis and trance, what do other of his contemporaries and successors have to say here? Returning to my survey, of the 35% of respondent who claimed to use Erickson's test, all stated that when „catalepsy“ was judged present, there was a muscular rigidity and a lack of flexibility of the arm. A number, 53%, saw this rigidity as the cause of a characteristic „immobility.“ Another 6% simply referred to „immobility“ as the main feature. I should add that in later years Erickson (Erickson and Rossi, 1981) seems to have shifted toward viewing immobility as the chief characteristic of „catalepsy.“ For reasons I have given elsewhere (Weitzenhoffer 1989) it is my feeling that this shift may be more a reflection of Rossi's thinking than it was of Erickson's. Both Yapko (Yapko, 1990) and Gilligan (Gilligan, 1987), two Ericksonian authorities, appear also to place their emphasis on immobility as the chief sign of „catalepsy“. References were also made to „waxy flexibility“ by 12 percent of my respondents while another 12 percent referred to a „tonic change.“ For 41% the subject's ability to keep his/her arm „involuntarily“ elevated „without effort“ was also listed as a major feature of „catalepsy.“ I take the expression „without effort“ to possibly be synonymous with „without fatigue“. Clearly, then, there has been a great deal of diversity in the chosen characteristics of catalepsy and no certainty that they actually denote one and the same thing.

While we can find modern and nineteenth century authorities on hypnotism writing about similar behavioral features indicating „catalepsy“, it should be kept in mind that the term „catalepsy“ is used differently by these two groups. This may or may not be significant. In any event, Charcot and his contemporaries used the term mainly to denote a syndrome, a global, systemic state, a general nervous disorder, of which the postural effect was only one symptom. On the other hand, with the exception of Erickson, twentieth century authorities appear to use the term „catalepsy“ mainly to refer to a localized condition and/or as a synonym for what had once been only a symptom of the disorder going by that designation. Thus, it is the arm and not the individual which is often said these days to be in a state of catalepsy. Or again, a detected or suggested rigidity of the arm is often said to be a „catalepsy of the arm.“ Likewise for the perseveration of the position an arm has been given. Of course, this may be seen as being consistent with the fact that, today, the notion of hypnosis being related in any way to catalepsy proper has been totally discarded. It would also have been consistent to altogether drop the use of the term „catalepsy.“ But then so would it have been consistent, long ago, to cease using the term „hypnosis“ and its derivatives in the context of so-called „hypnotic“ phenomena and to qualify them, instead, as being „suggested“, as Bernheim had proposed in 1884.

Actually the situation regarding both natural and induced catalepsy was not as clearcut in Charcot's time as he made it seem to be. In his 1882 presentation he did make some brief

allusions to the existence of incomplete and mixed hypnotic states but left the impression they did not constitute any problem. As a matter of fact, they did, as Richer (1881) had clearly shown earlier in his monumental work on major hysteria. His data had definitely indicated that the symptoms of hysterical and induced catalepsy were not unique to them. Some were also to be found in cases diagnosed as being primarily lethargy and somnambulism. As for catalepsy proper some of its symptoms were and are still said by various authorities to be present not only in hysteria, but also in various psychoses, parkinsonism and encephalitis. This has led many past and even present experts, as can be seen, for instance, from Kaplan and Sadock's (1985) compendium of psychiatry, to view catalepsy sometimes as a syndrome, sometimes as a symptom of other syndromes. The situation was sufficiently bad in this regard by 1877 that, in a lengthy encyclopedia article, Linas felt obliged to state at its start, "... one must ask oneself whether the cataleptic state merits to be admitted in a nosological framework, as a specific illness, or should merely be considered a symptom of one" (Linas, 1877), and further he adds that the concept of catalepsy is, "dominated by an uncertainty, a confusion and even near incoherence that has led to a profuse synonymy and a remarkable variety of definitions" (ibid.). But he finally concludes that catalepsy is both an illness and a symptom of other illnesses and that the extant literature describes at least five major categories of catalepsies, of which one itself possesses 14 further subcategories!

Given this sort of history, plus the fact it became widely agreed, from 1890 on, that Charcot's nosology was a product of unwitting suggestions given to the subjects and of their expectations, why is it we are still talking of "catalepsy" in the context of hypnosis? The fact, of course, is that while the label is questionable, one does see occurrences of many of the features that we have seen were associated with it in the past. But, first of all, do all these features belong under a single inclusive label? Does "immobility", for instance, merely reflect the existence of the peculiar neuro-muscular condition posited by Erickson, or does it independently reflect something else, such as the subject's understanding he/she is not to do anything more than what he/she understands the hypnotist to be requiring, in this case to hold his/her arm and hand as placed by the operator? Or is it possibly a combination of the two? In fact, one should seriously consider the possibility the procedure involved in performing the postured arm test is a source of subtle, indirect, non-verbal proprioceptive and kinesthetic messages that, with a subject of high suggestibility, become suggestions. If so, the postured arm test of Erickson might entirely reduce to a test of suggestibility!

In view of the fact the postured arm test is one of the objective signs of hypnosis most amenable to a detailed study, it is surprising that so little has been done in this regard. To

my knowledge, Richer (1881, 1885) and Binet and Fere (1888) have been the only ones to have attempted to physiologically examine what happens when an arm is positioned with subjects in a "cataleptic" and in a normal state. On the basis of recordings of muscular trembling and of the rate of descent of the arm they were led to conclude, among other things, that the onset of muscular fatigue was appreciably delayed during catalepsy. Since then nothing more has been done along these lines in spite of the fact that we are much better equipped today for this kind of research. For instance, what would Stulen and De Luca's (1982) median frequency index of muscular fatigue show were it to be employed with Erickson's test when using simulating and non-simulating subjects? With electromyographic recordings from appropriate agonist and antagonist muscles it might now also be possible to find evidence for or against the well-balanced tonicity hypothesis Erickson spoke about. Studies, such as that of Waters and Strick (1981), are suggestive of what might be done here. Why has not such work been done in modern times? Surely validating signs of hypnosis ought to have a high priority! Well, maybe not. Bernheim's doctrine of suggestion, which is pretty universally accepted, clearly gives primacy to suggestion in the production of so-called hypnotic phenomena. From this standpoint, the existence of hypnosis may be of more academic than practical interest and the only value Erickson's postured arm test might have would then be as a possible measure of suggestibility. We noted earlier that, indeed, it may be merely just that and, if so, certain remarks made by Bernheim (1886) suggest that, as such, it might indicate very high suggestibility. In this case Erickson's test would hold a special place among tests of suggestibility. True or not, there is a clear imperative need for further research in this area.

I chose to start out by talking about catalepsy. Actually I could just as well have started out by examining any one of the other visible signs that are in use and which I am now listing as a reminder of what they are.

1. Trance stare
2. Psychomotor retardation
3. Waxy flexibility; catalepsy
4. Absence of initiative and spontaneity
5. Automatism
6. Tonic immobility; indisposition toward action
7. Hyposuggestibility
8. Rapport
9. Literality
10. Dissociation

11. Spontaneous posthypnotic amnesia
12. Trance logic
13. Loss of affect
14. General decreased reactivity; anesthesia
15. Pupillary changes (dilation, constriction)
16. Mild overt age-regressive changes

Had we done this we would have come up with the same desultory results. Like „catalepsy“, all these other signs are poorly and ambiguously defined, they lack standardization, and often involve a great deal of subjectivity on the part of the hypnotist in determining their presence. They are far from being unique to hypnosis and can be found associated with a number of other well-known and well-established conditions. Furthermore, there is a strong possibility that they are partly, or wholly, produced as responses to the suggestions and/or instructions that accompany the production of hypnosis and/or its testing. For example, „Just listen to my voice“ and „you will hear only my voice“, two frequent initial instructions and/or suggestions made to the subject, may also be instructions (suggestions) of „don't do anything else“ and/or „do not feel anything else“, resulting in immobility and/or the absence of fatigue. Of course immobility is not necessarily a sign of the absence of fatigue and should not be misconstrued as such, as it has been. And again one should be careful not to confuse immobility and lack of initiative. Also, as one watches a hypnotist testing for „waxy flexibility“ one can readily see how this very testing may introduce all sorts of non-verbal cues and suggestions creating the very sort of thing one is seeking to establish as being present. Finally, these various signs may not all be independent of each other, nor is it clearly established that they are all present when a person is said to be hypnotized.

In the light of what I have now said, it seems to me there is currently great need for researchers to undertake a critical examination of all these signs of hypnosis, not just catalepsy. Let me, however, now add that I believe the issues that have come up are but one aspect of a much broader and more fundamental topic and problem.

What are we doing when we make use of the signs of hypnosis? Of course we do so to ascertain whether or not hypnosis is present. But also, we are defining what it is. We are saying in effect, „hypnosis is that condition which is accompanied by such and such observable effects.“ This is a defining process that has been very effectively used in this fashion by such disciplines as medicine, geology (petrology, to be more specific), and qualitative chemistry. In brief, besides looking at the problem of diagnosing hypnosis, we have also been looking at the nomenclature of hypnotism and one of its problems, the great ambiguity

ty and vagueness that pervades it. It is not just „catalepsy“ that has too many referents, but this can be said of most other key words, such as „hypnosis“, „suggestion“, „trance“, and „hypnotizability“, to mention only these few. Consider, for instance and for a moment, what it is that is actually being said when one states that „catalepsy“ is a sign of „hypnosis.“ Namely, that „catalepsy“, meaning, (1) an absence of „fatigue“, or (2) a „sustained“ posture, or (3) the absence of „resistance“ in the joints, or (4) „immobility“, or (5) a suggested „rigidity“, etc., or various combinations of these, is a sign of the presence of „hypnosis“, meaning, (1) a „trance“, or (2) an „altered state of consciousness“, or (3) an „experience“, or (4) a „model of influential communication“, or (5) „relaxation“, etc. Quite clearly some of the assertions that result from these two groups of meanings make little if any sense at all, some are even contradictory, and others are essentially semantically empty statements. For consider that they lead to saying, for instance, that a „sustained posture“ or a „suggested rigidity“ are signs of a „state of relaxation!“ A clear contradiction. Or again, that „immobility“ denotes a „model of influential communication“, which at best might make an excellent koan. Or again, we are led to saying that „the absence of fatigue is a sign of „an experiencing year. Indeed, consider this recent proposition that strategic therapies are „hypnosis without hypnosis“, (Malarewicz, 1988), or the one that Erickson's postured arm test is an instance of „partial hypnosis“, (Malarewicz, 1990), and finally the one that hypnosis is „a part of every interaction in which a person has influenced another“, (Yapko, 1990)!

Many clinicians, particularly Ericksonians, maintain that hypnotism research has little relevance to their work. If this is true, this gives hypnotism a unique position among sciences, for in the case of other, well-established sciences, research and practice have always gone on hand in hand, interacting in a beneficial manner. On the other hand, all things considered, how could the situation be otherwise for hypnotism when, according to some clinicians, any influential communication is „hypnosis“ and, according to other clinicians, a limp arm and hand, following exhortations to relax, is sufficient evidence that „hypnosis“ is present!

The absence of a proper language and nomenclature for hypnotism was recognised over a 100 years ago when, in 1889, a congress convened in Paris with one of its two stated purposes being to develop such a nomenclature (Berillon, 1889). When the congress ended at the end of four days this topic had yet to be broached and there it has remained. I believe the time has come for this matter to be very seriously reconsidered and acted upon if we want to have a true science of hypnosis. Those disciplines that have successfully attained the status of sciences, characteristically have been those that have developed a relatively precise nomenclature and have adhered to it. Should we expect hypnotism to be an excep-

tion? Let us not fool ourselves into thinking this may be the case.

It would have been well if the 1889 congress had heeded what it had been enjoined to do. Well, it did not, but I do not think it is too late to start now. However, rather than leaving the task to some future congress I would propose that ISH, with the help of its component societies do one of two things: Sponsor one or more conferences to meet in the near future to work on the formulation of a proper nomenclature and ways to enforce adherence to it; or, better, name a committee or a task-force specifically charged with this task. The creation of the DSM might provide a model for this, but other models exist that might be better suited. If we do not take such steps, I am afraid it will truly become a case that, as Bandler and Grinder were fond of saying in the seventies, „hypnosis is everything, hypnosis is nothing!“

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Part II: Clinical issues and applications