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The ineffable: A framework for the study of methods through the case of mid-century mind-brain sciences

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Abstract

Conventionally, the story of modern research methods has been told as the gradual ascendancy of practices that scientists designed to extract evidence out of minds and bodies. These methods, which we call 'methods of extraction', have not been the exclusive ways in which experts have generated evidence. In a variety of case studies, scholars in Science and Technology Studies have persuasively documented scientists' efforts to know the extra-linguistic, internal experiences of other beings – prior to or aside from their efforts to represent those experiences in words and images. We propose a new framework to resolve a seeming contradiction in STS, which stems from the fact that the language of 'subjectivity' has been used to refer to two analytically distinct features of scientists' methods: the epistemological premises of a method, on the one hand, and the evaluation of the method in the moral economy of science, on the other hand. Building on Shapin's provocation to study the 'sciences of subjectivity', we analyze three sites in the epistemic niche of 1950s US Federal mind-brain scientists and find that 'methods of extraction' neither replaced nor invariably trumped additional methods that researchers designed to provide evidence of people's interior experiences. We call these additional approaches 'methods of ingression' because researchers purported to generate authoritative evidence by climbing inside the experience of another being, rather than pulling the evidence out. Methods of ingression and methods of extraction coexisted and developed iteratively in dynamic relationship with each other

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– not in isolation nor in competition, as is commonly assumed. Through this empirical study, we provide a new framework that departs from the binary framework of objectivity-subjectivity to allow scholars in STS to more aptly describe scientists' epistemic worlds; to discern a greater range of methods at play; and to appreciate the warrants for knowledge used in our own field.

Keywords
auto-experiment, empathy, epistemic virtue, hacking, human sciences, LSD, NIMH, psychiatry, psychedelic, research methods, subjectivity

Knowing without showing: The problem of 'the ineffable'

In 1966, legendary researchers Walter N Pahnke and William A Richards articulated a long-standing problem in the mind-brain sciences: Most conscious experiences take shape in feelings and perceptions; they do not, in the first instance, take shape in words. The consequence, Pahnke and Richards (1996) explained, was that '[w]hen a subject attempts to communicate mystical consciousness verbally to another person, he usually claims that the available linguistic symbols – if not the structure of language itself – are inadequate to contain or even accurately reflect such experience'. Beyond 'mystical' consciousness, they were interested in how scientists could accurately communicate subjects' interior experiences to the scientific community, given that even subjects felt that their interior experiences were beyond conventional modes of representation, namely words. In their own research, Pahnke and Richards were studying the therapeutic potential of psychotropic drugs, which mid-century scientists used to treat psychosis and addiction, but which also seemed to conjure experiences that defied words even in their healthy human subjects.

> Perhaps the reason such experience is felt to be beyond words is to be found in a frustration with language, which, in turn, arises out of the paradoxical nature of the essential phenomena and the incomparable uniqueness of the experience itself. (Pahnke and Richards, 1966: 181–182)

They referred to the irreducible, extra-linguistic, interiority that defined lived experience as 'the ineffable'.

Pahnke and Richards's concern with the ineffable marked a two-part problem that vexed not only psychotropic researchers, but scientists generally – since William James, they claimed. It started with the question of how scientists could know another person's interior experience when it had not been formed into words, whether in thought, speech, or writing. And following from this first question was the second issue of how scientists could give words to other people's non-verbal experiences. To render these experiences in language would seem to defy the very nature of the experience itself. In this article, we document how Pahnke, Richards, and other twentieth-century clinical researchers used two broad types of methods, which we call 'methods of ingression' and 'methods of extraction', to know the interiority of others and to represent experiences that they regarded as beyond representation.

Conventionally, the story of modern research methods has been told as the gradual ascendancy of practices that scientists designed to extract evidence out of people's minds and bodies. These methods yielded externalized, material evidence that, although disembodied, was presumed to index people's interior experiences, sensory perceptions and emotions. We call these 'methods of extraction'. Scholars in Science and Technology Studies (STS) have documented the particular material, institutional, and interpersonal contexts in which researchers developed methods of extraction.[1] Scientists used these methods to create material inscriptions that documented activity inside people's minds and bodies (Guenther and Hess, 2016). In doing so, they registered their assumption that a subject (human or nonhuman) had an interior experience and simultaneously, as Lemov (2011b) puts it, that this 'inner space' could be 'externalized by the process used to capture it' (p. 263). Researchers developed methods to extract people's interiority whether they regarded their subjects as sick or healthy, exotic or familiar, and reliable or untrustworthy reporters of their own experience. While recognizing the limits of their endeavors, researchers used methods of extraction to create evidence that seemed unmediated by language, explanation, and interpretation, and thus offered as much precision and rigor as possible (see, e.g. Alder, 2007; Daston and Galison, 2010; Porter, 1995; Schaffer, 1992). These methods were no doubt important among mid-century clinical and social scientists. But they were mutually constituted with an additional set of methods that has been set out of view by the conventional narrative of the rise of modern research methods.

Our analysis shows that 'methods of extraction' neither replaced nor invariably trumped additional methods that were designed to provide evidence of another person's interiority. These additional methods were likewise developed as solutions, however imperfect, to the problem of knowing the ineffable. They entailed using techniques that created in the researcher's own body and mind the experience of another type of person (though not the experience of a specific individual). We call these 'methods of ingression', reflecting how researchers used them as formal warrants for their claims to know another consciousness by climbing inside that experience, rather than pulling out evidence of it. Although these methods did not, in the first instance, yield external, material inscriptions – graphs, images, texts – scientists valued these methods for their presumed ability to avoid the distortions that they believed were introduced inevitably in the process of extracting people's interior experience. In the examples we describe, clinical psychiatrists and biochemists, who were part of a 1950s network of researchers based primarily in the United States, used several methods. For example, they used 'self-administration', which involved ingesting psychotomimetics to experience psychosis (in an effort to develop concepts for exploring mental states, to create biochemical theories of mental illness, and to test pharmacotherapies for it). In addition, they underwent psychoanalysis (often grudgingly) at government request and expense to learn 'clinical empathy' and thereby apprehend their patients' interior states in the service of testing therapeutic interventions. Many of these methods asserted a divide between researcher and subject – a division marked by difference that simultaneously indicated faith in the possibility of transcending it.

Scholars can be tempted to relegate methods of ingression to quirks of history. By contrast, we show that scientists used 'methods of extraction' in concert with 'methods of ingression'. Scientists regarded methods of ingression and methods of extraction as separate, yet complementary techniques, each type equally essential to knowledge-

making. We argue that the two types of methods coexisted in the same research spaces and, further, that researchers developed these methods iteratively in dynamic relationship with each other – not in isolation nor in competition, as is commonly assumed. By proposing a framework that delineates these two types of methods, our aim is to provide new analytic tools to understand practices without assuming a direct relationship between 'epistemic virtues' and method choice.

Our framework is broadly relevant for scholars in STS. In individual case studies, scholars have persuasively documented scientists' efforts to know the extra-linguistic, internal experiences of other people – prior to or aside from their efforts to represent those experiences in words and images. For example, mainstream cultural anthropology over the past century came to adopt a very particular version of ethnography based on specific premises that can be traced to the work of influential twentieth-century anthropologists WHR Rivers and Bronislaw Malinowski, who taught that 'if the anthropologist conducted himself as his subjects did, he would become an embodied instrument, literally thinking and feeling as they did,' as Kuklick (2011) explained. The prototypes for early cultural anthropologists as they developed the modern ethnographic method were medical specialists on colonial expeditions who used the method 'of sympathetic observer, who could reproduce in himself the emotions of the person he wished to understand by imitating that person's postures, gestures, and facial expressions' (Kuklick, 2011: 20; see also Steinmetz, 2007; Wilner, 2016). Ultimately, anthropologists' evidence from field research was credible – and stands as credible today – to the extent that they convincingly experienced in their own minds and bodies the interior experiences of the people they sought to know. Likewise, the science of bibliotherapy emerged in the middle of the twentieth century on the grounds of a remarkable claim about the internal experience of readers. Advocates of bibliotherapy promoted specific reading practices among students, clinicians and citizens, on the logic that reading teaches people to enter the embodied worlds – the interiority – of other people, popularizing the now-pervasive notion that reading literature is good for people because it teaches us to 'empathize' (Dufour, 2014). In these examples and many others, scholars in STS have pointed towards researchers' use of methods of ingression to adopt the experience of other people and forms of life – whether in medicine, social sciences, or the humanities.[2] At the same time, there is a grand narrative of the declining authority in the modern period of 'self evidence' (Schaffer, 1992), and its eclipse by seemingly impersonal evidence and methods used to generate it (Porter, 1995). As a result, the many isolated empirical studies have yet to be consolidated, thematized, and understood.[3] We hope to start that project here.

This article gives a sample of methods used in the research spaces of 1950s mind-brain scientists. Today, the analytic framework of objectivity-subjectivity predisposes STS scholars to see methods always already in terms of scientific debates about objectivity and subjectivity, as we explain in the following section. Yet the examples we elaborate suggest that the objectivity-subjectivity framework can at times incline scholars to overlook or dismiss commonly used formal practices and to narrow the questions asked about how scientists make truth claims. In our analysis, we examine texts, images, and oral histories from an 'epistemic niche' of US Federal mind-brain sciences – a field that acutely, though not uniquely, faced the challenge of knowing the interior states of other people. For Federal mind-brain scientists, methods of ingression offered solutions to a

specific epistemic problem: How could a researcher produce evidence of a fundamentally different interior experience without corrupting or contaminating the evidence? We document in the subsequent sections that our actors considered methods of ingression complements, not rivals, to methods of extraction that they also used to produce evidence that was borne inscribed – for example, texts, numbers, graphs or maps that appeared to come into the world unmediated by the researcher. We use this analysis to inductively create the framework of ingression and extraction as an alternative to the objectivity-subjectivity framework (though not as a replacement for it). In our conclusion, we show that methods of ingression, not only methods of extraction, are still used and considered legitimate in fields beyond the niche we explore, including in STS today. This observation invites a reconsideration of the analytic categories of STS and of the methods STS scholars use as knowledge-makers ourselves.

Two meanings of subjectivity: Interiority and inferiority

The distinction between methods of extraction and methods of ingression is useful because current work in STS tends to frame scientists' methods in terms of objectivity and subjectivity – where subjectivity has two analytically distinct features that tend to get conflated. First, subjectivity is conventionally defined as the 'evil twin' of objectivity in the moral economy of modern science. In terms of 'epistemic virtues', practices regarded as subjective came to occupy a relatively inferior position compared to practices regarded as objective (Daston, 1995; Daston and Galison, 2010). Second, subjectivity has been used as a synonym for interiority (e.g. Anderson, 2013). When these two analytically distinct meanings of subjectivity are conflated, it becomes difficult to recognize methods that scientists used to access interiority that they simultaneously regarded as legitimate warrants for truth claims.

In their landmark history of objectivity, Daston and Galison have shown how changes in understandings of 'the human' starting in the eighteenth century reshaped understandings of the scientific self. As human perception came to be thought of as fallible, impressionable and unreliable, scientific communities developed ways to generate empirical evidence that safeguarded themselves (and their claims) against charges of bias and flaw. 'Subjectivity was the enemy within which the extraordinary measures of mechanical objectivity were invented and mobilized to combat', they explain, and mechanical objectivity was gradually supplemented with additional indicators of epistemic virtue in the modern West (Daston and Galison, 2010). Such measures depended on technologically mediated methods that materialized experts' empirical evidence, making it seem intersubjective and thus trustworthy precisely because results appeared untouched by the scientists' own (unreliable) hands, minds, and wills. Building on this work, STS scholars have documented how human scientists tried to manage their own biases, as well as the flaws and fits of their human subjects, by using tools and techniques to signal epistemic virtue (Canales, 2001; Daston, 2007; Morawski, 2015). Yet, Daston and Galison caution, it would be a mistake to think that the rise of externalized, technologically mediated forms of evidence require a narrative of replacement, in which externalized evidence (aimed at bypassing the scientist's interiority or 'subjectivity' in one meaning of the term) is always relatively superior (figured as less 'subjective' in a second meaning of the term).

Thus, there is good reason to avoid investing too heavily in a narrative of the rise of authoritative ways of knowing in which trustworthy observation was equated with external material evidence.[4] The pivot point in this objectivity narrative is the Western Enlightenment, and it turns on a historiographical faith in the overwhelming influence of the philosophy of Immanuel Kant, who insisted that humans could only know the world as it truly was by suppressing their passions, taming their desires, and mistrusting their senses (Daston and Galison, 2010: 205–216; Kant, 2015; Kuehn, 2001). Implicit in current STS scholarship is an assent to the narrative that Kantian epistemology had a singular, reorienting effect on knowledge-making in the modern West. We agree that the influence of Kantian philosophy led many scientific communities to privilege externally verifiable evidence. Yet, this story of the Kantian foundation of modern epistemic virtue tends to be treated as a premise rather than a question, with the result that scholars in STS tend to assume, and therefore find, the mark of Kantian philosophy in modern Western knowledge-making practices.

Yet Kant's metaphysics was challenged even in his lifetime; other equally influential philosophies emerged in critical response to his transcendental philosophy. The possibility of additional philosophical foundations of modern Western science, rather than one exclusive foundation in Kantian philosophy, opens the further possibility of multiple, even contradictory, lineages within the broader story of the rise of authoritative methods of knowledge-making in modern science (Riskin, 2017; Wise, 2018). Specifically, we wonder whether the emphasis on the Kantian foundations of modern scientific methods has foreclosed questions in STS about methods that do not conform to the narrative in which the expert's body is a problem to overcome, rather than a resource for knowledge-making – a perspective that Brain (2015) has criticized as a 'negative epistemology' and challenged in his study of 'physiological aesthetics'.

We take our cue from Shapin's agenda-setting proposal that STS scholars attend to the forms, modes, and practices of the 'modern sciences of subjectivity', by which Shapin means the sciences that use experts' interiority as a resource or that seek to explain other people's interior states. Shapin (2012) observes that expert communities have come 'to free and practical interactional assent about what is, from another point of view, private to the experiencing and knowing subject'. Forms of singular, embodied knowledge have been the central work of large communities of experts who take for granted that interior experience is formally knowable and broadly illustrative, Shapin observes. To be sure, he adds, scholars have studied the experiential dimensions of knowledge-makers and, in the human sciences, the embodied worlds of their subjects, but have done so to elaborate the master narrative of the ascent of externalized evidence (p. 172). We diagnose this state of affairs as the outcome of the conflation of the two meanings of subjectivity: interiority and inferiority. By limiting the role of interiority in practices of knowledge-making to 'whatever it is that makes trouble for objectivity-stories', Shapin argues, we as STS scholars have let ourselves off the hook. 'The idea that there is nothing coherently and stably to be said about the subjective element in knowledge-making, that it is inchoate, arbitrary, unstable, and endlessly varying, fits subjectivity for its supposedly contaminating task', Shapin writes, and 'also excuses us from making its workings an explicitly framed topic of inquiry' (p. 172).

Much of the work developing in this area, whether explicitly in reference to Shapin's manifesto or not, has looked at how personal knowledge is externalized, that is to say, made intersubjective (e.g. Cohen-Cole, 2014). Other work in direct conversation with Shapin's provocation has focused on how experts have standardized and persuasively communicated private, embodied knowledge, giving what could be cast off as individual experience the status of intersubjective, authoritative knowledge (Chong, 2013; Phillips, 2016). These studies complement classic work on the body as research instrument – a literature focused on how bodies and practices are standardized to produce sharable material evidence (Lawrence and Shapin, 1998; Schaffer, 1992).

In sum, the two persuasive accounts offered by Shapin, on the one hand, and Daston and Galison, on the other, present a puzzle. Scholars in STS have demonstrated, in keeping with Daston and Galison, how methods were designed to reduce ('subjective', unvirtuous) mediation by fallible humans – both the knowers and those they have sought to know. Yet STS scholars have also shown how additional ('subjective', interior) forms of evidence are valued precisely because they are personal, sensory experiences, in keeping with Shapin's provocation. We propose a framework that reconciles these two seemingly contradictory claims about scientific authority by avoiding the semantic and therefore analytic conflation of subjectivity as a moral judgement with subjectivity as first-hand experience of interiority. This conflation of the descriptive and evaluative dimensions of scientific practice is the source of the mixed literature on subjectivity, we suggest. Our framework does not associate 'methods of extraction' with more authoritative and 'methods of ingression' with less authoritative forms of knowledge. Nor do we purport to show that experts' methods and their rhetoric clashed. Rather, we inductively arrive at a description of what scientists collectively regarded as legitimate experimental methods, which we capture in the framework of ingression and extraction, and we analyze what their practices allowed them to accomplish.

We highlight methods whose perceived legitimacy and centrality would be easy to overlook if methods for knowing interiority are collapsed into judgements about epistemic virtue – that is, if they are assumed *a priori* to be inferior or interpreted exclusively as the fringe activities of rogue scientists (Giffort, 2015). In the cases we study, most researchers deployed both methods of ingression and extraction in order to overcome the shortcomings of each. As we document, researchers toggled between the two broad types of methods, iteratively responding to the perceived failures of each. In the experimental spaces we explore, scientists developed and deployed both kinds of methods despite, and at times, we argue, precisely *because* the two kinds of methods operated from two different conceptual premises. These two types of methods emerged in dynamic relation to each other. Our framework prompts our main argument: that scientists treated methods of extraction and methods of ingression not as competitors but as necessary complements.

Approach

Our analysis observes the research space of a community of Federal mind-brain researchers in the 1950s as they passed along methods for studying 'the ineffable' to students, newcomers, and colleagues. To do so, we adapt Hacking's (2002) approach of studying an 'ecological niche'. Hacking (2002) proposes the concept of an ecological niche to study

the emergence of diagnostic categories that consolidate feelings and activities that had long existed but only crystalize as targets of medical intervention, expert knowledge, and self-understanding in specific historical moments and locations. Although Hacking (1994) has tended to prioritize language and ideas to explain scientific change, he nonetheless recognizes that language is inseparable from the spaces in which it is used – and the geographical, material, political, and temporal specificity of those spaces. The metaphor of the ecological niche registers the fact that scientific concepts and practices only emerge and persist if there is good fit between the concept and multiple elements in a working space, including physical arrangements, social expectations, organizational infrastructures and patterns of interaction. Hacking's metaphor also emphasizes that particular practices may be loosely contained in a specific space but that some conditions (or 'vectors') that enable those practices, can extend beyond that space. In contrast to an approach like Actor-Network Theory, which takes similar factors into account yet posits an agonistic field, Hacking's ecological approach removes the assumption that a space is best understood in terms of conflict and, by extension, that some practices will come to dominate and others die (Pettit et al., 2015). Thus, while the specific methods we examine may have prompted struggles in other times and places, there is no need to assume they were inherently contestable. We sample the methods researchers used within a niche and find that methods of ingression and of extraction not only co-existed but were mutually sustaining: Researchers used and developed them in dynamic relation to each other.

Case: 1950s Federal mind-brain sciences

We examine the niche constituted by three laboratories of the US National Institute of Mental Health (NIMH), which organized the activities of approximately two hundred clinical researchers in the 1950s. At all three facilities, researchers were Federal employees, going to work each day in government clinics and laboratories that were part of the NIMH 'intramural' research program. They were not 'extramural' grant recipients working at any number of universities, research institutions, or private clinics, but a group of government scientists that was sizable but bounded, both spatially and organizationally.[5]

The US Congress formally established NIMH in 1949 during the waning days of the Truman administration with a mandate to study the perceived public health crisis of schizophrenia (Grob, 1991). The prevalence and incidence of schizophrenia, as well as a number of other disorders, had spiked in the USA and across the globe in the years after World War II (American Psychiatric Association, 1952; Pilgrim, 2014) – though historians have shown that the uptick in schizophrenia diagnoses was an artifact of looser diagnostic standards, shifting clinical practice, professional rivalries, and structural discrimination against women and minorities, who were disproportionately diagnosed with mental illnesses in the 1950s (Heinrichs, 2001; Metzl, 2009; Warren, 1987). As one indication of the perceived seriousness and extent of mental illness, the World Health Organization and UNESCO declared the year 1959 'World Mental Health Year' (UNESCO, 1959).

The researchers in this niche shuttled between three main NIMH facilities: St. Elizabeth's Hospital in Washington, DC; the Addiction Research Center (ARC) in the US PHS Narcotic Hospital in Lexington, Kentucky; and the NIH Clinical Center in

Bethesda, Maryland, where NIMH had dedicated wards in the research hospital it shared with the other institutes that constituted the (plural) National Institutes of Health. These three NIMH facilities were unlike other mental health institutions in that they were Congressionally mandated to do research, not treatment exclusively. At St. Elizabeth's Hospital, researchers studied patients admitted for major mental illness. At ARC, researchers studied drug effects in incarcerated 'postaddicts' (Campbell, 2007). At the NIH Clinical Center, located on the agency's main campus, researchers relied on a national network of colleagues to supply sick patients for experiments and recruited healthy civilians through organizations with which they had contracted for 'normal controls' (Stark, 2018).

Many researchers in this niche worked with psychotropic drugs as research materials in efforts to study the brain as substrate of the mind. Some researchers dosed healthy humans with psychotropic drugs to induce the signal symptom of schizophrenia – hallucination. By making 'normal' people temporarily psychotic, they aspired to identify the biochemical cause of mental pathology. Other researchers gave patients psychotropic drugs to make them more tractable for talk therapy – itself an object of study. Researchers tested a range of synthetic and natural psychotropic drugs as they compared rival hypotheses about how the normal human brain worked and effective therapies. The most notorious among them would eventually be LSD (Bachi, 2013; Dyck, 2008; Giffort, 2015; Novak, 1997; Shortall, 2014). In the 1950s, researchers considered LSD a legitimate research tool but felt it had major practical limitations: its effects lasted longer than a typical work day and could not be antidoted once underway. One of the main efforts of researchers was to develop and study additional psychotropics, such as tryptophans, in order to find research alternatives to LSD.

These researchers were trained in a wide range of disciplines, even though they worked in a small number of facilities in a limited geographic range. Researchers were in frequent, sustained contact with mind-brain scientists trained in fields other than their own because of the arrangement of their workspace and daily routines by their common employer: the federal government. The interdisciplinary fields of biological psychiatry and neuro-psychopharmacology were just emerging within an expanding global network of mind-brain scientists who were organizing new professional associations, national conferences and international symposia (Healy, 2002; Langlitz, 2013; Scull, 2015).

Two implications of this spatial arrangement are worth noting. First, researchers were aware of findings, assumptions, and studies in allied fields, whether they wanted to be or not. In addition to carrying out their studies, researchers were fulfilling their administrative duties as employees of the Federal government. From across the three NIMH sites, they came together in conference rooms and cafeterias to approve research protocols, set local policies, and draft annual reports, so were in constant, pragmatic, personal contact. Second, because researchers saw and talked to each other informally, there was little need to write each other letters, circulate memos, or create the kinds of written record conventionally found in archives. They recognized, carried out, and discussed research practices that were widely known and taken for granted. These scientists' practices are all the more important to study because the methods they illuminate were central to mid-century mind-brain science and yet can be easily overlooked because written texts are the privileged form of historical evidence.

In our analysis, we attempt to capture researchers' everyday research practices by examining visual and textual materials, published and unpublished sources, and present-day oral history evidence (Stark, 2017a).[6] In doing so, we articulate tacit practices that researchers described explicitly only on rare occasions. Explicit accounts of these practices may be infrequent, but this cannot be taken to imply that they were uncommon or unconventional since, as STS scholarship shows, scientists have tended to articulate their methods when pushed to defend themselves or to de-legitimate other perspectives during a controversy. By attending to images and approaching texts with an eye to what scientists were doing (without overtly stating) it is apparent that methods of ingression, not only methods of extraction, were both common and taken to be legitimate within this epistemic niche of Federal mind-brain science. No doubt, our study is far from exhaustive in the methods we sample. We intend it to be suggestive, rather than exhaustive, and generative of a new way of conceptualizing methods in STS.

A sample of methods

Method of ingression: Clinical empathy

By the 1950s, psychoanalysis in the USA was a well-established, if not universally respected, therapeutic technique. For mind-brain researchers, regardless of their scientific backgrounds, the psychoanalytic context (Mayer, 2013) was a necessary condition for a method of gathering evidence about patients' interiority, called 'clinical empathy'. The meaning of 'empathy' in psychoanalysis derived from the German term *Einfühlung*, or 'feeling into', which points to how researchers designed clinical empathy as a method of ingression to allow direct access to the interior experiences of others. 'Empathy' had been a freighted concept from the early decades of the twentieth century because it was at the center of a debate between Freud and one of his rivals (Brain, 2015; Lunbeck, 2011). In 1950s America, however, empathy was influentially redefined by research analyst Heinz Kohut as a 'value-neutral mode of observation', rather than as an emotional stance or colloquial catch-all (Kohut, 1959; Lenoff, 2003). Kohut described empathy as one of the 'essential constituents of psychoanalytic fact finding' (quoted in Lunbeck, 2011 see also Kohut, 2011: 212). Kohut's specification of empathy as a method of observation turned him into a leader of the new postwar psychoanalysis. Kohut's work on empathy as a scientific method prompted 'the first successful psychoanalytic revolution', Lunbeck writes, 'Empathy, his signature concept, did yeoman work in advancing his program' (p. 256: see also Lenoff, 2003).

Like many of his contemporaries in the 1950s mind-brain sciences, Kohut worked to refine methods that allowed researchers, first, to know the interiority of another consciousness, and only then to address the additional question of how to represent that interiority; that is, to inscribe it in some external symbol system (words, numbers, images) producing what Craciun (2018: 961) calls 'ego inscriptions': material evidence that psychoanalysts create to establish the credibility of their claims precisely because they use their emotions as tools to better understand study participants' or collaborators' internal states. Lunbeck (2011) writes that Kohut sought to answer the question, 'How was one to observe the inner processes – the thoughts and feelings – of another?' Empathy as a scientific method became

key to his solution. 'He maintained that psychoanalysis had shown it was possible to understand mental processes, to methodologically investigate the mind, by means of transmissible technique', and he cultivated a scientific community that absorbed empathy as a research method. 'He stressed it was foremost a mode of data collecting', Lunbeck writes, 'the analyst-as-scientist properly ordered and scrutinized his assembled data just like any other scientist' (p. 268).

As with many scientific methods, proper training was thought to be crucial to researchers' successful use of empathy. Aspiring researchers, therefore, were trained in clinical empathy as a method of scientific observation, separate from any benefit that researchers' empathy might have in giving patients comfort (see also Craciun's distinction between psychoanalysts' supportive versus inductive uses of emotions). Researchers' own course of analysis – in which they would be the patient – was crucial for developing this research skill. As Lunbeck (2011) explains, '[t]he only analytic truth was that produced by, or between, analyst and patient, and it was necessarily limited by the former's access by means of empathy to the latter's interiority' (p. 269). Analysis was at once a literal swapping of roles that enabled researchers to appreciate the generic patient experience and also a sort of path-clearing to allow researchers a smoother transit into the interiority of specific patients in the future, whether for research, therapy, or both.

At the NIH Clinical Center, it was expected that researchers would use clinical empathy as a method of knowledge production. As a matter of course, new hires to NIMH in the 1950s were routinely requested to undergo psychoanalysis as part of the NIH training program. For example, in 1951, biochemist Seymour Kety was recruited as Scientific Director for NIMH, a job that involved coordinating and overseeing lab and clinical research. Kety's research reputation as a biochemist was based on his work developing techniques to measure cerebral blood flow, and he supported the then-nascent idea that biological interactions with drugs provided clues to mental illness. Shortly after his arrival at NIMH, Kety underwent psychoanalysis as an informal condition of employment. NIMH Director Robert H Felix asked all NIMH researchers to undergo psychoanalysis as standard training, and the NIMH Director of Training arranged for Kety to undergo psychoanalysis at government expense. Although resistant to it, the esteemed biochemist and Scientific Director of NIMH went to psychotherapy four mornings a week for more than a year as part of his job training (Farreras et al., 2004; Squire, 1996).[7]

Kety did not use clinical empathy in his own research, but had to undergo psychoanalysis because the scientists and research teams he supervised did use it. For them, in keeping with the broad influence of Kohut, empathy indicated a formal method of data collection through which an analyst accessed another person's interiority and made unbiased observations of it (More and Milligan, 1994; Savage, 1961). Training in psychoanalysis and the method of clinical empathy it was designed to teach, was regarded as especially important for scientists studying patients with psychoses (such as schizophrenia) who were unlikely to represent their experiences in traditional ways – in grammatical sentences, for example. Even when collecting evidence from healthy 'normal control' subjects, however, clinical researchers regarded empathy as indispensable for specific kinds of studies, such as drug evaluation. When the object of study was an experimental chemical agent, rather than the healthy human to whom the drug had been administered,

clinical empathy was important to researchers because the effects of drugs such as LSD defied conventional modes of representation. Normal controls often experienced hallucinations, nausea, anxiety and other effects that were hard to capture in physiological inscriptions because the drug experiences defied standard description. Teams that included psychiatrists along with biochemists often observed 'normal controls' in experimental drug tests and used not only methods of extraction but also methods of ingression, such as clinical empathy and additional methods, to claim authoritative knowledge of human subjects' interiority, as we show in the next section (e.g. Szára et al., 1966).

Method of ingression: Self-administration

Scientists used the term 'self-administration' to refer to researchers' method of taking experimental drugs, often psychotropic, to gather evidence of drugs' effects on perception and mental states. To be sure, the technique was related to a longer tradition of auto-experiment (Altman, 1986; Dyck and Stewart, 2016; Guerrini, 2003; Halpern, 2004; Herzig, 2006; Lederer, 1995). And on the surface, 'self-administration' can appear to be identical to auto-experiment: a practice in which scientists would take a biologic to screen for toxicity and dosage. Yet for researchers 'self-administration' marked a distinct knowledge-generating activity.

Throughout the 1950s, Federal mind-brain scientists ingested a wide array of psychotropics in labs and government research hospitals (Dyck, 2008). Some drugs arrived in the mail already synthesized and standardized; others they synthesized themselves. William Richards, the researcher we quoted at the start of this article, characterized self-administration as an extension of psychoanalytic methods, framing it as essential to researchers' ability to accurately observe 'these other states of consciousness'. Richards explained in a 2008 interview that the expectation among Federal mind-brain scientists was that researchers would experience the drug they studied in others:

> It was just unquestioned. Anyone we hired new as part of their orientation, would have an LSD session …. I guess the justification for that came from the tradition of psychoanalysis, that you don't go and do psychoanalysis unless you've been analyzed. (Campbell, 2008)

The assumption that researchers needed firsthand experience with what Richards and Pahnke called 'the ineffable' justified self-administration:

> some of these states of consciousness are so beyond what words can describe or what the human mind can even imagine, that it's hard to really be appreciative and remain calmly centered with a person if you don't have some idea of what's going on. (Figure 1)

Researchers had two goals in self-administration. First, researchers aimed to experience temporary psychosis that mimicked schizophrenia. Researchers at the time were searching for a drug that would create a 'model psychosis', which they could then replicate in other healthy humans. If they could find a drug that would make a 'normal' brain experience psychosis, researchers reasoned, then they could pinpoint the biochemical causes of psychosis itself. Thus, self-administration was part of an iterative process

Figure 1. Dr. Harry L. Williams squirts LSD from a syringe into the mouth of Dr. Carl Pfeiffer at Emory University in Atlanta, GA.
Credit: Library of Congress.

researchers used to refine hypotheses about drug receptors, neural pathways, and the workings of neurotransmitters in normal as well as pathological human brains.

Second, researchers aimed to know the types of effects a biochemical had on consciousness. Influential mind-brain scientist Humphry Osmond was an advocate of self-administration. He conducted a literature review and found that all investigators considered self-administration 'the essential precondition for such work' (Osmond, 1957). He explained that self-administration was a crucial method for improving psychiatric research because it enabled investigators to 'see *in* themselves' and thus recognize the inarticulable experiences of patients, both sick and healthy subjects 'under duress'. One potential payoff of self-administration was researchers' better evaluation of new psychotherapeutic interventions because researchers could decipher inarticulable experiences of their analysands or patients. Osmond's work was widely read by mind-brain scientists in the NIMH epistemic niche; his readers included NIMH Scientific Director Seymour Kety and Clinical Center colleagues (Farreras et al., 2004; Kopin, 1995). Part of the research program that Osmond and others advocated was the 'psycholytic model', what today might be called micro-dosing. The researchers Kety oversaw were testing the hypothesis that administering low doses of psychotropic drugs to patients (sick and healthy) helped facilitate psychotherapy. In addition, researchers ran comparative studies of sick and healthy human subjects to determine drug dosages and duration of drug effects. The drug experience of the researcher was implicated in the quality of the research itself. 'Such work', Osmond (1957) argued, 'can only be done by those who are used to these substances'.

There were right and wrong ways to self-administer. Researchers typically arranged for trained colleagues to record their words and actions, and, importantly, to mark time. At the NIH Clinical Center, Kety oversaw one NIMH research program directed by Stephen Szára, who was a biochemist, native of Hungary, and new hire to NIMH in 1956 (Farreras et al., 2004; See oral history interview with Szára in Stark, 2017a). After training in Hungary, Szára was hired by a psychiatric hospital in Budapest, where he started a laboratory for clinical biochemistry. Thanks to the hospital's strong library, Szára stayed current with the growing literature on the biochemistry of psychosis coming from labs in Western Europe and North America. There he read a 1955 report from NIH's Laboratory of Chemistry of Natural Products, headed by a leading American chemist Evan C Horning. The article reported on psychedelic tryptophan derivatives isolated from the seeds of peregrine plants in South America and extracts of the cohoba bean plant, which was used in Haiti to produce hallucinations (Fish et al., 1955).[8] The Horning lab's work documented structural resemblances between the tryptophans, serotonin (which researchers were surprised to find present in the human brain, not only in the human gut as they had known), and LSD, which was considered a serotonin antagonist (as it also is today). Researchers' surprise at the possibility that both too much and too little serotonin in the brain could produce hallucinations inspired new lines of inquiry (Gaddum, 1953; Wikler, 1957: 120). If hallucinogens were serotonin antagonists, researchers surmised, then people diagnosed with schizophrenia might simply be suffering some kind of serotonin imbalance – a deficiency or an excess (Woolley and Shaw, 1962), a blockage in serotonin production, or problems at the receptor site that prevented uptake.

Seeing an uncharted area of clinical research, Szára launched a research agenda from his Hungarian lab into tryptamine derivatives prompted by the Horning lab report and his recent reading of Aldous Huxley's *Doors of Perception*, which describes the author's experience on mescaline (Stark, 2017a). After getting a small supply of several hallucinogens behind the Iron Curtain – no mean feat – Szára decided to do a comparative study of drug effects using self-administration. He ultimately compared the effects of mescaline, LSD and tryptophan derivatives (which he produced in his lab) and presented the results of his comparative study at an international meeting in Milan, which published his findings the following year (Szára, 1957; see also Szára, 1956).

Speaking to the international audience of scientists at the meeting, Szára described his method, namely self-administration. 'I believe that this method of experimentation is one of the best ways of obtaining direct information on subtle psychopathological phenomena, which are of great importance in understanding the schizophrenic syndrome', he told the scientific audience (Szára, 1957: 461). In a 2014 interview, Szára explained more and less rigorous practices of self-administration. He contrasted his experience taking a tryptophan called DET in a hospital setting where colleagues observed and recorded him in a standard setting, with his earlier self-administration of mescaline alone at home, which he regarded as scientifically useless. 'This time I did it in the hospital, and with experts around me, my colleagues. They were asking, "What's happening to you?" Within two minutes after the injection the whole world is exploding' (See oral history with Stephen Szára in Stark, 2017a). By carrying out the method properly – in a constrained setting with other scientists recording time, behavior, and his descriptions

– Szára was able to present and publish a comparative chart of dose, date, location and drug. His research was accepted by his peers to be a reliable report of the relative effects of the drugs on a person's interiority, specifically on perceptions and emotions.

At the Milan meeting where he reported his findings based on evidence from self-administration, Szára was interviewed and hired by NIMH to work under Joel Elkes, who had been newly recruited to NIH to direct the NIMH Behavioral and Clinical Studies Center at St. Elizabeth's Hospital in Washington, DC, and the Clinical Neuropharmacology Research Center at NIH Clinical Center.[9] Elkes was a trained psychoanalyst and his research on myelin in the central nervous system had established a neurochemical basis for psychiatric phenomena. He had founded the Department of Experimental Psychiatry at the University of Birmingham in England, which was among the first experimental facilities dedicated to psychiatry and psychopharmacology (Shorter, 1997; Stafford, 2015). When Elkes hired him at NIMH, Szára split his time between the NIH Clinical Center and St. Elizabeth's Hospital, working at both sites on comparative studies of hallucinogenic drugs using schizophrenic patients and 'normal controls'. Szára's work also involved training postdoctoral researchers at NIH.

Szára's training regimen for young researchers included self-administration. For example, Szára mentored psychiatrist Lawrence H Rockland and together they observed and recorded the effects of the tryptophan DMT on each other. They then used their first-hand knowledge of 'altered consciousness' to interpret data they collected in comparative studies of DMT on 'normal controls' living in the NIH Clinical Center – the meaning of which was otherwise difficult to decipher – and on patients diagnosed with psychoses, such as schizophrenia, institutionalized as St. Elizabeth's Hospital (Faillace et al., 1967; Szára et al., 1966). Thus self-administration allowed them to warrant their interpretations of interior experiences of human subjects beyond themselves. As Szára explained, self-administration was part of an overall 'evaluating process for the changing mental states of the patients'. Scientific reports based on self-administration were considered authoritative to the extent that they allowed researchers to justify additional studies of DMT on schizophrenic patients and normal controls.

By the late 1950s, the method of self-administration had multiple goals. Some researchers practiced self-administration of psychotropic drugs to experience temporary psychosis to refine hypotheses about the neurochemical basis of human and animal behavior. Other researchers sought to experience temporary psychosis in order to interpret data that they observed in their 'normal control' patients and in psychotic patients – that is, to warrant their scientific claims about the interior experiences that corresponded to patients' words and deeds that were beyond representation. To be sure, some researchers also used psychotropic drugs recreationally (some loved, while others loathed, the drug experience). Some also ingested the drugs as a more familiar form of auto-experiment, that is, to screen for drug safety and appropriate dosage. But for Szára and colleagues, 'self-administration' was a specific method that enabled them as trained scientists – in controlled experimental settings and following specific protocols – to apprehend the interiority of others who by definition could not represent their own experience. These researchers were neither unique nor isolated in using 'self-administration' as a method of ingression.

Method of extraction: Physiological inscription via electroencephalogram (EEG)

In any given study, NIMH research teams toggled between methods of ingression and methods of extraction to triangulate their objects of study, whether mental illness or therapies to treat it. NIMH scientists, including Szára and Rockland, practiced and trained in clinical empathy and self-administration, and yet also typically incorporated physiological tests into their research. As Szára explained, their research team occasionally included David Rosenthal and JH Handlon, two research psychologists who gave research subjects questionnaires and 'ran the gadgets' in the reaction-time tests the team also gave to subjects as part of the same study protocols (See oral history with Szára in Stark, 2017a; see also Szára et al., 1966). Thus 'normal control' patients who were on the receiving end of researchers' methods of ingression in NIMH research wards were also simultaneously subjects of methods of extraction – that subset of methods that scientists designed to pull out evidence of people's inner experience from their body-minds.

By the early twentieth century, researchers regarded tests of short-term changes in people's physiology – from heart rate to hormone level – to be external, material indexes of people's inner experience. One of the most recognizable mid-century technologies for extracting signs of inner experience was the electroencephalogram (EEG). Building on the premise that the physical brain was the location of the mind, scientists worked to decipher signals from the brain as a way to access the mind itself (Canales, 2011; Hagner, 2012; Hagner and Borck, 2001). The EEG was originally assembled in the 1920s, around the idea that brain waves represented the material basis of people's psychic and sensory worlds in the form of detectible electrical activity that correlated with interiority (Borck, 2001; Millett, 2001). By the 1930s, the American and European scientific community had begun to accept that brain waves existed, that they were detectible, and that they were electrical evidence of mental activity (Belkin, 2014). The analogy to radio was central in the development and popularization of the EEG. Some hypothesized that especially sensitive people emitted their own electrical currents (called cerebral radiation) or absorbed those transmitted by others on the same 'wavelength'. Writing for popular audiences, Upton Sinclair explained in his book *Mental Radio* how his wife was able to communicate in this way (Borck, 2013; Sinclair, 1930).

The EEG's apparent ability to detect electrical currents passing through people's heads without external stimulation – such as sounds, sights, visions – led experts to hope (and others to fear) that electrical stimulation could be used to insert knowledge into people's heads or even render them more or less 'suggestible'. Chemists, physiologists, and psychiatrists explored whether the hallucinogenic effects of psychotomimetic agents (like LSD or cohoba plant) might be based in electrophysiology, developing a field called pharmacoelectroencephalography (Canales, 2011; Fink, 1984, 2010). At the 1961 World Congress on Psychiatry, one scientist assessed the reputation of the EEG from the end of the 1950s, and summarized that it was a 'matter of astonishment that so many of the brain's secrets escape across the wall of the skull to electrodes fixed to the scalp of man. That they do so is testimony to the fact that the brain's electrical activity is a most sensitive indicator of its function' (see also Fink, 1969; World Congress of Psychiatry, 1961). Thus the EEG joined a suite of machines in mid-century America, including the lie

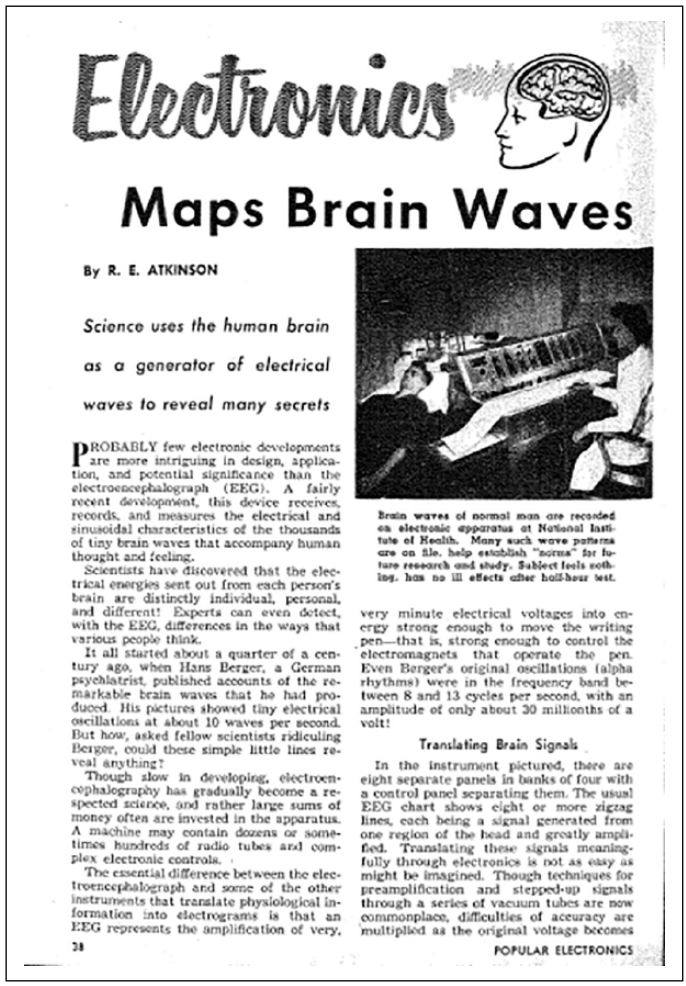
Figure 2. 'Normal control' Dale Horst appeared in a staged photo with an EEG at the NIH Clinical Center, circa 1955.
Credit: US National Institutes of Health, Office of Patient Recruitment and Public Liaison.

detector and magnetic-resonance spectrometer, that scientists promoted on the premise that people's inner experience – unexpressed or inexpressible knowledge – could be pulled out of them with the assistance of these machines (Alder, 2007; Dumit, 2004; Prasad, 2014).

The EEG was among the most iconic technology in scientists' toolkit of physiological recording technologies. This feature helps to explain why the methods of extraction are more challenging for STS scholars to detect today than methods of ingression. Inside NIMH facilities, the sheer size and apparent intricacy of the machine was promoted as an emblem of scientific might that overshadowed less technology-intensive methods of ingression. At the Clinical Center, for example, Federal administrators arranged requests for photoshoots from the popular press to include the EEG (Figure 2).

An article from *Popular Electronics* in 1956 pictured one 'normal control' at the Clinical Center, Dale Horst, in an EEG study. Scientists used Horst in research protocols in which team members used methods of extraction, such as collecting EEG data, in concert with methods of ingression, specifically clinical empathy, which emphasizes our point that scientists developed both types of method by holding them in dynamic relation to each other (Savage, 1955; See oral history with Walter Dale Horst in Stark, 2017a). The article on the EEG tempted readers with the title 'Science uses the human brain as a generator of electrical waves to reveal many secrets' and the piece described the implications:

> Probably few electronic developments are more intriguing in design, application, and potential significance than the electroencephalograph (EEG). A fairly recent development, this device



Electronics

Maps Brain Waves

By R. E. ATKINSON

Science uses the human brain as a generator of electrical waves to reveal many secrets

PROBABLY few electronic developments are more intriguing in design, application, and potential significance than the electroencephalograph (EEG). A fairly recent development, this device receives, records, and measures the electrical and sinusoidal characteristics of the thousands of tiny brain waves that accompany human thought and feeling.

Scientists have discovered that the electrical energies sent out from each person's brain are distinctly individual, personal, and different! Experts can even detect, with the EEG, differences in the ways that various people think.

It all started about a quarter of a century ago, when Hans Berger, a German psychiatrist, published accounts of the remarkable brain waves that he had produced. His pictures showed tiny electrical oscillations at about 10 waves per second. But how, asked fellow scientists ridiculing Berger, could these simple little lines reveal anything?

Though slow in developing, electroencephalography has gradually become a respected science, and rather large sums of money often are invested in the apparatus. A machine may contain dozens or sometimes hundreds of radio tubes and complex electronic controls.

The essential difference between the electroencephalograph and some of the other instruments that translate physiological information into electrograms is that an EEG represents the amplification of very,

Brain waves of normal man are recorded on electronic apparatus at National Institute of Health. Many such wave patterns are on file, help establish "norms" for future research and study. Subject feels nothing, has no ill effects after half-hour test.

very minute electrical voltages into energy strong enough to move the writing pen—that is, strong enough to control the electromagnets that operate the pen. Even Berger's original oscillations (alpha rhythms) were in the frequency band between 8 and 13 cycles per second, with an amplitude of only about 30 millionths of a volt!

Translating Brain Signals

In the instrument pictured, there are eight separate panels in banks of four with a control panel separating them. The usual EEG chart shows eight or more zigzag lines, each being a signal generated from one region of the head and greatly amplified. Translating these signals meaningfully through electronics is not as easy as might be imagined. Though techniques for preamplification and stepped-up signals through a series of vacuum tubes are now commonplace, difficulties of accuracy are multiplied as the original voltage becomes

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Figure 3. At the Clinical Center, new technologies to extract traces of people's internal experiences were used to promote Federal mind-brain research in the popular press, such as in the 1956 issue of *Popular Electronics*. The 'normal control' pictured is Dale Horst, who was serving a one-year voluntary service term as a 'healthy patient' through the Mennonite church. The technician is a model brought in for the photo shoot, according to Horst (Stark 2017a).

> receives, records, and measures the electrical and sinusoidal characteristics of the thousands of tiny brain waves that accompany human thought and feeling.

The 1956 article explained that the EEG could reveal peoples' sympathies for different sports teams and distinguish between soldiers who were true 'mental cases' and those who were merely 'malingering' (Atkinson, 1956: 39). There was more. 'Experts can even detect, with the EEG, differences in the ways that various people think', the

journalist wrote following his visit to the NIMH wards inside the Clinical Center. 'But the instrument does not really read minds … not yet, anyway' (Atkinson, 1956: 39).

Scientists hoped that methods of extraction, which often relied on machine recording technologies like the EEG, could materialize and therefore make knowable those things that people could not express – or refused to say. In its 1954 Annual Report, NIMH described Federal scientists' use of the EEG at the Addiction Research Center in Lexington, where research teams bundled a variety of methods to study the effects of drug withdrawal. 'Former addict volunteers are chronically intoxicated with alcohol or barbiturates which are finally withdrawn abruptly; physiological, psychological, biochemical, and laboratory observations are made during both phases; various methods of withdrawal (discontinuing drugs) are studied …' (NIMH, 1959). As part of their protocol, researchers hooked the men up to an EEG to observe brain functioning, which showed that substituting alcohol for barbiturates reduced the (bad) brain activity that accompanied symptoms of withdrawal.

Yet many Federal researchers were skeptical of the EEG and others keenly aware of its shortcomings. Although they used and promoted the EEG as the best available technology at the time, they suspected that EEGs picked up artifacts, that is to say, outputs that were misleading because they were the function of the experiment itself or ancillary to the phenomena the machine was imagined to capture. In addition, experts felt that the evidence the EEG produced was far from definitive because of the interpretive work necessary to 'read' EEG output. One science journalist explained the problem and researchers' tepid endorsement of the EEG at the Clinical Center. 'There is little doubt that the brain wave recordings have vastly more inherent information than scientists now can understand', he wrote. 'EEGs may rightly be called "coded messages from the brain"'. Yet the task of 'decoding' these messages was a matter of human interpretation and there was no Rosetta stone. 'As science learns how to decode these "brainprints" more thoroughly, their contents may reveal more about how the brain works as well as prove to be a valuable weapon in man's fight against disease' (Atkinson, 1956: 39). Despite the advantages of machine-produced evidence, human interpretation was still necessary and proved to be a limiting factor. Such challenges led NIH Clinical Center scientist Edward Evarts to advocate a new machine, the 'evoked response detector' (Cox and Evarts, 1961).

The weaknesses that researchers associated with EEG as a method of extraction help to explain why these scientists continued to use and develop methods of ingression within a given research protocol. They felt methods of extraction, like methods of ingression, yielded partial knowledge about referents that could never entirely be brought into view.

Method of extraction: Standardized questioning

In the 1950s, Federal mind-brain scientists seemed constantly to be asking questions. Working between the Clinical Center, the Addiction Research Center, and St. Elizabeth's Hospital, they administered questionnaires, interview protocols, surveys, scales and other standardized batteries of questions (Butcher, 2010; Gibby and Zickar, 2008; Miller, 2015; Schilling and Casper, 2015). They did so both independent of clinical experiments and during experiments designed to establish the essential elements of personality structure or disease profile, to discern drug effects, or to gauge the effectiveness of therapeutic

interventions. Researchers posited that people's responses to questions were an index of inner worlds that could be materialized on paper from spoken or written responses, a premise that signaled researchers' epistemological commitment to knowing a person's interiority by bringing it out of them.

Throughout the 1950s, Federal mind-brain scientists often asked standardized questions during trials of experimental drugs on psychiatric patients, as well as trials on normal controls. They designed questions to compare multiple drugs, or to detect when a drug wore off by documenting when healthy human subjects started sounding 'normal' again; that is, when they expressed conventional thoughts and feelings according to scientific observers. Within the epistemic niche we observe, researchers at the Addiction Research Center first used standardized questions on post-addicts in clinical trials of analgesics, barbiturates, chlorpromazine, hallucinogens, and other experimental drugs, after which their colleagues at the Clinical Center followed suit, using the same methods on healthy 'normal controls' who were given experimental drugs. Often researchers verbally delivered their standard questions through research protocols, since literacy could not be assumed and sanity was often the ultimate question. They recorded and transcribed their human subjects' verbal responses, and often reproduced those responses verbatim in published reports (e.g. Stark, 2017b).

Research teams working between the Clinical Center, the Addiction Research Center and St. Elizabeth's Hospital also regularly extracted data that was then translated into quantitative scores using one of many scales invented for the purpose. Researchers sought to close the gap between a quantitative output and the wide-ranging, idiosyncratic verbal responses their human subjects might offer by requiring that subjects respond to questions with numbers rather than prose. Thus, in any given study, they might combine standardized interview protocols, questionnaires, surveys and scales so that material extracted in qualitative form could also be inscribed as quantitative evidence of 'subjective effects'.

But the accuracy of this method for indexing inner states was perpetually under suspicion by the very mind-brain scientists asking the questions. Researchers suspected that conventional assessment instruments were not accurately registering the experiential effects of drugs – but were rather registering artifacts of the clinical setting. They felt that social context and relationships affected how subjects responded to questions, making it difficult to know whether people's answers measured the interior experiences caused by biochemicals or the effects of clinical encounters. Federal mind-brain researchers were alert to what psychologists called 'experimenter effects' and worked to standardize social settings as much as the questions asked and the dosages delivered (Stark, 2010). For example, Joel Elkes – the director of St. Elizabeth's Hospital who had hired Szára and who, like Szára, worked across NIMH sites – described the importance of social arrangements in shaping subjects' inner states and therefore the answers they gave, since that clinical research, as opposed to bench research, inevitably involved 'a group of people' (Elkes and Elkes, 1954: 573). The move to quantify their subjects' responses emphasized a key point of weakness of the method of extraction: that even in quantitative form, the evidence extracted was an accomplishment of human interaction in time and space.

At NIMH facilities the number of instruments available for standardized questioning proliferated, rather than narrowed, and were rapidly replaced, rather than stabilized

– despite NIMH's attempts to consolidate and validate the instrument of choice. For instance, in 1959 Rockland, the young psychiatrist whom Szára had trained in self-administration and had collaborated with, developed a scale with senior NIMH scientist William Pollin to use with psychotic patients. Rockland and Pollin (1965) created the new scale because they felt other instruments proved inadequate to quantify levels of 'psychoticness', and they designed their own scale to rate patients along sixteen functional continua. The Rockland-Pollin scale was validated in 1959, and was for a brief time widely used, before being discarded. In a 2014 interview, Rockland recalled 'that scale became pretty popular in the field for a while, for about five years. And then it was replaced by another ostensibly better scale, an easier scale called the BPRS, the 'Brief Psychiatric Rating Scale'' (See oral history with Rockland in Stark, 2017a). The scale had become part of NIMH routine, only to be replaced. 'For about five years I'd meet researchers who would say, "Oh you're the Rockland of the Rockland-Pollin scale." It was pretty good for a while', Rockland remembered, 'But it didn't last.' When asked to explain why this once-popular scale was replaced, he replied, 'I couldn't really answer that. I think that – most honestly I don't know. I don't know.' The quick, inexplicable replacement of widely accepted, validated instruments points to researchers' limited confidence in any given instrument to extract interiority.

There were instances in which NIHM leaders succeeded in stabilizing an instrument. But they did so by building large batteries that subsumed multiple existing instruments that had been introduced to the research site by the personal tastes or distinctive research experiences of a researcher hired in from another institution. For example, during the mid-1950s, Federal mind-brain scientists regularly administered the Jarvik-Abramson scale (Abramson et al., 1955), a practice that psychologist Conan Kornetsky brought with him when he was hired at the NIH Clinical Center in 1954 from its local site of origin in New York City. Kornetsky also carried the Jarvik-Abramson scale to the Addiction Research Center, where scientists then incorporated it into an elaborated Somatic and Dysphoric Scale (for LSD) that became part of the ARC Inventory (ARCI), which included multiple scales to allow researchers to classify drugs based on subjective effects.

During the 1950s, the number of instruments proliferated because they were under suspicion by the very researchers who used them. Scientists who held power in the hierarchy of a research team perpetually developed new instruments or introduced their own trusted favorites in new locations to manage their worries. Thus, scientists' use of questionnaires, interview protocols, surveys, scales and other batteries of questions registered their epistemological commitment to knowing the ineffable by extracting it at the same time as it revealed their sense of the impossibility of the task.

Conclusion: A new framework for studying methods

'We can know more than we can tell', wrote the chemist and philosopher of science Michael Polanyi (2009: 4). In his classic formulation of tacit knowledge, Polanyi observed that the craft of making knowledge was a full-body experience that relied on capabilities that exceeded the strictures of language. The work of science involved skills, sensibilities, and modes of comportment that could not fully be expressed in conscious thought, much

less in written word – in textbooks, for example, as Kuhn (2012 [1962]) famously observed (see also Kaiser, 2005; Sibum, 1995; Warwick, 2003). Polanyi's point was not to register the shortcomings of scientists, but to mark their surfeit of knowledge that defied language. Polanyi's observations about tacit knowledge have been absorbed into STS and have invited questions about how experts succeed (and fail) in making their claims credible. This is a puzzle because, on the one hand, experts in the dominant traditions of modern Western science claim that their evidence and findings are credible to the extent that human intervention was minimized, managed or, ideally, absent. On the other hand, as Polanyi reminds, scientists rely on their own senses and sensibilities – their own flesh and bones – to create, share, interpret, and package knowledge.

Through the literature on 'epistemic virtues', scholars in STS have explored the ways in which scientists managed these credibility claims by developing and deploying methods to limit the appearance of human sensory influence. In this article, our aim has not been to replace the epistemic virtues framework; we insist that it remains a useful analytic device for studying the moral economy of science. We observe, however, that experts – not only the scholars who study them – have also been alert to Polyani's insight that people can know more than they can tell. The challenge for experts has been to develop methods for making knowledge about others' experience – of mental illness, of dream states, and much more – with the recognition that other people, or other beings, cannot always express their interior experience through words and other forms of representation (images, numbers), which might otherwise serve as scientific evidence.

One main argument of this article is that many researchers have regarded their own interiority not as a problem to be overcome but as a resource that, when carefully, expertly managed, can aid the process of knowing the mind-body experiences of others. We have used the term 'methods of ingression' to refer to those techniques scientists actively deploy to access the interior experience of other beings through their own interiority. To be sure, scientists also generated knowledge of others' interiority by attempting to pull it out of people's mind-bodies and to remove their own minds-bodies from the process. We refer to these techniques as 'methods of extraction'. In the epistemic niche we examine, scientists did not consider these methods inherently superior to methods of ingression. Instead, scientists regarded each type of method as having advantages as well as weaknesses. They adopted and refined methods of ingression to counterbalance methods of extraction because of the reductive and distortive effects of extraction on the very objects they sought to know. Thus, a second main argument of this article is that methods of ingression and extraction not only cohabited in the same experimental spaces, scientists developed these different types of methods in dynamic relation to each other. Rather than imagine these methods in an agonistic field, in which one type was expected to dominate and the other to fade from use, researchers held them in productive tension – developing methods of extraction and ingression in a generative, compensatory fashion.

Our observation that neither type of method was considered inherently superior points to the usefulness of our framework. The language of 'subjectivity' has bedeviled STS in recent years because 'subjectivity' has been used to refer to two analytically distinct features of methods: the epistemological premises of a method, on the one hand, and the evaluation of the method in the moral economy of science, on the other. As our analysis shows, there is neither a direct nor obvious relationship between epistemological assumptions built into methods and the moral evaluation of them.

We propose our framework of methods to bring clarity to important debates where it has become common to conflate interiority (as a tool or target of research) and inferiority (as a judgement about quality of research). For example, in an otherwise insightful article on Western scientists' research into brain disease among indigenous Pacific islanders, Anderson (2013) interchanges 'subjectivity' with 'relativity' – terms that he uses to refer to the practices that a legendary virologist used to embed, sympathize and emote with his human subjects in an effort to remove bodily materials from their local, interactive settings. In Anderson's case, the scientist was not treating himself as a subject – which is a well-established and thoroughly studied practice – but was seeking to temporarily become his subjects; akin to 'going native' but with effort and intention, rather than by accident. As Anderson puts it, the scientist cultivated and practiced methods 'of opening ourselves to others' (p. 559), and argues that the scientist did not avoid or hide the methods he used to feel his way into his human subjects. Anderson shows that what we call 'ingression' (and he calls subjectivity and relativity) was necessary for the scientist to claim to fully understand his subjects and was productive of authoritative scientific knowledge: the scientist's work was ultimately celebrated with a Nobel Prize. Anderson's recourse to the seemingly contradictory claim that scientists can work both objectively and subjectively shows how two analytically distinct features of knowledge-making can get confused. Anderson uses the language of subjectivity to describe scientists' legitimate method (ingression) and objectivity to describe colleagues' positive moral judgement about the method (that is, its epistemic virtue). In doing so, Anderson exemplifies the usefulness of our framework.

We offer the framework of ingression-extraction to orient STS in new ways. First, we hope scholars may find the framework useful in clarifying and broadening the analytic vocabulary of STS. We suspect that the confusion surrounding the two analytically distinct meanings of 'subjectivity' is an artifact of a framework for 'epistemic virtues' that was developed based on examples from the physical sciences. By expanding to the human sciences – without excluding the physical sciences, such as chemistry – our framework allows STS scholars to attend to a fuller variety of methods that scientists used authoritatively to study interiority, and at the same time can be used in parallel with analysis of how those methods operate in the moral economy of science. The framework can also map relationships across disciplinary lines and institutional spaces that may seem surprising or be easy to miss. Among other things, we hope our framework might allow scholars, in the words of Canales (2001), 'to explore the forgotten common ground between the physical sciences and the sciences of man, and bring together the severed histories of objectivity and the body'.

Second, the framework of ingression and extraction helps thematize case studies in STS that currently stand in isolation. Through cases that have ranged from bibliotherapy to crystallography, from oenology to cetology, STS scholars have documented how experts use their mind-bodies, first, to understand and, second, to express the ineffable (Burnett, 2012; Dufour, 2014; Myers, 2015; Shapin, 2016). By extending the framework beyond the niche we examined here, it will enable scholars to recognize similarities in the questions and findings of their individual case studies. Further, we hope the framework will help to organize studies yet to be done. Consider, for example, the controversy surrounding the science of Virtual Reality (VR). Used by humanitarian organizations to

compel potential donors and activists towards humanitarian action, VR technologies have been claimed to recreate for viewers the interior experience of refugees or people who are homeless (Decety, 2012; Rosenberg et al., 2013; Sydell, 2017). Yet VR humanitarianism has detractors, who warn against VR experiences not because they fail to make viewers inhabit the interiority of others, but because they do it *too well* (Bloom, 2016). Research into technologies of ingression, to complement the vast literature on technologies of extraction, seems an area ripe for study in STS.

Finally, scholars in STS are knowledge makers too, and methods used in STS have affinities with those of the scientists we studied. Witness, for example, efforts to reproduce experiments based on the assumption that in doing so we will glean insights from our own mind-bodies about researchers' interiority (their gestural knowledge, for example) that exceed documentary evidence (Long, 2001; Sibum, 1995). Likewise, ethnographic methods in STS are often taken to be credible to the extent that scholars convincingly claim to have approximated in their own minds and bodies the interior experiences of the people they seek to know (Dickson and Holland, 2016). As we mentioned at the start of this article, the influential ethnographer Bronislaw Malinowski developed and advocated a method of participant observation in which 'the anthropologist came to understand his subjects by embracing their way of life' (Kuklick, 2011: 23). Postwar graduate training programs have continued to teach that the ethnographer 'becomes the other through experience' (Biehl et al., 2007; Rabinow, 2007; Salzman, 2002). Similarly, in the field of history, Collingwood advocated 'reenactment' (a technique of disciplined imagination) as a method to know the interior experiences of historical actors (Collingwood, 1999; D'Oro, 2000; Dray, 1995; Ivanhoe, 2009). And the translator's method was developed with the aim 'to cross over into the affective and symbolic universe of those they wanted to capture' (Wintroub, 2015). Finally, Kuhn (2012 [1962]: 280) argued that the task of the well-trained historian was 'to climb into other people's heads' (see also te Hessen, 2018). Kuhn modelled his ideal historical method on his personal experience of psychoanalysis – a vision of historical method as psychoanalysis that he held for his entire career (Forrester, 2007: 790). Forrester, a former student of Kuhn, has documented that it was the 'experience of climbing into one's own thought processes' that allowed Kuhn to treat the minds of the scientists he studied as 'other' through an iterative, recursive process of ingression (p. 793). Beyond Kuhn, feminist epistemologies in STS have pointed out how practices of knowledge-making – both among the scientific communities we study and within our own field – are based on relationships of interdependence with the existences we seek to know. They do not preexist our efforts to know them, but are produced in the very process of knowing, which for many feminist scholars involves a responsive, embodied process of ingression (Barad, 2007; Haraway, 2007; Kohn, 2013; Myers, 2015). By theorizing the methods of Federal mind-body scientists and of STS with this feminist point in mind, we hope our framework can help scholars to more aptly describe scientists' epistemic worlds; to discern a greater range of methods at play; and to appreciate the warrants for knowledge in our own field.

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Notes

1. Research on methods of extraction include work on physiological tests such as electroencephalograms (Borck, 2001; Farreras et al., 2004; Hagner and Borck, 2001; Millett, 2001; Pérez, 2016), projective tests such as the Rorschach Inkblot Test, Goodenough Draw-A-Person Test, or Thematic Apperception Test (Akavia, 2010; Buchanan, 2000; Capshew, 1999; Galison, 2007; Herman, 1996; Lemov, 2011a, 2011b; Miller, 2015; Puglionesi, 2016), and formal surveys, structured interviews, scales, and inventories to extract inner elements from desires to dream states (Carson, 1993; Gibby and Zickar, 2008; Igo, 2007; Lemov, 2015; Morawski, 2015; Schilling and Casper, 2015).
2. E.g. Burnett (2012), Chong (2013), Guenther and Hess (2016), Hennion (2007), Laemmli (2016), Miller (2015), Myers (2015), Perlman (2004) and Winter (2012).
3. Moreover, from the late nineteenth century, authoritative science became increasing defined as raw method (Cowles, 2015).
4. No doubt, the history of the human sciences shows that externalized evidence, or 'mechanical objectivity', was increasingly prized, for example among dominant physiologically grounded experimental psychologists in the late nineteenth and early twentieth centuries. We do not contest the importance or the accuracy of these historical accounts. Rather, we follow the work of historians who have documented a parallel history of science, and who demonstrate the value of Daston and Galison's idiom of layering, rather than replacement, of methods (Riskin, 2017).
5. NIMH researchers also moved between Federal custodial facilities for intellectual and material resources, including Spring Grove State Hospital in Maryland; the medical campus of The Johns Hopkins University; the University of Maryland at College Park; the Walter Reed Army Institute of Research; and Edgewood Arsenal Chemical Center.
6. Oral histories, historical images and documents, and additional materials from more than one hundred former Normals at the Clinical Center and the scientists who studied them are collected in the Vernacular Archive of Normal Volunteers generously maintained by Harvard's Countway Library of Medicine. Access to some data files is restricted until 2019. Access to restricted data may be granted at the discretion of the author; to request access, contact Laura Stark (laura.stark@vanderbilt.edu). See Stark (2017a).

7. Kety described the arrangement:

   [S]hortly after my appointment at the NIH, Seymour Vestermark, director of training at NIMH, asked whether I would like to have some experience with psychoanalysis which he thought would be only reasonable for the scientific director of a mental health institute… Dr. Edith Weigert, probably the senior psychoanalyst in the Washington area, agreed to be my analyst and I spent more than a year in a classical analysis four mornings a week. I found it very pleasant, lying relaxed and talking about myself; I remember dipping into the unconscious very sparingly, but enough to make me aware of its existence.

8. In 1950, NIH recruited Horning from the University of Pennsylvania to the National Heart Institute to start the laboratory for the study of natural products.
9. Elkes arranged for an NIH colleague to interview Szára on May 9–11, 1957, at an international symposium on psychotropic drugs in Milan, Italy. The symposium was one of several international events that built relationships between researchers in the USA and in Western Europe and Soviet protectorates (Garattini and Ghetti, 1957).

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Author biographies

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