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The Fear of Simulation: Scientific authority in late nineteenth-century French disputes over hypnotism.

Abstract

This article interrogates the way/s in which rival schools studying hypnotism in late nineteenth-century France framed what counts as valid evidence for the purposes of science. Concern over the scientific reality of results is particularly situated in the notion of *simulation* (the faking of results); the respective approaches to simulation of the Salpêtrière and Nancy schools are analysed through close reading of key texts: Binet and Féré for the Salpêtrière, and Bernheim for Nancy. The article reveals a striking divergence between their scientific frames, which helps account for the bitterness of the schools' disputes. It then explores Bernheim's construction of scientific authority in more detail, for insights into the messiness entailed by theorizing hypnotism in psychical terms, while also attempting to retain scientific legitimacy. Indicative of this messiness, it is argued, is the way in which Bernheim's (apparently inconsistent) approach draws on multiple epistemic frames.

In 1890 as he surveyed the field of hypnotism research, German physician Albert Moll wrote to refute a “mistaken notion” in relation to the “real difference” between rival French schools of hypnotism,¹ the Salpêtrière school, based at the Paris hospital of that name and headed by the celebrated neurologist Jean-Martin Charcot,² and its much smaller provincial opponent, the Nancy school, grouped around physician Hippolyte Bernheim.³ It would be a mistake, according to Moll, to imagine that “the question of *simulation* forms the point of difference between the two schools”, where *simulation* refers to the deliberate faking of phenomena by hypnotic subjects, imputed (erroneously) here to the Nancy school’s subjects.⁴ Rather, the chief point of contention, as Moll saw it, concerned the role of and the power attributable to hypnotic suggestion.

The “embittered struggle” between the Salpêtrière and Nancy schools merited attention.⁵ It dominated discourses on hypnotism in an historical context remarkable for according clear medico-scientific legitimacy to the topic: France in the last two decades of the nineteenth century. In contrast with so many other times and places in the history of hypnotism, the key question was no longer whether it was an activity fit only for charlatans, but rather the relative merits of the French schools’ conceptions of hypnotism.⁶ While the Salpêtrière school was the early leader in the field, particularly through the work of Jean-Martin Charcot,⁷ the Nancy school came to prominence (and controversy) with the publication in 1884 of Hippolyte Bernheim’s *De la suggestion dans l’état hypnotique et dans l’état de veille* (*On suggestion in the hypnotic state and in the waking state*).⁸ By the release in 1886 of Bernheim’s second book, *De la suggestion et de ses applications à la thérapeutique* (*Suggestive therapeutics*),⁹

the schools' "fierce struggle" concerned more than just the medical fraternity, reverberating in the educated and popular press of the time.¹⁰

"[C]entral to the controversy",¹¹ as Moll insisted, was whether or not suggestion should be understood as "the key to all or almost all so-called hypnotic phenomena".¹² The Nancy school advocated a "keystone" role for suggestion,¹³ a view which, together with an insistence that hypnotic susceptibility was near universal, informed their promotion of hypnotism as a "universal and powerful therapeutic tool".¹⁴ This stands in stark contrast to the Salpêtrière's notions of hypnotism as an experimental neurosis, presenting (almost) only in hysterical patients, and like hysteria, characterized through somatic symptoms organized into distinct states (or stages). Consequently, while Salpêtrière researchers accorded suggestion a role, they tended to emphasize the many hypnotic effects which they saw as possessing a purely physiological basis.¹⁵

Most historians tend to agree with Moll's analysis, confining themselves to the surface level of conflicting ideas to conceptualize the 'battle' as a straightforward matter of the schools' divergent theories, particularly in relation to suggestion and the characteristics of suitable subjects.¹⁶ What these approaches assume, however, is that sufficient common ground existed between Nancy and the Salpêtrière as to what constituted "proof of the reality of [an] experiment" for their differences to be explicable purely on the level of theory.¹⁷ Returning to Moll's formulation, this equates to presupposing that the schools agree on what counts as simulation. But does the 'violence of altercations'

between the schools mask such shared assumptions,¹⁸ or is there some fundamental disjunction in their interpretation of experimental results?

This paper asks how scientific enquiry was conceived and practised at the Salpêtrière and Nancy at the height of the ‘battle’, a period centred on the 1880s. Through close reading of major texts from both schools, I seek to explicate their approaches to ‘objective’ evidence and the sources of scientific authority. A model for this sort of study is Lorraine Daston and Peter Galison’s history of scientific objectivity, which explores changes in what they call ‘ways of seeing’ through the nineteenth and early twentieth centuries.¹⁹ For Daston and Galison, such epistemic codes, the frame for scientific engagement, “were [...] seldom a matter of explicit argument, for they drew the boundaries within which arguments could take place”.²⁰ Without, in any sense, seeking simply to impose their classification rigidly onto the subject-matter of hypnotism, this paper will engage with Daston and Galison’s specific categories as a way of enriching its analysis.

Some of the material covered by this paper appears in Andreas Mayer’s just-published comparative study of cultures of knowledge-production and practice in hypnotism and in early psychoanalysis.²¹ Where Mayer explores attitudes to simulation at the Salpêtrière and Nancy for what they reveal about “experimentalis[ing] the unconscious mind”,²² my own study’s more intellectual-historical approach, following Daston and Galison, interrogates the background of what it meant to consider hypnotic phenomena as objects of scientific study. Furthermore, in question here is the nuance of epistemic values underpinning hypnotism research in the highly specific context of late-nineteenth-century

France, with no stake in mapping issues forward to later practices, like the early Freudian psychoanalytic setting that is the ultimate concern of Mayer's book.²³

By interrogating *scientific* epistemic codes framing hypnotism research, I centre this enquiry on hypnotism insofar as it constituted an 'object of study' for researchers, rather than a therapeutic technique, in which case distinctly medical epistemic values would come into in play.²⁴ Even though Bernheim was known for using hypnotism therapeutically, his 1886 book *De la suggestion et de ses applications à la thérapeutique* exemplifies this separation of the scientific from the medical. Bernheim structures his book in two parts, each making distinct claims and in a distinct style: the first part, "on suggestion", undertakes a scientific investigation of 'what hypnotism is', written to appeal to the educated public; whereas the second part, subtitled "on suggestion applied to therapy", comprises a collection of case studies (with occasional commentary) written in specialized medical language and intended to demonstrate that 'suggestive therapy is effective'.²⁵ This is not to contend that Bernheim's therapeutic engagements did not influence his experimental programme, and accordingly, the analysis will signal those instances where medical frames inflect his understandings of hypnotism as a scientific object.

At stake in my study is how researchers from the Salpêtrière and Nancy schools delimited scientific 'reality'; under what circumstances did phenomena manifested by their object of study, the hypnotic subject, count as 'real' for the purposes of science? The notion of simulation is crucial here, for the presence or absence of simulation fundamentally challenges or confirms the reality, and ultimately, the authority of a claimed result. As a locus for concerns about and

constructions of scientific authority in hypnotism, simulation thus serves as a focus for my analysis. I limit my attention to simulation conceived as located in the hypnotic subject only, as more-or-less deliberate fakery from which the experimenter stands aloof. The related phenomenon of unconscious suggestion, where the experimenter is held to provide subtle 'clues' guiding his subject's actions, falls outside the scope of this paper, particularly as it was construed as distinct from simulation.²⁶

I argue, firstly, that their respective engagements with simulation reveal a fundamental divide between the Salpêtrière and Nancy schools' 'ways of seeing' scientific reality.²⁷ In turn, this disjunction is intimately bound up with the question of whether hypnotism is by nature fundamentally psychical (involving the psyche, mind, or personality)²⁸ or physiological. It is not, however, my purpose here to assign causality between the schools' epistemic approaches and their theoretical perspectives; of greater interest is the messiness of their reciprocal interactions. Contributing to this messiness is that researchers' accounts do not simply provide transparent insight into 'ways of seeing' as pure practice, but also perform a persuasive function. What appear in this paper as differences in 'ways of seeing' between Salpêtrière and Nancy school texts hence necessarily reflect disparate rhetorical exigencies. Thus, the Salpêtrière stressed the value of objective, physical evidence as a defence against simulation, while at the same time focusing on physiological phenomena of hypnotism. Conversely, at Nancy, advocating an essentially psychical hypnotism based on suggestion coincided with less concern over simulation and an absence of structuring methodological principles. Divergence between the schools' interpretative

frames, I contend, provides another interpretative lens for understanding the bitter antagonism of the ‘battle’ which dominated late-nineteenth-century hypnotism research. To be clear, I am not suggesting that the battle was fought on epistemological or even methodological grounds, but rather that discordant *epistemic* frames, in Daston and Galison’s sense of differing ‘ways of seeing’ phenomena, subtended the schools’ otherwise apparently purely theoretical clashes. Secondly, while the Salpêtrière’s focus on physical signs takes the form of a coherent methodological position, the distinct Nancy approach, as exemplified by the work of Bernheim, is less considered, to the point of inconsistency. Since Bernheim’s psychical theorization of hypnotism is generally considered as emerging ‘victorious’ in the schools’ battle,²⁹ it is worth accounting for its conceptual underpinnings in greater detail. Moreover, teasing out the inconsistencies in Bernheim’s views of scientific evidence sheds light on epistemological concerns which arise once human psychical states become the object of scientific enquiry.

Physical Signs in a Bitter Struggle

The Salpêtrière School

Turning first to an examination of the Salpêtrière school’s views, we find that the “fear of simulation” which deterred academic interest in *magnétisme animal* (animal magnetism, or mesmerism) earlier in the nineteenth century³⁰ also informs the school’s positivist³¹ focus on physical/physiological signs as both the

principal symptoms of hypnotism and the necessary proof of an experiment's 'reality'. The main text to be examined here is Alfred Binet and Charles Féré's 1887 *Le magnétisme animal (Animal magnetism)*,³² which was the major extended study discussing hypnotism in its own right, rather than as a corollary to hysteria, published out of the Salpêtrière in the 1880s. Although Charcot as the school's head might seem a more obvious choice to represent the Salpêtrière, his own work on hypnotism was scattered between journal articles and clinical *leçons*.³³ More importantly, Binet and Féré appeared to contemporaries and opponents as key Salpêtrière hypnotism researchers, with Bernheim, in particular, directing much of his criticism against the pair.³⁴

From the outset, Binet and Féré's preoccupation with the shadow of *magnétisme's* past is clear, their opening three chapters tracing how "hypnotism emerged from *magnétisme animal*, as the physico-chemical sciences emerged from the occult science of the Middle Ages".³⁵ Through a story of "portion[s] of truth" (*part de vérité*) obscured by misguided emphasis on mysterious diversions,³⁶ denigration of *magnétisme* on the part of educated (*éclairé*) and medical publics (not necessarily distinct entities) is construed by Binet and Féré as at least partly due to their perception of "wholesale simulation" in hypnotic experiments.³⁷ In other words, these publics tended to view practitioners of hypnotism as deliberate frauds or unwitting dupes of their subjects, leading to widespread scepticism over hypnotism's scientific legitimacy.

What changed public attitudes, according to Binet and Féré, was the application of properly scientific method to hypnotism, as in Charcot's "revolutionary" paper before the Académie des Sciences in 1882.³⁸

Correspondingly, scientific experimentation remains Binet and Féré's overwhelming focus; for them, hypnotism constitutes an important tool in experimental medicine. Hypnotized subjects are "invaluable experimental subjects in mental medicine", for "in effect, the study of hypnotism [...] is chiefly important in enabling the study of physiological processes, and in particular the cerebral functions, *in vivo* in man".³⁹ As for investigation into hypnotic phenomena themselves, the Salpêtrière's approach prioritizes rigorous method over explanatory power:

The Salpêtrière School has as its object, less to give a definitive description, than to show that hypnotism can be studied in accordance with the best developed procedures in experimental physiology and clinical science, and that it is *solely* by following the characteristic steps of these modes of study that *science* can be done.⁴⁰

By referring to best practice in experimental physiology, Binet and Féré situate their method in direct relation to the positivist principles applied to medical research by Claude Bernard, in his eminent work on experimental physiology, *Introduction à l'étude de la médecine expérimentale* (*An introduction to the study of experimental medicine*).⁴¹ Key to the method is identifying objective, physical signs to "serve as proof of the reality of the experiment".⁴² Binet and Féré's understanding is effectively that a 'real' phenomenon displays material symptoms free from the perturbative influence of the subject's (or the experimenter's) personality. The presence of simulation, where a subject consciously influences the result, thus automatically denies an experiment

scientific reality. Fortunately for the legitimacy of hypnotic research, positivist experimental methods mean that “[t]he fear of simulation, which dominated the entire history of *magnétisme animal*, has today become, for the informed and careful experimenter, a completely imaginary danger”.⁴³

In practice, careful experimentation means seeking out physical/physiological signs of each hypnotic state which do not admit the possibility of conscious dupery. Hence, Binet and Féré measure their subjects’ respiration and neuro-muscular responses, like the precise localization of contractures, “because the Salpêtrière school believes that in these phenomena they have found physical signs offering irrefutable proof of the sincerity of the experiments”.⁴⁴ Allied to prioritizing physical observations is the methodological precept of first securing simple phenomena for science, before approaching more complex domains.⁴⁵ For the Salpêtrière researchers, the complex and the psychical are almost synonymous, with “physical facts, [...], however complex they appear, [being] always simpler than the simplest of mental facts”.⁴⁶ There are, moreover, eminently pragmatic grounds for a cautious approach to the psychical. By their nature, subjective psychical phenomena are far more difficult to secure against the taint of simulation; indeed, “one can always simulate a psychical phenomenon”, affirm fellow Salpêtrière researchers Richer and Tourette.⁴⁷ Such considerations inform Binet and Féré’s treatment of suggestion; suggested hallucinations, for instance, are studied in relation to their transformation by optical instruments, such as mirrors, lenses and prisms.⁴⁸ For Binet and Féré, “the goal to achieve is the *objectification* of these subjective disturbances”.⁴⁹ In predicating authority and legitimacy in scientific results on

objective physical signs, the Salpêtrière school thus does not simply make an epistemological choice; this choice frames the methods employed by researchers (graphical recording of respiration traces), their privileged objects of study (neuro-muscular responses and optical transformations in hysterical subjects), as well as the very scope of their research (limited interest in giving a “definitive description”⁵⁰ or of postulating a mechanism for hypnotism⁵¹). It is not simply that the Salpêtrière school has some theoretical objection to hypnotic suggestion, as the straightforward account would have it, but that investigating suggestion is intrinsically devalored by the school’s frame for interpreting scientific reality. Investigation of *any* particular hypnotic phenomena, including the therapeutic applications promoted by Bernheim and others, is framed here as subordinate to considerations of method. That the Salpêtrière’s initial work aimed primarily to demonstrate the “reality and scientific determinism of hypnotic sleep”,⁵² rather than anything particular *about* it, is an appreciation missing from, if not severely misunderstood in, contemporary and critical discussion.⁵³ Such a misapprehension on Bernheim’s part sheds light on his dismissal of his rivals’ work as, at best, of extremely limited value, itself seemingly related to his failure to understand the assumptions behind Salpêtrière researchers’ criticism of his overly ‘complex’ approach.⁵⁴

In a broad sense, the Salpêtrière school is another specific historical example of the consequences of choosing objectivity as a solution to perceived causes of scientific error, as explored by Daston and Galison in regard to scientific atlases. Binet and Féré’s methods would fit into the ‘mechanical objectivity’ epistemic code identified by Daston and Galison, where the

overwhelming concern is to avoid any subjective interference in scientific results.⁵⁵ Examining more carefully, however, we see that the location of the subjectivity that troubles Binet and Féré is transposed from that which concerns the scientific atlas makers discussed by Daston and Galison. In this latter case, objective signs are sought so as to limit any “trace of the knower”, a researcher’s unwitting interpretation of results through the lens of favoured theories, for example.⁵⁶ The object of study is treated here as fixed, unlike in what Daston and Galison term ‘truth-to-nature’, an earlier (for the most part) epistemic code under which researcher intervention was required to stabilize a too-varying nature.⁵⁷ For Binet and Féré, objectivity is *prima facie* a solution (of the ‘mechanical objectivity’ kind), but not (primarily) to the problem of the researcher’s subjectivity; rather as in ‘truth-to-nature’, at issue is instability in the object of scientific study, in the form of potential simulation. The Salpêtrière approach thus blurs the boundaries of Daston and Galison’s categories. This observation doesn’t imply any criticism of those categories, but instead, indicates the singular questions raised by the Salpêtrière school’s attempt to construct a properly scientific hypnotism, and more generally, by any attempt to study scientifically an object (such as the subject of hypnotism) with its own subjectivity.⁵⁸

Bernheim and the Nancy School

Turning next to the Salpêtrière’s opponents from Nancy, a superficial examination would seem to indicate shared assumptions in the schools’ ways of seeing physical evidence, with Bernheim also reporting experiments which probe

suggestive phenomena for their adherence to optical laws.⁵⁹ On the face of it, these experiments closely resemble Binet and Féré's attempts to 'objectify the subjective', with much use of prisms and coloured filters. But to what ends does Bernheim deploy the physical observations he gathers? Rather than predicating reality on objective physical signs, as in the Salpêtrière school's approach, Bernheim paradoxically deploys physical evidence to signal its own inadequacy, privileging more subjective markers of reality as he enunciates the essentially psychological nature of hypnotic phenomena.⁶⁰ The frames for scientific reality at Nancy and the Salpêtrière are thus seen to diverge, with implications for understanding the bitterness of the schools' dispute.

In this paper, I take the revised and augmented second edition of Bernheim's *De la suggestion et de ses applications à la thérapeutique (Suggestive therapeutics)* as a comprehensive statement of the Nancy school's views in the 'battle'. (This edition includes material developed by Bernheim in the mid-1880s, which is absent from the 1886 first edition.⁶¹) The choice to focus on Bernheim is based on his greater role in formulating the Nancy doctrine in opposition to the Salpêtrière, through his numerous scientific works and propagandist review pieces,⁶² as well as the greater importance accorded to him in historical accounts of the battle. It should be noted that Henri-Etienne Beaunis, while sharing Bernheim's insistence on the psychological, distinguished himself from the other Nancy researchers by employing positivist experimental methods and devoting considerable attention to the question of simulation, in a manner similar to Binet and Féré.⁶³ This epistemological diversity appears to have been little remarked, either at the time, or in later historiography.⁶⁴

Unlike Binet and Féré's work, Bernheim's methodological approach is not inspired by the "fear of simulation"; in fact, the term 'methodology' cannot strictly be used in regard to Bernheim, for it implies a certain coherence, which will be seen to be lacking in Bernheim's work. He mentions simulation only occasionally and haphazardly throughout his book, introducing it for the first time as a seeming afterthought to his classification of the various degrees of hypnotism. On this occasion, he admits the possibility of simulation, but insists that the real problem is the prejudiced insistence of some observers on invoking non-existent simulation, understood as enabling *a priori* dismissal of certain hypnotic phenomena:⁶⁵ "It is often difficult to penetrate the psychical state of the subjects influenced; observation is delicate, and analysis is subtle. Doubt exists in certain cases; simulation is possible, even easy; it is even easier to believe in simulation where it doesn't exist."⁶⁶ More striking, for our purposes, is that Bernheim links simulation, not to physical signs, but to the "psychical state" of the hypnotic subject. It is by penetrating or analysing this psychical evidence that Bernheim determines whether a subject is 'influenced', in other words, the absence of simulation and reality of the result.

As he investigates suggested loss of vision, known as *amaurosis*, "wishing to see whether these suggestive amauroses were *real* or not", it thus might seem surprising that Bernheim enlists apparatus designed to unmask simulation by means of optical laws.⁶⁷ For to judge 'reality' by using physical laws to test for simulation implies a conception of the real based on physical signs. This apparent paradox can be explained, however, by interrogating the slippage in what is connoted by 'real', as Bernheim's discussion of the experiment progresses. With

the physical ultimately decoupled from the real, Bernheim's optical experimentation will be seen to function as demonstration, rather than discovery. Applying his psychical theorization of hypnotism, he radically reinterprets physiological evidence in the light of the subject's inferred psychical state. This serves to undermine physiological explanations by opening them up to a strikingly diverging interpretation. His experiments on suggested amaurosis are thus intended precisely to refute Binet and Féré's physiologically based theories, which link it to organic paralysis.

The test device in question is known as Stoeber's apparatus, and is "designed to outsmart (*déjouer*) simulation" of loss of vision.⁶⁸ Subjects with suggested amaurosis in one eye wear a pair of spectacles comprising a green filter on one side and a red filter on the other. They view a set of six white letters on a black background, three letters covered with green glass, the other three with red glass. The underlying physical principle is that light from the letters covered with green glass will be blocked by the red filter, but passed by the green filter. Thus the subject should physically be able to see only three of the letters with each eye. When Ganzinotty, Bernheim's *chef de clinique*, performed the experiment, "he was tempted to believe there was simulation".⁶⁹ Although each subject saw nothing with only her 'blind' eye open, as expected, she saw all six letters with both eyes open. In other words, "they read those [letters] which they were not supposed to see".⁷⁰ An observer interpreting this result from the (intended) standpoint of a reality grounded in the physical, under which apparent unphysicality signals the presence of simulation, would follow Ganzinotty in doubting the reality of the subject's amaurosis. For Bernheim, in

contrast, matters are not so straightforward. Far from suspecting possible simulation, he affirms his subjects' good faith because he views the psychical as implicated in this paradox: he explains that "the amaurosis being purely psychical, that is *imaginary*, [it] couldn't obey optical laws."⁷¹ He maintains that the subjects are not 'blind' in the physiological sense, but that perceptions from the 'blind' eye reach the visual centre only to be "neutralized" by the imagination, if and when subjects *know* they are supposed to 'see' (or rather not to see) with that eye.⁷² The amaurosis "is *real*, but it only exists in the subject's *imagination*".⁷³

Unpacking Bernheim's explanation, we find that in his conception, psychical phenomena are not constrained by physical laws; correspondingly, physical signs have no regular explanatory power in the psychical domain. Where Binet and Féré attempt to objectify this subjective domain, Bernheim rejects it outside the bounds of the regularly observable. The assumptions underlying his usage of 'real' have slipped, from a basis in physical evidence (as implied by the choice of Stoeber's apparatus to judge reality), to excluding the physical in favour of an apparently diametrically opposed domain, the imaginary. Not only is the "real", but also "purely psychical, that is imaginary", amaurosis inaccessible to physical evidence, it even lacks any physiological support, having "no organic substratum, [...] no anatomical localization".⁷⁴

This raises serious implications for the notion of simulation; if the category of the real slips, then so too must conceptions of what it means to fake results. The usual criteria for fraud, manifested in Stoeber's apparatus and amply met here, are disqualified, in Bernheim's account, as a source of meaning. How,

then, does Bernheim determine if a subject is simulating, if experimental results are 'real'? His description of the amaurosis experiment provides few indications, apart from suggesting that subjective categories are in play. I will return to teasing out how Bernheim apprehended reality and its concomitant term, simulation, in a later section; what should be emphasized at this stage is how strikingly Bernheim's understanding differs from the Salpêtrière's interpretative frame, how each school could 'miss the point' with respect to the assumptions informing the other's accounts. For, while Bernheim asserts that his subjects act in good faith, that the phenomena he reports have 'real' worth, the opposite interpretation is left entirely open. All the more so as Bernheim provides no alternative justification for his belief in the trustworthiness of these results. Within the Salpêtrière's epistemic frame, there is clear evidence to discount Bernheim's results as the product of simulation, on the basis of his own physical tests, which they hold to be objective and thus to reflect reality. From this perspective, all Bernheim enunciates is his unsubstantiated opinion: his conviction that suggestion is a psychical phenomenon, and that the unphysicality of his results simply reflects that reality. This fundamental disjunction in the formulation of claims to scientific authority between the Nancy and Salpêtrière schools can help us understand the bitterness of their disputes. With such inherent differences never aired, we can conceive why the 'battle' was so readily framed (and subsequently read) in absolute terms, as "a complete and total antagonism", from which only one school could emerge victorious, its doctrine completely replacing (and effacing) the other's.⁷⁵

Bernheim's Construction of Reality

Subjective Testimony

With Bernheim essentially dismissing physical evidence as an effective marker of reality, what epistemic virtues function for him as sources of scientific authority? How does he judge that his subject has been 'influenced'? As noted above, Bernheim establishes no coherent framework to structure his work; we must therefore probe instances in which the reality of his hypnotic influence is under scrutiny, particularly those involving the concept of simulation. An initial examination reveals two distinct forms of subjective evidence, sited respectively in the hypnotic subject and the researcher.

Consistent with privileging the subject's psychical state in determinations of simulation, Bernheim understands hypnotic influence to be manifested by changes in this state. In particular, a subject is held to be influenced if she experiences amnesia on waking from hypnotic 'sleep':⁷⁶ "there is no longer doubt as to the [hypnotic] influence, as there is amnesia on waking," he states of several degrees in his classification scheme.⁷⁷ Such (if indeed any) psychical information is not directly available to researchers, but must instead be mediated either by the hypnotic subject's individual 'subjectivity' or by the observer's interpretation of the subject's demeanour. The former type of mediation occurs when Bernheim deduces the reality of his influence from the subject's testimony: for instance, the bare affirmation, "I slept! I don't know anything about it."⁷⁸ A subject's testimony can even override Bernheim's own

perceptions, as when Mme X..., given a dramatic suggested hallucination, “behaves absolutely like someone wide awake”, but is finally judged as under hypnotic influence because “she affirms that she sleeps and that she is conscious of sleeping”.⁷⁹

What happens, however, when a subject does not or cannot enunciate his internal impressions? It remains for the observer to account for the subject’s psychological state in terms of his outward behaviour (including various physical signs), opening the possibility that some observers will perceive simulation. An example which occurs ‘daily’ before Bernheim’s students is that of a subject who opens his eyes when the hypnotic operator’s back is turned. In this case, “the assistants believe there is trickery; they pity the operator’s naïve credulity, [believing that] the subject is being deceitful or complaisant”.⁸⁰ Just as when confronted with Ganzinotty’s physical evidence of simulated amaurosis, Bernheim demonstrates an alternative interpretation of these signs, declaring that “the subject is not deceiving [me] and neither am I deceived”.⁸¹ *Bien entendu*, in Bernheim’s text, his version carries greater authority. ‘Interpretation’ is the key term here; the evidence of reality is again mediated, this time by the researcher’s subjectivity, while previously the subjects’ apperceptions were in play. Under the epistemic frames discussed by Daston and Galison, both these cases count as subjective evidence, defined in opposition to ‘mechanical objectivity’, for there is the possibility of “witting and unwitting tampering with the facts”.⁸² What signals the richness of hypnotism as an area in which to explore epistemic categories is that the potential for “tampering” is sited in both the object of study and its observer. The question of just what it means to be

scientific is opened up when both object and experimenter's reliability can be questioned.⁸³

Meriting particular attention is how Bernheim manages dissent between the two forms of subjective evidence. Which player, observer or subject (object of study), should be adduced as more susceptible of "tampering with the facts"? In one case outlined above, the subject's assertion of having slept prevailed over the operator's impressions; however, to infer a general rule from this case is problematic. Notably, to elevate the subject's (subjective) evidence over that of the observer is to open results to the charge of simulation. The classic story of simulation involves, after all, a credulous observer too readily trusting his fraudulent subject. Most dangerous for the experimenter's scientific authority, is when a subject himself claims to have simulated; Bernheim responds to this situation by assessing his subjects' testimony in a flexible, contingent manner. Correspondingly, he asserts that the subject who claims to have simulated is misguided; the subject retains an illusion of free choice, while in fact his "will" and "power of resistance" are weakened, such that he is "forced" to comply with the operator's influence.⁸⁴ "[The subject] sometimes tells himself that he simulates or is complaisant; he boasts in good faith [...] of not having slept but having pretended to sleep. He doesn't always know that *he cannot not simulate*, that *his complaisance [or obliging compliance] is forced*."⁸⁵ This view of simulation has implications, on one hand, for its relevance as a concept once psychical interpretations are valorized. On the other hand, it problematizes Bernheim's evaluation of his subjects' testimony.

Firstly, by conceiving simulation as compulsory in this situation, with subjects “not free not to simulate”,⁸⁶ Bernheim effectively re-casts its definition. The final result is the same, whether a subject is simply influenced or ‘forced’ to simulate. The term loses its discriminatory power: to speculate about (or fear) simulation is no longer productive of insight into the ‘reality’ of phenomena. At stake instead is the presence or absence of an effect, like the introduction of a suggested idea, on the subject’s psychical state; “it matters little whether this idea proceeds from a suggestion by the operator or from the subject’s simulation”, for the two “may, in some cases, be absolutely confused”.⁸⁷ In other words, shifting the focus to the psychical domain seems to require prioritizing *that* an effect occurs over *how* it occurs.⁸⁸

Bernheim’s re-conceptualization of the notion of simulation, tending to collapse its relevance, can explain his comparative lack of interest in the phenomenon. It should not be forgotten, however, that he claims the compulsory nature of simulation in response to, and for the purposes of neutralizing, a specific piece of subject testimony: the allegation of having simulated. Although effective, this move is also precarious. For by discounting one form of utterance, Bernheim risks undermining the very basis of his results: subjects’ accounts of what they see, feel and afterwards remember of hypnotic suggestions. The problematic nature of Bernheim’s tactic is illustrated in a case where he twice challenges a young (hypnotized) man to alter a suggested pose.⁸⁹ Bernheim works from the understanding that a subject who is really influenced would have insufficient willpower to do so. On the first occasion, the man contends he could have moved his limbs, but instead chose to maintain the pose;

in essence, he affirms he simulated out of a desire to please Bernheim. Following his general opinion that such *compliance* is 'forced', Bernheim doubts the authenticity of the man's statement. Ironically, what validates this doubt (and confirms that the man was 'really influenced' the first time round⁹⁰), is the man's assertion that, in the second experiment, "there was not simply *compliance*, there was material impossibility to resist the suggested act".⁹¹ In other words, a second piece of testimony is accepted as valid and reliable precisely for the purpose of dismissing a preceding statement. With both assertions made by the same subject, in essentially the same conditions, and in a very short space of time, there seem no logical grounds for accepting the one, but refuting the other. One way to account for this inconsistency would be to invoke extra-scientific factors.⁹² This argument would paint Bernheim as unscientific for interpreting his results in accordance with his theoretical prejudices, for bending reality to his own ideas about hypnotism. Castel, for instance, takes up this notion when he contends that "Bernheim is guided only by the logic of his position and admits nothing which does not provide clinical support for it".⁹³

While other aspects of Bernheim's *œuvre* can certainly be construed as supporting Castel's position, it is more valuable for my purposes here to go beyond simply painting Bernheim as 'unscientific' and try to apprehend how he could have believed his work had scientific value. Specifically, what source/s of scientific legitimacy did Bernheim mobilize in order to justify his contingent approach to the hypnotic subject's testimony? For as Daston and Galison's analysis of epistemic frames highlights, what is justified in one frame can, and frequently does, appear unscientific in another. Bernheim's 'prejudice' is only

unscientific in a frame, like ‘mechanical objectivity’, which devalorizes the experimenter’s intervention in results. Closer examination of Bernheim’s stance is particularly worthwhile for what it tells us about theorizing hypnotism, and human behaviours more generally, in psychical terms.

Experience and Individual Temperament

Insight into what informs Bernheim’s approach is best found in those exceptional instances where he expresses concern over the reality of observed phenomena. One such instance appears in his work on suggested hallucinations, phenomena which are particularly open to (mis)interpretation as simulation due to their spectacular and striking nature.⁹⁴ Experimenting on one subject, known as Cl..., Bernheim “provoke[s] a truly dramatic scene”, in which Cl... commits murder (on an imaginary victim) and subsequently undergoes (play-acted) judicial interrogation.⁹⁵ Another subject, G..., is induced to take on the roles, in quick succession, of a twenty-year-old singer in a casino, a drunken wagoner, a great lady riding in a carriage, and a corporal in the army.⁹⁶ It is not hard to imagine that such “curious phenomena”⁹⁷ might be attributed by Bernheim’s readers to simulation; only a fine distinction exists, after all, between acting and simulating. Seemingly to counter this threat, Bernheim here treats the matter more seriously; rather than dismissing simulation as illusory or forced, he admits that “one can encounter subjects who simulate knowingly or who, through complaisance, feel themselves obliged to simulate; one can encounter doubtful cases which do not appear convincing”.⁹⁸ Note that this return to formulating

simulation as an occasional risk is a necessary pre-condition for any claim to having addressed the problem. In keeping with his general devalorization of physical evidence, Bernheim's solution is not to be found in physical tests, but instead in the experimenter's experience: "Here, as in all things, *experience* teaches [us] to discern whether the influence obtained is real."⁹⁹ Experience informs an observer's interpretation, permitting him to overrule apparent physical signs, as Bernheim did in the case of suggested amaurosis. This notion similarly permits us to decode Bernheim's apparently inconsistent treatment of successive statements in the case of the young man. It is of no matter to Bernheim to undermine selective portions of his subject's testimony, while relying on the remainder as evidence, because he can deploy his experience as the pre-eminent means of validating such evidence.

Having established experience as Bernheim's key arbiter of reality, we are left to elucidate the epistemic frames upon which he draws in this process. On one hand, the role of experience in legitimating observations can be formulated via the concept of reproducibility:¹⁰⁰ a (new) result is only incorporated into scientific theories if it is common to a (preferably large) series of (ideally independent) experiments. Prior to this verification process, the result must be seen as only provisional. Bernheim expresses a version of this notion as he declares, "[i]t is not lightly, from a single positive or negative observation, that one should make a judgement".¹⁰¹ It is, furthermore, central to his assertion of the value of Nancy's work over that of the Salpêtrière school. As he puts it, his version of the reality of hypnotism is to be preferred because he has obtained concordant results a "very great number of times" on a "very great number of

subjects”, amassing “many hundreds of observations”, showing phenomena which are “constant and uniform in mode” over a variety of subjects of different types.¹⁰² While Bernheim clearly values repeated observations, a notion like reproducibility, with its positivist overtones,¹⁰³ does not account persuasively for how he could conceive as scientific his second use of experience, interpreted now as a form of intuition¹⁰⁴ acquired from long observation of similar phenomena.

As Bernheim describes his wide-ranging experiments on suggested hallucinations, it becomes evident that, in practice, this more subjective notion of experience guides his evaluations.¹⁰⁵ Thus, Sch... eats imaginary strawberries as part of a post-hypnotic suggested hallucination “with an appearance of reality which it would be difficult to imitate”, “imitate” through simulation, that is.¹⁰⁶ The “appearance of reality” returns as a common determinant of an observation’s legitimacy, as a young hysteric darns her stockings, “with a striking appearance of reality”, or as Cl... assumes the suggested role of a parish priest (*curé*) “with a seriousness and an appearance of reality which defy any idea of simulation”.¹⁰⁷ To judge matters on ‘appearance’ is to appeal subjectively to a number of potentially unconscious signs, which combine into an intuition or ‘feel’ for the absence or presence of simulation.

This conception of experience suggests we can position Bernheim’s attitude to scientific evidence in two perhaps overlapping ways. In the first instance, it relates to the epistemic code termed ‘trained judgement’ in Daston and Galison’s study. They construe such a stance as prioritizing interpretation and intuition, ‘celebrating’ judgement, in place of excessive concern over

subjective interference by the observer in capturing reality. Researchers are explicitly expected to learn, from examining large series of observations, to judge on the basis of 'subjective criteria', an approach which is flexible enough to deal with data containing normal variations.¹⁰⁸ (Although I have not developed the idea here, it should be evident that such variation is necessarily present in observations involving subjects' psychical states or 'personalities'.) By explicitly encompassing large numbers of observations, 'trained judgement' provides an alternative understanding of what we earlier related to 'reproducibility'. By prioritizing intuition and experience during the heyday of 'mechanical objectivity', with 'trained judgement' only becoming prevalent in the early twentieth century according to Daston and Galison,¹⁰⁹ Bernheim might be said to 'look forward' to an emerging epistemic code. A second possibility is that in using his experience to judge reality, Bernheim appeals less to a nascent scientific frame than to medical sources of authority, particularly the forms of clinical judgement prevalent in nineteenth-century medicine. The medical gaze described by Foucault in *Naissance de la clinique (Birth of the clinic)*, whereby a clinician's all-encompassing gaze enables him to detect symptoms and diagnose maladies at a glance,¹¹⁰ has clear parallels with Bernheim's recognition of results as simulation-free on the basis of their "appearance of reality". Like Bernheim, the clinician is not led astray by the immediate appearance of physical signs; rather, his trained eye, or medical 'tact', penetrates to the essential reality in play.¹¹¹

Whether we render Bernheim as 'looking forward' to 'trained judgement' or 'looking back' to the 'art' of clinical medicine is arguably only a matter of

perspective; his valorization of experience in reading scientific evidence can be interpreted as a reaction against the limitations of an objective, positivist epistemic code.¹¹² Indeed, Daston and Galison suggest that twentieth-century moves towards ‘trained judgement’ in the reading of medical images (seen as an “empirical art”) overlap, or even “absorb”, a “rear-guard” action by physicians to preserve their clinical status against an encroaching objective medicine.¹¹³ A productive way to envisage Bernheim’s epistemic shift is to follow Daston and Galison in linking it to a displacement of “epistemological worries”.¹¹⁴ Privileging (subjective) experience over (objective) physical signs would thus correspond, for Bernheim, to a diminished “fear of simulation”. To fear simulation, as do Binet and Féré, is automatically to marginalize subjective phenomena (of suggestion, for instance), which are so significant in the Nancy school’s theorization of hypnotism. In contending that the greater problem is an over-readiness to see simulation where it isn’t, Bernheim could be said to react against the ‘paralysis’ entailed by suspecting simulation at every turn.¹¹⁵ (A preoccupation with paralysis is understood by Daston and Galison as underlying the ‘trained judgement’ frame.¹¹⁶)

We can speculate that hypnotism research, at least in its Nancy-school incarnation, is a privileged site for confronting these limitations due to the distinct nature of its object of study, even within medical science. Unlike the medical/anatomical examples explored by Daston and Galison, the object of hypnotism has its own subjectivity. Subjectivity, a potential site of instability, is thus located in both the observer and his scientific object. This distinguishes, I argued above, Binet and Féré’s application of objectivity from that of the atlas

makers in Daston and Galison's work. It similarly works to complicate further any straightforward reading of Bernheim as some kind of 'precursor' to Daston and Galison's 'trained judgement'. In prioritizing subjective experience in his interactions with individual subjects, Bernheim again both 'looks forward' in scientific terms and 'harks back', medically speaking, to pre-positivist medicine and its authoritative foundations in medical tact. Notably, to build up experience with individualized hypnotic subjects requires taking account of precisely this individuality, the subject's involvement as "an ego idiosyncratically determined vis-à-vis the physician".¹¹⁷

This is best seen by following Bernheim's experimentation with suggested hallucinations on Cl... Although he performs suggested murder and the role of a parish priest, with an "appearance of reality" that "defies simulation", Cl...'s hypnotic acting abilities suddenly fail when asked to play a defence barrister. Despite Bernheim explicitly suggesting "words come very easily to you, you are very eloquent", after a single sentence, "[Cl...] stammers, and stops shamefacedly".¹¹⁸ Judging this performance by the experience-based measure of "appearance of reality", it would seem a failure: either Cl... is simulating (unsuccessfully), or the suggestion has no real influence over him. Just as in cases examined previously, Bernheim does not endorse apparently straightforward evidence of simulation, but maintains Cl...'s good faith.¹¹⁹ Alongside his experience of subjects' acting abilities in general, Bernheim takes into consideration an additional, and eminently flexible, source of authority, namely knowledge of the subject's "individuality" (*individualité propre*).¹²⁰ From a series of personal clinical interactions, Bernheim has built up an understanding of Cl...'s

“individual temperament”,¹²¹ a privileged insight into his character. Bernheim thus knows that the role of defence barrister is suggested “in vain”, because Cl... can only bring to the suggested role “the aptitudes [naturally] at his disposal”, and Cl... is “timid”, not “naturally endowed with the gift of eloquence”.¹²²

Cl...’s case problematizes our understanding of Bernheim’s epistemological stance, by demonstrating that generalized experience of a class of scientific objects does not suffice to judge reality, when those ‘objects’ possess their own personality. In requiring experience of specific, individualized objects, as well as of the class as a whole, Bernheim’s approach diverges from that described by Daston and Galison’s ‘trained judgement’. It rejoins instead specifically medical, essentially therapeutic, concerns, in that Bernheim understands individualized insight to play a major role not just in ruling out simulation, but more broadly in ensuring the effective deployment of suggestions, which must “be varied and adapted to the particular suggestibility of the subject”.¹²³ Now, we could interpret Bernheim’s valorization of individualized experience as further evidence that he ‘looks back’ to notions like medical tact in his evaluation of reality, perhaps as a reaction against a de-personalizing objective medicine. But it is also possible to suppose that he mixes scientific and medical domains, viewing his subject as simultaneously object of study and patient. In this case, it is less clear that Bernheim’s move rejects positivist medicine, as this frame left room for the exercise of medical tact for therapeutic ends.¹²⁴ Unfortunately, Bernheim’s work is not sufficiently methodologically self-aware for us to situate his apparent reversion to individualized medicine in relation to Daston and Galison’s framework in any

precise way. What we can say, is that far from being unified and internally consistent, his approach mobilizes multiple frames for scientific evidence, including those derived from more strictly medical/therapeutic considerations, which should be construed as co-existing rather than necessarily cohering. That Bernheim's *œuvre* constitutes such a fertile site for the negotiation of conflicting frames is indicative of the challenges posed by thinking hypnotism in psychical terms. These challenges appear particularly stark in that, as we shall see, Bernheim does not entirely abandon notions of physical evidence as privileged arbiter of reality in legitimating new discoveries.

A Multiplicity of Frames

Although notions of experience and individuality underlie Bernheim's valorization of the psychical, his work on waking-state suggestions reveals that these more explicit modes of scientific authority co-exist uneasily with an implicit reliance on physical evidence as guarantor of reality. This seems to stand in stark contradiction with his earlier radical re-interpretation and invalidation of physical signs; however, as I have argued above, simply labelling Bernheim as blatantly inconsistent is not especially productive. More fruitful is to interrogate the tensions involved in his exceptional deployment of a highly sophisticated, controlled experiment, in the positivist mode.

Whereas we could attribute Bernheim's "fear of simulation" in relation to suggested hallucinations to their spectacular nature, it is rather the novelty of waking-state suggestions which seems to prompt his concern over the reality of

these phenomena. Waking-state suggestions are distinguished from their hypnotic counterparts only by being given to subjects in their 'waking' state (*état de veille*), rather than in the hypnotic (or 'sleep') state.¹²⁵ Bernheim views himself as a pioneer in the area, vehemently stressing that he has "priority" in its discovery, particularly in relation to waking-state suggested anaesthesia and analgesia. He is so insistent that he makes this very "question of priority" a significant portion of the chapter on waking-state suggestions, as well as citing it as one of his key contributions to hypnotism research in an 1887 article.¹²⁶

Claiming (the glory of) a new discovery, however, carries a concomitant risk in potential for doubt over the reality of the phenomena, particularly in the historical context of hypnotism research informed by a "fear of simulation". It is to such a need for definitive proof that we can ascribe Bernheim's preoccupation with, and precautions against, simulation in his account of waking-state effects, as illustrated by the recurrence of terms like "*supercherie*" (trickery), "error" and "*contrôle*" (in the sense of verification).¹²⁷ When it comes to waking-state suggested deafness, however, Bernheim must rely on the subject's (unverifiable) testimony. In a rare departure from his usual practice, he acknowledges that this is problematic. Even though the subject affirms the suggestion was successful, Bernheim introduces a note of doubt: "Verification is no doubt lacking here; I can only cite the subject's assertion."¹²⁸ As we well know, it is not only *here* that verification is lacking, but rather across almost the entire range of observations related by Bernheim. Why is this acknowledgement of the limitations of subjective testimony not transferred into his experimental practice as a whole? How are we to construe this sensibility to disparate epistemological frames?

These questions appear even more pressing when we consider Bernheim's final demonstration of (the reality of) waking-state suggested anaesthesia, a rigorously controlled experiment, which in its sophistication of scientific method surpasses anything that the positivists Binet and Féré report.¹²⁹ In order "to exclude any idea of trickery", Bernheim and his colleague Parisot enlist a specialized piece of physiological apparatus, known as a Dubois-Reymond chariot.¹³⁰ This device exploits electromagnetic induction in two coils to produce an electric shock of variable intensity, which increases as the distance between the coils diminishes.¹³¹ The key measurement is the coil separation distance at which the pain of the shock becomes "unbearable" (*insupportable*) for the subject.¹³² By using the Dubois-Reymond device, Bernheim effectively objectivizes the intensity of the applied stimulus; it becomes repeatable and able to be varied in a regular manner, in contrast to the more usual pin-pricks, which rely on the experimenter's subjective judgement for their intensity. Just when pain becomes "unbearable", however, is a subjective, individualized measure, open to 'tampering' or accusations of simulation. One means of objectivizing this subjective judgement is to perform a control experiment to establish that the subject reports unbearable pain in a consistent manner. Again markedly varying his usual practice, Bernheim undertakes such a control, measuring the inter-coil distance at which his subject G..., in her 'normal' (or pre-suggestion) waking state, claims the pain is unbearable. As the various distances are in agreement and were obtained with G...'s eyes held "hermetically closed", Bernheim establishes that "the pain is really perceived, and is not simulated".¹³³ It is upon the basis of these non-simulated 'normal' measurements that Bernheim bases

his trust in the measurements to follow. He gives G... the waking-state suggestion of anaesthesia in one arm,¹³⁴ before administering a shock of maximum intensity; she manifests no reaction, as would be expected for 'real' anaesthesia. Furthermore, removing the suggestion of anaesthesia causes G... to pull her arm away "sharply" (*vivement*). Together these results demonstrate, in Bernheim's view, that waking-state suggestion is "not exceptional", but rather a real effect, especially as the results remain constant on repetition.¹³⁵

Quite evidently, this meticulous experiment draws upon key notions from positivist science, particularly in the care taken to objectivize otherwise subjective sources of evidence through the use of physical apparatus. It appears signally at odds with the rest of Bernheim's work; however, unpacking the dynamics of this experiment can shed light upon why Bernheim might choose precisely and *only* this phenomenon for positivist-style experimentation. It will consequently enable us to say something about the tensions encountered in attempting to frame hypnotism as simultaneously psychical and scientific. Carefully examining Bernheim's experimental procedure, we find the critical step for establishing that waking-state suggestion is not a product of simulation is the repeated measurement of G...'s 'normal' threshold distance for unbearable pain. It is telling, for instance, that Bernheim only mentions precautions against simulation ("hermetic" closing of G...'s eyes) and repetition of measurements in relation to this control experiment. Determining that G... 'really' perceives pain (itself bound up with the reliability of her perception) serves to 'guarantee' that her post-suggestion behaviour may be trusted. In turn, when G... reacts as predicted (for real anaesthesia), this confirms the reality of waking-state

suggestion in general; the phenomenon's demonstrable 'reality' in one case is effectively generalized to all manifestations of related phenomena. By this logic, only one careful experiment would be required to validate a whole class of phenomena; to doubt the reality of subsequent cases would amount to suspecting a widespread conspiracy between subjects, in league to deceive researchers.¹³⁶ The exceptional nature of this experiment (within Bernheim's general account of waking-state suggestion, at least) can thus be explained.

It is straightforward to rule out alternative explanations, like mistrust of a particular subject, as motivating these elaborate precautions; if G... is above suspicion playing a drunken wagoner (among other roles) in experiments on suggested hallucinations,¹³⁷ she is hardly likely to simulate in the comparatively tame case of suggested anaesthesia. More plausible, but less productive, would be to attribute the impetus for complex physical measurements, not to Bernheim himself, but to the colleagues who tended to assist him in his more technical experiments: Parisot, here, Ganzinotty, previously. Even if Bernheim is just not a very competent experimenter,¹³⁸ that only defers the question: if he is uncomfortable with this sort of approach, why incorporate it into his work?

This leads back to the larger questions of why Bernheim performed only one such positivist, objective experiment, not just in his investigation of waking-state suggestion, but over the course of all his work on hypnotism. Here, too, the notion of guarantee can be beneficial. If we consider the major conceptual difference between waking-state and other suggestive phenomena, it is the absence of hypnotic 'sleep'. Could it be that hypnotic sleep constitutes a privileged state, somehow already proof against simulation? It is not that sleep

itself cannot be simulated (Bernheim admits this possibility), but rather that hypnotic sleep state/s are widely held to be real in general; all hypnotism is no longer viewed as the product of fraud or simulation, as was the case in the early nineteenth century. This is primarily thanks to experiments at the Salpêtrière describing hypnotic states on the basis of objective, physiological signs, as in Charcot's famous paper before the Académie des Sciences.¹³⁹ In the logic of the guarantee, hypnotic sleep thus counts as already validated. In other words, the Salpêtrière school's work can be interpreted as underwriting the reality of hypnotism, leaving Bernheim free to ignore simulation and pursue enquiries in a more subjective mode. For all his explicit, and vehement, rejection of any implication that the 'sleep' state is privileged¹⁴⁰ or that hypnotism involves measurable physiological changes,¹⁴¹ Bernheim appears implicitly to accept these notions in order to benefit from their guarantee of the reality of hypnotic sleep.¹⁴² I am not suggesting that Bernheim therefore subscribes, however implicitly, to the full Salpêtrière description of hypnotism, simply that he accepts the Salpêtrière's physiological proofs of simulation-free hypnotic sleep as sufficiently conclusive in establishing that the state exists at all (i.e. is not always the product of duplicity or connivance).¹⁴³ Beaunis explicitly enlists a similar guarantee in his own work, where (his own) physiological experiments "vouch in advance" (*répondent d'avance*) for the absence of simulation in their psychical/psychological counterparts.¹⁴⁴ The notion certainly accounts neatly for the disparity between the exceptional positivist precautions of the waking-state suggestion experiment and the highly subjective forms of evidence underpinning

those involving *hypnotic* suggestion (notably the suggested hallucinations discussed above).

For Bernheim to deploy Salpêtrière-style experimentation in an area where the Salpêtrière's 'mandate' does not apply has implications for both the schools' battle and the epistemological supports of Bernheim's approach. Firstly, the potential for unacknowledged reliance by Bernheim on the Salpêtrière school's work markedly increases the messiness of the schools' disputes, and underlines the difficulty of accounting for them in straightforward terms. Coinciding with the radical disjunction in the schools' scientific frames appears a certain implicit shared acceptance of physical signs as the ultimate arbiter of reality. On Bernheim's part, this follows not merely from his exploitation of a guarantee, but also from the very structure of his experiment on waking-state suggestion. Disparity between the schools' approaches remains, nonetheless, in precisely the notion of a guarantee, or rather, in its absence from the Salpêtrière's positivist frame, which instead emphasizes controlling the conditions of each and every experiment so as to build up a solid scientific edifice.¹⁴⁵ (Not to mention in the principle of 'simple-to-complex'.) Secondly, for Bernheim to base the reality of a class of phenomena on a single experiment, itself reliant on objectivized physiological measurements, is to draw upon a positivist frame for scientific authority. As well as adding another frame to the set of those informing Bernheim's approach, the notion of the guarantee also seems to establish a hierarchy among the frames, postulating physical evidence as the pre-eminent determinant of reality. Any such ordering of authoritative frames is, however, problematic, given Bernheim's vehement devalorization of

the physical/physiological in hypnotism in favour of the psychical. The tension between these stances suggests we cannot resolve Bernheim's approach into a neat account, but rather that we should construe it as informed by a co-existing set of divergent, if not thoroughly discordant, scientific (and occasionally medical) frames, their uneasy balance shifting with the contingencies of different experiments. More generally, it indicates the difficulties encountered by Bernheim in his attempt to think hypnotism in psychical terms, while also asserting its scientific legitimacy. Ultimately, he fails to find a means of 'scientifically' (or definitively) proving the reality of results, without reverting to precisely that way of seeing, grounded in physical signs, which his psychical approach aims to surpass. Hypnotism research is thus revealed as a key site for tension between epistemic frames, and is a likely site of evolution in modes of seeing scientific authority.¹⁴⁶

Conclusion

The notion of simulation, with its potential for fraud, functions as a locus for unpacking the ways in which researchers into hypnotism framed their understanding of what counts as valid evidence for the purposes of science. Analysing, in turn, key texts from representatives of the Salpêtrière and Nancy schools of hypnotism, we find that Salpêtrière researchers Binet and Féré's preoccupation with simulation is intimately bound up with their positivist methodological approach. Bernheim of the Nancy school, in contrast, dismisses the physical evidence on which Binet and Féré base their judgement of

simulation. He radically re-interprets this evidence through the filter of his psychological theorization of hypnotism, a move justified as scientifically legitimate under an epistemic code which values intuition-like experience, but which appears signally unscientific under a positivist frame. This divergence in the schools' constructions of reality helps account for a key episode in the history of hypnotism in nineteenth-century France, the Nancy-Salpêtrière battle. More than simply a matter of Nancy 'vanquishing' the Salpêtrière because Bernheim's theories better described hypnotism, as the standard historical account would have it, the battle's bitterness and polarization may be ascribed in part to a lack of shared assumptions about what it means to undertake scientific research. That incompatibility also perhaps points to why many historians have tended to 'pick a winner' (Bernheim) in the dispute.

It is difficult, however, to classify Bernheim's approach to scientific authority in any clear-cut manner, particularly given what appears as a striking inconsistency in his evaluation of physical evidence. Rather than attempting such a classification, this paper teases out the various strands of Bernheim's thinking, proposing that his work be understood as informed by multiple scientific, and even medical, frames, co-existing rather than cohering into a unified stance. The tension between valorising subjective testimony, and implicitly relying on physical/physiological tests to guarantee reality speaks to the messiness entailed by trying to think hypnotism, or indeed any scientific endeavour, in psychological terms. Conversely, it indicates the difficulty of dealing with the psychological in a scientific manner; sustained concern over simulation is paralyzing, yet more subjective approaches fail the test of scientific reality, still thought, deep-down,

in physical, not psychical, terms. In the context of the schools' battle, it is indeed ironic that Bernheim's 'victorious' psychical approach is not so much a coherent methodology as a medley of contingent epistemological choices, which, most ironically of all, relies on Salpêtrière-like physical tests of reality at crucial points. From a broader epistemic perspective, the existence of such issues highlights the notable place occupied by hypnotism research in relation to questions of what it is to be scientific, at a time when epistemic change was underway.

¹ Albert Moll, *Der Hypnotismus* (2nd vermehrte und umgearbeitete edn, Berlin, 1890), 227. Translations are my own.

I would like to thank Peter Cryle, Dolly MacKinnon, and Douglas Turk for their helpful comments and suggestions on earlier versions of this piece.

² Charcot (1825–93) is perhaps best known today for his nosological work on hysteria, and as a founder of modern neurology. Biographical details may be found in Christopher G. Goetz, Michel Bonduelle, and Toby Gelfand, *Charcot: Constructing neurology* (New York and Oxford, 1995); A. R. G. Owen, *Hysteria, hypnosis, and healing: The work of J.-M. Charcot* (London, 1971).

³ Bernheim (1840–1919) was Professor of Clinical Medicine at the Nancy Faculty of Medicine from 1879, treating mainly patients with tuberculosis, typhoid, or pneumonia in the attached hospital. See Serge Nicolas, *L'hypnose: Charcot face à*

Bernheim. *L'école de la Salpêtrière face à l'école de Nancy* (Paris, 2004), 23–24;
Jacqueline Carroy, *Hypnose, suggestion et psychologie. L'invention des sujets*
(Paris, 1991), 62–64.

⁴ Moll, *Hypnotismus* (ref. 1), 227. Emphasis added.

⁵ Citation from Henri F. Ellenberger, *The discovery of the unconscious* (New York, 1970), 87.

⁶ For treatment of the fate of hypnotism in the earlier part of the nineteenth century, see Dominique Barrucand, *Histoire de l'hypnose en France* (Paris, 1967); Ellenberger, *Discovery* (ref. 5); Pierre M. F. Janet, *Les médications psychologiques: Etudes historiques, psychologiques et cliniques sur les méthodes de la psychologie* (Paris, 1919). The German and British contexts are explored in, respectively Heather Wolfram, *The stepchildren of science: Psychical research and parapsychology in Germany c. 1870–1939* (Amsterdam and New York, 2009); Alison Winter, *Mesmerized: Powers of mind in Victorian Britain* (Chicago and London, 1998).

⁷ In presenting a paper on hypnotism to the Académie des Sciences in 1882, Charcot was held by many to have 'rehabilitated' hypnotism from its association with the denigrated *magnétisme animal* (mesmerism in English), paving the way for serious medico-scientific study. Alfred Binet and Charles S. Féré, *Le magnétisme animal* (Paris, 1887), 57. Janet, *Médications psychologiques* (ref. 6), 155. Anne Harrington, *Medicine, mind and the double brain: A study in nineteenth-century thought* (Princeton, 1987); Robert G. Hillman, "A scientific study of mystery: The role of the medical and popular press in the Nancy-

Salpêtrière controversy on hypnotism”, *Bulletin of the History of Medicine*, xxxix (1965), 163–83. The last two follow Janet.

⁸ Hippolyte Bernheim, *De la suggestion dans l'état hypnotique et dans l'état de veille* (Paris, 1884).

⁹ Hippolyte Bernheim, *De la suggestion hypnotique et ses applications à la thérapeutique* (Paris, 1886). The English edition appeared under the title *Suggestive therapeutics*; a literal translation would read *On suggestion and its applications to therapeutics*.

¹⁰ Citation from Barrucand, *Histoire* (ref. 6). The role of the press is examined in Hillman, “Scientific study” (ref. 7).

¹¹ Hillman, “Scientific study” (ref. 7), 173.

¹² Hippolyte Bernheim, “Hypnotisme et suggestion: Doctrine de la Salpêtrière et doctrine de Nancy”, *Le Temps (supplément)*, 29 janvier 1891, 1–2, p. 2.

¹³ Hippolyte Bernheim, *De la suggestion et de ses applications à la thérapeutique* (2nd edn corrigée et augmentée, Paris [reprinted 2005], 1888 [1886]), 13.

¹⁴ Ruth Harris, “Murder under hypnosis in the case of Gabrielle Bompard: Psychiatry in the courtroom in Belle Epoque Paris”, in William F. Bynum, Roy Porter, and Michael Shepherd (eds), *The anatomy of madness: Essays in the history of psychiatry* (London and New York, 1985), 197–241, p. 208.

¹⁵ Chief among these were the basic phenomena of the three states (or stages) of hypnotism: lethargy, catalepsy, and somnambulism. Jean-Martin Charcot, “Essai d'une distinction nosographique des divers états compris sous le nom d'hypnotisme”, *Progrès médical* 10e année (1882), 124–6; Binet and Féré, *Magnétisme animal* (ref. 7).

¹⁶ For instance, Barrucand, *Histoire* (ref. 6); Ellenberger, *Discovery* (ref. 5); Harris, “Murder under hypnosis” (ref. 14); Hillman, “Scientific study” (ref. 7). Notable exceptions are the more nuanced (but also unfortunately somewhat haphazard) historical treatment by psychologist and former hypnotism researcher Pierre Janet in *Médications psychologiques* (ref. 6), as well as Carroy’s 1991 study (*Hypnose* (ref. 3)) of the ‘invention’ of hypnotic subjects both culturally and experimentally and Andreas Mayer’s recent exploration of experimental cultures of hypnotism at the Salpêtrière, Nancy, and in Vienna. Mayer, *Sites of the unconscious: Hypnosis and the emergence of the psychoanalytic setting*, trans. Christopher Barber (Chicago, 2013). The schools’ dispute, however, is rarely the focus of historical study. Thus Harris concentrates on broader social implications of speculation about criminal suggestions, while historians like Evans and Showalter discuss hypnotism as a corollary to feminist studies of hysteria. Nicolas’s work, which bears the subtitle *The Salpêtrière school against the Nancy school*, presents the conflict through a collection of historical documents, notably from the (non-aligned) Belgian philosopher and psychologist Joseph Delboeuf. Martha N. Evans, *Fits and starts: A genealogy of hysteria in modern France* (Ithaca and London, 1991); Elaine Showalter, *The female malady: Women, madness, and English culture, 1830–1980* (New York, 1985); Elaine Showalter, “Hysteria, feminism, and gender”, in Sander L. Gilman, et al. (eds), *Hysteria beyond Freud* (Berkeley, Los Angeles and London, 1993), 286–344; Nicolas, *L’hypnose* (ref. 3).

¹⁷ Citation from Binet and Féré, *Magnétisme animal* (ref. 7), 244.

¹⁸ This view is espoused by Harris in relation to the wider social implications of the schools' contested ideas on hypnotism. While Harris's argument is persuasive, I argue that the domain of shared assumptions between the schools cannot necessarily be extended from the social to the scientific. Harris, "Murder under hypnosis" (ref. 14), 203.

¹⁹ Lorraine Daston and Peter Galison, *Objectivity* (New York, 2007).

²⁰ Daston and Galison, *Objectivity* (ref. 19), 369.

²¹ Mayer, *Sites* (ref. 16).

²² Mayer, *Sites* (ref. 16), 5.

²³ Or early experimental psychology, which is Carroy's destination in her epistemological analyses. Carroy, *Hypnose* (ref. 3).

²⁴ In particular, the question of simulation is far less critical if one's major aim is to achieve an improvement in patient condition. More generally speaking, medicine could in no way be termed a wholly scientific field in the late nineteenth century (if ever). It seems that Daston and Galison are nonetheless able to include medical atlases in their study by considering the medical objects represented therein as objects of study (in keeping with their general approach). On atlases as collections of 'objects of inquiry', see Daston and Galison, *Objectivity* (ref. 19), 19, 22. Daston and Galison discuss medical atlases within each of their epistemic frames, see among others, 70–79, 115–20, 321–4.

²⁵ Carroy has Bernheim as using suggestion for both an 'experimentation' and a 'psychotherapy'. Carroy, *Hypnose* (ref. 3), 28. Similarly, in his critical review of Bernheim's 1886 book, Salpêtrière researcher Binet mentions the second section (therapeutic applications) only in one short paragraph at the end of the piece.

Alfred Binet, “Bernheim. – De la suggestion et de ses applications à la thérapeutique. Un vol. in-8, 428 p. avec figures dans le texte. Paris, Doin, 1886”, *Revue philosophique* xxii (1886), 557–63.

²⁶ Apart from Bernheim and Binet and Féré, a key contemporary theorizer of unconscious suggestion was Henri Bergson, who while terming the effect “unconscious simulation”, nonetheless distinguished it from the malicious form of simulation. Henri Bergson, “Simulation inconsciente dans l’état d’hypnotisme”, *Revue philosophique* xxii (1886), 525–31, p. 529.

²⁷ In his historical overview of hypnotism (*Médications psychologiques* (ref. 6)), contemporary observer Pierre Janet raises many points in question here: he remarks on the perceived differences in scientificity between the schools’ ‘doctrines’ (p. 188) and by linking the ‘battle’ to disputes in *magnétisme* between animists and vitalists, goes some way to attributing it to differences in opinion over physical effects (p. 145). Elsewhere, he mentions separately the schools’ interest (or lack thereof) in simulation (pp. 153–5, 157).

²⁸ The relevant French term is *psychique*.

²⁹ A Nancy school ‘victory’ is almost taken for granted by many historians (Harris, Hillman, Nicolas), and is often linked to a view of Bernheim as a precursor of Freud (Barrucand, Ellenberger). Harris, “Murder under hypnosis” (ref. 14); Hillman, “Scientific study” (ref. 7); Nicolas, *L’hypnose* (ref. 3); Barrucand, *Histoire* (ref. 6); Ellenberger, *Discovery* (ref. 5).

³⁰ Binet and Féré, *Magnétisme animal* (ref. 7), 98. Chapters 1–3 of Binet and Féré’s text treat the early history of hypnotism. See also Barrucand, *Histoire* (ref. 6); Ellenberger, *Discovery* (ref. 5); Janet, *Médications psychologiques* (ref. 6).

³¹ Positivism is taken here in its historically specific sense, as a key mode of framing medical science in nineteenth-century France, one strongly associated with physiologist Claude Bernard and his seminal text *Introduction à l'étude de la médecine expérimentale* (Paris, 1865).

³² Binet and Féré, *Magnétisme animal* (ref. 7). Alfred Binet (1857–1911) worked on hypnotism with Charcot's intern and later secretary Charles Féré (1852–1907) under Charcot at the Salpêtrière in the 1880s, before joining the Sorbonne's Laboratory of Physiological Psychology. Today, he is best known for his studies of child intelligence and for developing the what would become the IQ scale. Féré transferred to Paris's Bicêtre Hospital in 1887 to head its psychiatric service and is today known for work on heredity and criminology. Théodore Simon, "Alfred Binet", *L'année psychologique* xviii (1911), 1–14; Frédéric Carbonel, "Le docteur Féré (1852–1907): une vie, une oeuvre, de la médecine aux sciences sociales", *L'information psychiatrique* lxxxii (2006), 59–69.

³³ Some of Charcot's articles were gathered together in 1890 to form part of volume ix of his *Œuvres complètes*. Jean-Martin Charcot, *Hémorragie et ramollissement du cerveau. Métallothérapie et hypnotisme. Electrothérapie* (Paris, 1890).

³⁴ Thus Gauld concentrates his examination of the school's battle in the 1880s on the figures of Binet, Féré, and Bernheim. Alan Gauld, *A History of Hypnotism* (Cambridge, 1992), 329–34. See also Binet, *op. cit.* (ref. 22), 559–60. It is beyond the scope of this paper (partly temporally, but particularly with regard the object of analysis) to explore the later reversal of Binet's views on the importance of physical evidence in psychological investigation, because he developed these as

part of work on experimental psychology, not hypnotism per se. In apprehending unconscious suggestion, not simulation, as the primary danger to experimental determinism, Binet can be said to have adopted one of Bernheim's positions. For an epistemological analysis of Binet's experimental psychology, see Carroy, *Hypnose* (ref. 3), 170–6. As for Féré, he, like many Salpêtrière researchers in the late 1880s or early 1890s, simply drifted towards other research questions.

³⁵ Binet and Féré, *Magnétisme animal* (ref. 7), 46.

³⁶ For example, Binet and Féré, *Magnétisme animal* (ref. 7), 10, 17, 20, 51.

³⁷ Binet and Féré, *Magnétisme animal* (ref. 7), 188.

³⁸ Binet and Féré, *Magnétisme animal* (ref. 7), 60–61.

³⁹ Binet and Féré, *Magnétisme animal* (ref. 7), 217, 276–7. Emphasis added.

⁴⁰ Binet and Féré, *Magnétisme animal* (ref. 7), 119–20. Emphasis added.

⁴¹ Bernard, *Introduction* (ref. 31).

⁴² Binet and Féré, *Magnétisme animal* (ref. 7), 244. See also p. 60.

⁴³ Binet and Féré, *Magnétisme animal* (ref. 7), 98.

⁴⁴ Binet and Féré, *Magnétisme animal* (ref. 7), 98.

⁴⁵ The importance of this precept for claims of scientific legitimacy in hypnotic experiments, especially by comparison with *magnétisme*, is discussed in more detail in an unpublished paper. Kim M. Hajek, "The shadow of *magnétisme*: Marking hypnotism's scientific boundaries in *fin-de-siècle* France" (paper presentation, ANZSHM 11th biennial conference, 28 September-1 October, 2009). It could be argued that an emphasis on simplicity structures Charcot's approach to hypnotism. Physical evidence, not to mention the use of hysterical subjects, would then be preferred for their greater simplicity of

description/interpretation. See Charcot, *Hémorragie* (ref. 33), chapter 9; Charcot, “Essai” (ref. 15).

⁴⁶ Alfred Binet and Charles Féré, “L’hypnotisme chez les hystériques. Le Transfert”, *Revue philosophique* xix (1885), 1–25, p. 4.

⁴⁷ Paul Richer and Georges Gilles de la Tourette, “Hypnotisme”, *Dictionnaire encyclopédique des sciences médicales* (Paris, 1887), 67–132, p. 116.

⁴⁸ Binet and Féré, *Magnétisme animal* (ref. 7), 168–74.

⁴⁹ Binet and Féré, *Magnétisme animal* (ref. 7), 140. Emphasis added.

⁵⁰ Binet and Féré, *Magnétisme animal* (ref. 7), 120.

⁵¹ Binet and Féré qualify the search for a mechanism as “the metaphysics of hypnosis”. Binet and Féré, *Magnétisme animal* (ref. 7), 77.

⁵² Fernand Levillain, “Charcot et l’Ecole de la Salpêtrière”, *Revue encyclopédique* iv (1894), 108–15, p. 111.

⁵³ For example, Joseph Grasset, “Leçons sur le grand et le petit hypnotisme”, *Revue de l’hypnotisme* iii (1888–89), no. 11, 321–35, no. 12, 356–63. Janet is again perhaps the best exception to this. Janet, *Médications psychologiques* (ref. 6), 157.

⁵⁴ See Bernheim, *Suggestion* (ref. 13), 122–8; Hippolyte Bernheim, “Réponse à l’article de M. Binet sur le livre de M. Bernheim: De la suggestion et de ses applications thérapeutiques”, *Revue de l’hypnotisme* i (1886–87), no. 7, 213–18.

⁵⁵ Daston and Galison, *Objectivity* (ref. 19), chapter 3. The Salpêtrière school’s work also coincides temporally with the heyday of mechanical objectivity.

⁵⁶ Daston and Galison, *Objectivity* (ref. 19), 17.

⁵⁷ Daston and Galison, *Objectivity* (ref. 19), chapter 2.

⁵⁸ Carroy examines these questions with regard to the role and characteristics of the hypnotic/psychological subject, while Castel and Wajeman explore related issues around hysteria in nineteenth-century France. Carroy, *Hypnose* (ref. 3); Pierre-Henri Castel, *La querelle de l'hystérie* (Paris, 1998); Gérard Wajeman, *Le maître et l'hystérique* (Paris, 1982).

⁵⁹ Bernheim, *Suggestion* (ref. 13), 64–71, 134–46.

⁶⁰ Mayer makes much the same point in his discussion of simulation and inscription practices at the two schools, arguing that Bernheim “reject[s] the machinery” of Salpêtrière experimentation. Mayer, *Sites* (ref. 16), 61. Gauld identifies Binet and Féré’s efforts towards objectification and Bernheim’s response as important issues in the school’s battle; however, his necessarily brief discussion remains on the more superficial level of a blow-by-blow account. Gauld, *History* (ref. 34), pp. 331–4.

⁶¹ While the second edition claims to be both “revised and augmented”, in practice, revisions tend only to be rhetorical (in the sense of hardening the Nancy position in opposition to the Salpêtrière).

⁶² Bernheim promoted the Nancy cause in Bernheim, “Hypnotisme et suggestion” (ref. 12); Hippolyte Bernheim, “L’Hypnotisme et l’école de Nancy”, *Revue de l’Hypnotisme* ii (1887–88, no. 11), 322–5; Bernheim, “Réponse” (ref. 54).

⁶³ Henri-Etienne Beaunis, *Le somnambulisme provoqué: Etudes physiologiques et psychologiques* (Paris, 1886).

⁶⁴ Carroy and Nicolas, citing Beaunis's unpublished memoirs, point to theoretical divergences between members of the Nancy school. Carroy, *Hypnose* (ref. 3), 61–62; Nicolas, *L'hypnose* (ref. 3), 25.

⁶⁵ See Bernheim, *Suggestion* (ref. 13), 101. Criticism of (perceived) opponents for their self-interested prejudices recurs throughout Bernheim's work.

⁶⁶ Bernheim, *Suggestion* (ref. 13), 18.

⁶⁷ Hippolyte Bernheim, "De l'amaurose hystérique et de l'amaurose suggestive", *Revue de l'Hypnotisme* i (1886–87, no. 3), 68–71, p. 71. Emphasis added. This paper is an earlier and slightly different version of the account in Bernheim, *Suggestion* (ref. 13), 66–68. Here, I work between the two versions.

⁶⁸ Bernheim, *Suggestion* (ref. 13), 67.

⁶⁹ Bernheim, *Suggestion* (ref. 13), 67.

⁷⁰ Bernheim, "Amaurose" (ref. 67), 68.

⁷¹ Bernheim, *Suggestion* (ref. 13), 67. Emphasis added.

⁷² Bernheim, *Suggestion* (ref. 13), 65–66.

⁷³ Bernheim, "Amaurose" (ref. 67), 71. Emphasis added.

⁷⁴ Bernheim, "Amaurose" (ref. 67), 68.

⁷⁵ Citation from Joseph Delboeuf, "De l'influence de l'imitation et de l'éducation dans le somnambulisme provoqué", *Revue philosophique* xxii (1886), 146–71, p. 148. Bernheim triumphantly announces the greatness of the Nancy school (over the Salpêtrière) in Bernheim, "Réponse" (ref. 54). Later accounts include Janet, *Médications psychologiques* (ref. 6); Barrucand, *Histoire* (ref. 6); Harris, "Murder under hypnosis" (ref. 14); Hillman, "Scientific study" (ref. 7).

⁷⁶ The question whether the hypnotic state could strictly be considered a form of (natural) sleep was itself contested; nonetheless, authors routinely employed the terminology of ‘sleep’ and ‘waking’ without necessarily intending it as indicative of reality. The same approach is taken here.

⁷⁷ Bernheim, *Suggestion* (ref. 13), 19.

⁷⁸ Bernheim, *Suggestion* (ref. 13), 89.

⁷⁹ Bernheim, *Suggestion* (ref. 13), 99.

⁸⁰ Bernheim, *Suggestion* (ref. 13), 18.

⁸¹ Bernheim, *Suggestion* (ref. 13), 18.

⁸² Daston and Galison, *Objectivity* (ref. 19), 122.

⁸³ The two forms of mediation interact strikingly around the questions of hypnotic training and unconscious suggestion, as discussed by Delboeuf, “Influence” (ref. 75).

⁸⁴ This is just one way in which hypnotism research troubles the notion of free will; the question was especially discussed in relation to the suggestion of criminal acts. Bernheim is inconsistent regarding subjects’ capacity to resist suggestions, leaving open the possibility, but more often making categorical statements implying the opposite. See, for instance, Bernheim, *Suggestion* (ref. 13), 52–54 (resistance), chapter 9 (criminal suggestions); Beaunis, *Somnambulisme provoqué* (ref. 63), part 2, chapter 5; Binet and Féré, *Magnétisme animal* (ref. 7), chapter 14.

⁸⁵ Bernheim, *Suggestion* (ref. 13), 19. Emphasis in original. Complaisance seems to differ from simulation only in intent: simulation is malicious, where

complaisance arises from a desire to please the operator. At no stage does Bernheim define either term.

⁸⁶ Bernheim, *Suggestion* (ref. 13), 268.

⁸⁷ The citations, in fact, are from Binet and Féré, in relation to ‘objectifying’ subjective suggested phenomena. Binet and Féré, *Magnétisme animal* (ref. 7), 141. This should not be taken, however, imply a broader *rapprochement* between the Salpêtrière and Nancy schools. While both schools might agree on the lessening of simulation’s distinguishability in suggested phenomena, they perceive quite different consequences for an experimental study of suggestion, in line with their diverging views on objective evidence. In the Salpêtrière’s view, even though it may not always be possible to detect simulation, it is no less important to design experiments so as to eliminate it to the greatest extent possible.

⁸⁸ One way to read this emphasis on the ‘effect’ rather than its ‘path’ is to suggest that psychical phenomena are *thermodynamic* in nature.

⁸⁹ Bernheim, *Suggestion* (ref. 13), 266–8.

⁹⁰ There is clearly a logical gap in Bernheim’s assumption that the second experiment necessarily sheds light on the first.

⁹¹ Bernheim, *Suggestion* (ref. 13), 268.

⁹² Sociologists of science Gilbert and Mulkay term this ‘error accounting’. Their analysis, however, assumes a shared conception of what it is to be ‘scientific’, which, I argue, is not the case here. Michael Mulkay and G. Nigel Gilbert, “Accounting for error: How scientists construct their social world when they account for correct and incorrect belief”, *Sociology* xvi (1982), 165–83.

⁹³ Castel, *Querelle* (ref. 58), 59.

⁹⁴ In stark contrast to his generally hagiographic discussion, Barrucand considers Bernheim to be “the dupe of patients simulating suggestion” in these experiments. Barrucand, *Histoire* (ref. 6), 111–12.

⁹⁵ Bernheim, *Suggestion* (ref. 13), 81–82. Citation from p. 81.

⁹⁶ Bernheim, *Suggestion* (ref. 13), 87–89.

⁹⁷ Bernheim, *Suggestion* (ref. 13), 100.

⁹⁸ Bernheim, *Suggestion* (ref. 13), 100.

⁹⁹ Bernheim, *Suggestion* (ref. 13), 100. Emphasis added.

¹⁰⁰ The term itself is more recent; however, we use it here as a useful descriptor of a certain type of scientific behaviour.

¹⁰¹ Bernheim, *Suggestion* (ref. 13), 101.

¹⁰² Bernheim, *Suggestion* (ref. 13), 129, 129, 101, 101.

¹⁰³ For the significant place of repeated observations in Claude Bernard’s work, see, for instance, *Introduction* (ref. 31), 277.

¹⁰⁴ My term, following Daston and Galison’s use of it in relation to ‘trained judgement’. See, for example, Daston and Galison, *Objectivity* (ref. 19), 313.

¹⁰⁵ Indeed, the two interpretations of experience can be rendered as how to judge a series of experiments compared to how to evaluate a single instance. The appeal of physical tests for simulation is that they can apply from the very first experiment, in line with the positivist precept of controlling experimental conditions; it is not necessary to have previously built up a bank of experience.

¹⁰⁶ Bernheim, *Suggestion* (ref. 13), 59.

¹⁰⁷ Bernheim, *Suggestion* (ref. 13), 83, 95–96.

¹⁰⁸ Daston and Galison, *Objectivity* (ref. 19), chapter 6, especially p. 314, 357–9.

¹⁰⁹ Daston and Galison, *Objectivity* (ref. 19), 319.

¹¹⁰ Michel Foucault, *Naissance de la clinique* (Paris, 2009 [1963]).

¹¹¹ Foucault, *Naissance* (ref. 110), especially p. 121–3. The notion of medical tact is a major way in which physicians understood their practice of medicine as an *art*. For Claude Bernard, it is synonymous with “intuition”, a form of “vague and unconscious experience”. (Bernard, *Introduction* (ref. 31), 96, 368–9; citation from p. 368.)

¹¹² Certainly, the pioneer of positivist medicine, Claude Bernard, positioned his objective stance in opposition to the “antiscientific” appeal to medical tact. See Bernard, *Introduction* (ref. 31), 96, 244, 355, 368–9.

¹¹³ Daston and Galison, *Objectivity* (ref. 19), 328–9.

¹¹⁴ Daston and Galison, *Objectivity* (ref. 19), 369–70.

¹¹⁵ This is exactly how Janet, writing in the early twentieth century (as ‘trained judgement’ was growing in importance, in Daston and Galison’s view), appraises Bernheim’s stance. Janet, *Médications psychologiques* (ref. 6), 157.

¹¹⁶ Daston and Galison, *Objectivity* (ref. 19), 370.

¹¹⁷ Castel, *Querelle* (ref. 58), 67–68.

¹¹⁸ Bernheim, *Suggestion* (ref. 13), 84.

¹¹⁹ In fact, he presents this example precisely to demonstrate that the subject’s individuality must be taken into consideration.

¹²⁰ Bernheim, *Suggestion* (ref. 13), 84. By precluding the development of generally applicable laws, an individualized mode of suggestion also clearly

conflicts with the principle of determinism which underpins the positivist scientific frame.

¹²¹ Bernheim, *Suggestion* (ref. 13), 126.

¹²² Bernheim, *Suggestion* (ref. 13), 84.

¹²³ Bernheim, *Suggestion* (ref. 13), 297.

¹²⁴ Claude Bernard described tact as a useful adjunct to science when it came to treating patients. See Bernard, *Introduction* (ref. 31), 360.

¹²⁵ It is (and was) debatable whether such phenomena properly fall under the category of hypnotism research.

¹²⁶ Bernheim, *Suggestion* (ref. 13), 120–1; Bernheim, “Réponse” (ref. 54), 215. Citation from *Suggestion* (ref. 13), 121.

¹²⁷ Bernheim, *Suggestion* (ref. 13), 109–17. For Bernheim’s precautions and tests, see especially p. 115–17.

¹²⁸ Bernheim, *Suggestion* (ref. 13), 116.

¹²⁹ Bernheim, *Suggestion* (ref. 13), 116–17.

¹³⁰ Bernheim, *Suggestion* (ref. 13), 116.

¹³¹ I have relied here on Beaunis’ description of the apparatus (from a different experiment), as Bernheim’s description is brief and unclear. Beaunis, *Somnambulisme provoqué* (ref. 63), 93–94; Bernheim, *Suggestion* (ref. 13), 116.

¹³² Bernheim, *Suggestion* (ref. 13), 116.

¹³³ Bernheim, *Suggestion* (ref. 13), 117.

¹³⁴ Bernheim doesn’t distinguish between anaesthesia and analgesia in describing this experiment.

¹³⁵ Bernheim, *Suggestion* (ref. 13), 117.

¹³⁶ Bernheim invokes the idea of such a conspiracy in order to dismiss it as clearly ridiculous. Bernheim, *Suggestion* (ref. 13), 101.

¹³⁷ Bernheim, *Suggestion* (ref. 13), 88. See above.

¹³⁸ Bernheim implicitly (inadvertently?) upholds this view when he defends his method by insisting that Beaunis (universally seen as a good experimenter) has confirmed and verified his experiments. Bernheim, “Réponse” (ref. 54), 216.

¹³⁹ Charcot, “Essai” (ref. 15).

¹⁴⁰ For example, Bernheim, *Suggestion* (ref. 13), 198.

¹⁴¹ See Bernheim, *Suggestion* (ref. 13), 102–8, 258–61, 134–46.

¹⁴² In enlisting the Salpêtrière’s guarantee, Bernheim would also have implicitly to accept that the phenomena of hypnotism observed in the Salpêtrière’s hysterical (i.e. pathological) subjects bear some relation to those seen in his own non-nervous patients, where he usually denies any parallel.

¹⁴³ It would be interesting to explore further the role of the public gaze in this implicit guarantee, especially as Binet and Féré’s “fear of simulation” appears in response to the concern of the educated public over *magnétisme*. Janet, while generally advocating a more ‘risk-taking’ approach to simulation, claims that “public opinion” made this impossible until Charcot’s “official consecration” removed the fear. Janet, *Médications psychologiques* (ref. 6), 157.

¹⁴⁴ Beaunis, *Somnambulisme provoqué* (ref. 63), 115. Richer and Tourette make a similar argument in “Hypnotisme” (ref. 47), 80.

¹⁴⁵ That some effect is ‘real’ in one case does demonstrate its existence *at all*; however, it can say nothing about, and therefore in no way guarantee, the absence of simulation in any other single experiment. The whole edifice is

susceptible of crumbling if general conclusions are based on these non-verified experiments.

¹⁴⁶ Epistemic clashes in hypnotism research might be said to foreshadow, if not to influence directly, the epistemological issues in play for an emerging experimental psychology at the turn of the twentieth century, as discussed in Jacqueline Carroy and Henning Schmidgen, “Psychologies expérimentales”, *Mil neuf cent. Revue d’histoire intellectuelle* i (2006, no. 24), 171–204; Jacqueline Carroy and Régine Plas, “The origins of French experimental psychology: experiment and experimentalism”, *History of the Human Sciences* ix (1996, no. 1), 73–84.