

## *Sleep and Dreams*

### Dream Research: 1953–1993

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**Summary:** A participant gives his account of the history of electrophysiological dream research in the 40 years following the discovery, reported by Aserinsky and Kleitman in 1953, of rapid eye movement sleep and of its relation to vivid dreaming in the adult human. **Key Words:** Dreaming—Dream psychophysiology—Dream research, history—Dream theory—REM sleep.

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This is, perforce, merely one person's account of the history of dream research since the discovery of REM sleep at the University of Chicago. My career-long participation in the field began during a second stage of dream research at that university, first as a student in Joe Kamiya's lab and later as a postdoc in Allan Rechtschaffen's lab.

#### 1953

The watershed year in 20th century research on dreaming was 1953. It was in 1953 that Eugene Aserinsky and Nathaniel Kleitman, at the University of Chicago, published their discovery of sleep periods with rapid eye movements (1). They also reported preliminary observations (50 awakenings) that dream experience apparently both reliably and almost exclusively accompanies these sleep periods. Thus, it seemed likely that recordings of these periods of (as it later was to be called) rapid eye movement, or REM, sleep could provide an "objective" indicator of the occurrence of dreaming as well as an efficient means for collecting reports of dream experience immediately upon such occurrence.

In the same year, Glenn V. Ramsey (2) systematically reviewed studies of dreaming performed before the Aserinsky-Kleitman discovery. These studies necessarily relied upon more conventional means of soliciting dream reports (questionnaires, interviews). Ramsey established that such studies had been both

relatively infrequent and largely unproductive: little more had been reliably established by this research than would already have been suspected by any unsophisticated self-observer.

Finally, Calvin S. Hall published one book (3) and two journal articles (4,5) proposing a "cognitive" theory of dreaming and dream symbolism. Dream content was viewed as reflecting the dreamer's concepts or knowledge of self and world, and dream symbolism was portrayed as largely expressive rather than defensively motivated (cf. 6). Although Hall's writings did not have highly visible effects on the field in the 1950s, they were later to prove influential and prescient. More immediately, in the 1960s (7), there were effects in the direction of providing exhaustive quantification in studies of manifest dream content (Ramsey had decried the lack of quantification and the generally low quality of both research design and data reporting in pre-1953 dream research). In the longer run, Hall's theorizing presaged attempts, in the 1980s, at integrating dream study theoretically with the empirical study of the waking mind ("cognitive psychology") (8,9).

The discovery of REM sleep itself—surely the single most important event in modern dream psychology—has been described by Aserinsky (10). Therefore, I will present—in relatively broad strokes because of restrictions on length—a conceptual history of the substantial transformations that that discovery wrought upon empirical dream research. In a field itself historically prone to confuse the two concepts, I want to make clear that the focus here is on dreaming, the experience, not on REM (or any other kind of) sleep. As soon will be seen, one of the recurrent research

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issues in the field has been whether REM sleep is, in fact, either a necessary or a sufficient condition of dreaming-as-experience. I also am focusing mainly on the findings of research either generated by sleep-laboratory methodology or sensitive to sleep-laboratory data. Studies continue to appear that are either ignorant or contemptuous of this research; by and large, they fall prey to the same sorts of evaluations Ramsey made of pre-1953 research. Compared with nonlaboratory research, work in the REM-monitoring tradition has given the promise of genuine progress in understanding dreams and dreaming. A major concern here will be to evaluate the extent to which such promise has, in fact, been fulfilled.

### **DREAMING AS REM SLEEP: THE DEMENT YEARS**

Before Aserinsky and Kleitman (1), there had been a number of studies of the occurrence of dream reports on awakening following the appearance of certain physiological variables, including the early alphabetic classification of electroencephalogram (EEG) sleep stages (11). It is interesting that this research did not find dreaming limited to EEG activity later identified as occurring during REM sleep. In fact, the most systematic of these studies in terms of searching for EEG correlates of dreaming (12) reported "that dreams may occur in association with any type of sleep potential pattern" (p. 12). It also is interesting that Ramsey's review (2) did not foresee the possibility that an "objective", i.e. physiological, indicator of dreaming might help to resolve at least some of the dream issues on which he reviewed evidence (e.g. how often do people dream? how long do dreams last?).

In its initial years, until 1957, electrophysiological (REM-monitored) dream research was largely synonymous with the career of William C. Dement (1928-), who worked in Chicago after Aserinsky had discovered sleep's REM. Dement reinforced the hypothesis that REM = dreaming with an independent investigation whose pooled results, however, came mainly from only five subjects (13). He then set forth indefatigably to answer some of the major questions of dream psychology, based on faith in the REM = dream equation. His interests in those years, culminating in his dissertation in physiology at the University of Chicago (14), were primarily focused on dream experience, in a way they never again would be. During that period, Dement was responsible for discovering and publicizing the major contributions of psychophysiological methodology to the understanding of dreams and dreaming. That is to say, he was largely responsible for findings that, decades later, still constituted introductory textbook answers to such questions as how often do we

dream?, how long do dreams last?, is dream time the same as real time?

In Ramsey's review (2), these questions had seemed unresolved or irresolvable. A few short years later, based on the equation that REM = dreaming, answers were confidently being supplied to them. Because we have multiple REM episodes every night, we dream several discrete dreams each night; because REM episodes last minutes rather than seconds, so too do dreams; because it takes about as long to act out a dream as the elapsed amount of REM sleep from the REM episode from which it was retrieved, dream time is more or less the same as real time. That was a period, perhaps unprecedented elsewhere in psychology, in which age-old questions suddenly seemed both resolvable and resolved. The heady atmosphere of this brief period, captured by, among others, Diamond (15), Trillin (16), and Luce and Segal (17), was soon to give impetus to a vast expansion in empirical research on dreaming, to the formation of a specialty organization where this research was presented and discussed [The Association for the Psychophysiological Study of Sleep (APSS), which met first (but under no particular name) at the University of Chicago in 1961], and to the creation of a new multidisciplinary research specialty ("sleep and dream research"). Anyone as successful as Dement had been surely was not meant to stand alone for very long.

Not all of Dement's original observations, nor, even more clearly, his interpretations, survived the following decades unscathed. As Starker (18) has observed, "Pioneering has never been a tidy process" (p. 899). It thus entails no diminution of Dement's accomplishments to note both that he was the unquestioned pioneer of the new (laboratory) dream psychology and that things never looked so clear-cut in succeeding years as they did in the late 1950s when Dement was, all by himself, this new field.

### **A FIELD IS BORN**

Upon completion of his medical and Ph.D. degrees at Chicago, Dement moved to New York City for his internship and began, in association with Charles Fisher, a psychoanalyst, his research on "dream deprivation", i.e. the determination of the effects of repeated interruptions of REM sleep (19). Freud's (6) theory of the dream as a safety valve, by which potentially disruptive drives are harmlessly expressed, had obvious predictions for the results of dream deprivation—it is dangerous to any system's integrity to cap its safety valve. The prevailing climate of high-visibility American psychiatry in the 1950s remained psychoanalytic. In view of psychoanalysts' long-standing claims to dreams and dreaming as a subject matter, it was not

surprising that a number of psychiatrists and psychologists trained in psychoanalysis soon were drawn toward REM dream research, some as participants, others as interested observers [numerous articles soon would appear on Freud's theory in the light of, or in relation to, the new research findings (e.g. 20-22)]. Even those not specifically schooled in psychoanalysis often would, *faute de mieux*, adopt psychoanalytic terminology and theory in interpreting their dream data. That Dement (19) had first reported deleterious psychological consequences of short-term REM deprivation, a finding that could not be replicated later (23), only enhanced the early interpenetration of psychoanalysis and laboratory dream research.

However, it would be incorrect to ascribe much ideological uniformity to early sleep and dream research. People from a variety of different disciplines and orientations often simply were attracted to the vast and untapped potential of the new sleep-recording technology. Some [e.g. Shapiro at the Downstate Medical Center in Brooklyn; Snyder at the National Institute of Mental Health (NIMH)] switched into the field in mid-career; others (e.g. John Antrobus at City College; Roffwarg at the New York State Psychiatric Institute) came into the field as relative neophytes and built their careers there. Some workers had underlying interests that were dominantly clinical (e.g. Whitman and Kramer at Cincinnati; Lewis of the Downstate group; Fiss of George Klein's group at NYU); some labs had underlying interests that were dominantly experimental [e.g. the two research labs remaining at the University of Chicago after Dement's departure and Kleitman's retirement: one, the short-lived (1957-1960) continuation of Kleitman's lab by Kamiya (with students such as Orlinsky and Stoyva); the other, the independently established and still productive lab of Allan Rechtschaffen (with students such as Monroe, Verdone, and Zepelin)].

On one dimension, however, there was a salient bond uniting most of the early sleep-dream community and differentiating it from subsequent times. Most researchers then were at least moderately interested in dreams, and most research groups had at least dabbled in dream research, even if only to the extent of seeing if they could independently corroborate the association of REM sleep with dreaming in the adult human. Thus, in the late 1950s and early 1960s, investigators later primarily identified with other research interests (e.g. Kales, Roffwarg, and Snyder) both studied dreams themselves and seemed to accept them as a relatively central concern.

One can even say that, at least in the United States, it was dream research that led to the subsequent explosion of neurophysiological and neurochemical research on REM sleep. In his reminiscences at the new

APSS (Association of Professional Sleep Societies) in 1995, Kleitman (24) stressed the central role that dreaming played in the very discovery of REM sleep. This early centrality of dreaming was not, however, destined to last. Dement's departure from New York to accept a position at Stanford was associated with his relative disengagement both from dream research and from construal of REM deprivation as dream deprivation. More generally, the "Psycho" portion of "Psychophysiological" in APSS's original title became progressively less salient by the late 1960s and early 1970s; the formal titular change (to Sleep Research Society in 1983) was but belated recognition of the earlier *de facto* change.

### THE NON-REM MENTATION CONTROVERSY

Part of this later relative devaluation of dreams and dreaming probably reflected the results of earlier dream research itself. From the outset of attempts to use or extend Dement's findings, it became apparent that dreaming could and did occur outside REM sleep (as, of course, had already been suggested by research in the 1930s and 1940s). One by one, the barriers defending the integrity of the REM = dreaming equation began to fall. No, the apparent non-REM recall could not be recall from an earlier REM period [it seemed to occur even before there had been any REM period (e.g. 25)]. No, non-REM recall was not a wholly different species of mental activity ("thoughtlike", for example) from dreaming. Although REM and non-REM reports were, on average, readily discriminable from one another (26), non-REM reports most often were "dreamlike" rather than thoughtlike (27), and there definitely was dreamlike mentation at sleep onset (28) in the absence of any REM sleep. Thus, non-REM dreaming is not, as is still widely misunderstood, dreaming by some sort of special definition; many non-REM reports are dreams by anyone's definition of dreaming.

Because the REM = dreaming equation had been of central importance in the development of the field, and because it had been the foundation of most of Dement's early assertions about the nature of dreaming, claims of the reality of dreaming outside REM sleep met much initial resistance, some of which continues to this day. But the earliest findings of non-REM dreaming were not the result of any deliberate attempt to refute Dement; rather, they simply arose, at levels too significant to ignore, in attempts to extend his REM-monitoring procedures to other problems. Thus, for example, the earliest counterevidence to REM = dreaming was observed by the Downstate group (29) in a study trying to determine whether self-

professed chronic nondreamers would have REM periods and, if so, if they would be able to recall dreams on REM awakenings. Their control group (of frequent extra-lab recallers) had 53% non-REM recall of dreamlike mentation. Foulkes (25), working in Kamiya's lab, wanted to find out how REM dreams "started", from an experiential point of view. But, as he pushed awakenings back into pre-REM epochs of non-REM sleep, he found no point at which dream recall ceased; thus, he gave up on trying to find out how and where REM dreaming began, concluding that dreaming might be more or less continuous through sleep. Kamiya (30) himself found substantial non-REM recall in an extensive parametric study of dream recall. Although Rechtschaffen's group (31) found considerably lower levels of non-REM recall than these other studies, their findings on the quality of non-REM reports dovetailed with those of Foulkes. Rechtschaffen's defense of the reality of non-REM dreaming, combined with his stature in the field (with Dement, he cofounded APSS and rapidly gained expertise in virtually all areas of sleep research), as much as the accumulating data, ultimately, if not easily, carried the day.

### DREAM PSYCHOPHYSIOLOGY RECONSIDERED

Where exactly did this outcome leave the status of Dement's earlier findings and claims? Had any substantial new discoveries actually been made about dreams and dreaming? As it turned out, the resolution of these questions that gained general consent left much to please all parties. As the insurgents had claimed, the Dement-Kleitman group had underestimated the extent, the nature, and the significance of non-REM mentation. The insurgents therefore were handed several new problems with which they could and would concern themselves for years to come: e.g. if REM sleep was not the invariable physiological substrate of dreaming, could one determine other physiological events or states that were associated with dreaming within both REM and non-REM sleep? In what ways were dream reports from the more traditionally defined REM and non-REM stages similar, and in what ways were they different from one another? But, it also generally was conceded that the first Chicago group had found a valid association between REM sleep and the generally most vivid/florid form of adult human dreamlife. Thus to the (considerable) extent that questions about dreaming applied to this form, then Dement's answers still held and still represented the same sort of revolutionary addition to human knowledge of dreaming as before.

Dream psychophysiology thus assumed several re-

lated forms during the 1960s and 1970s—its relative heyday. First, accepting that the most vivid and memorable dreams occurred during REM sleep, as classically [and, later, conventionally (32)] defined, researchers asked whether fluctuations in the quality of REM dream content were associated with fluctuations in either the defining [central nervous system (CNS)] or accompanying [autonomic nervous system (ANS)] physiology of the REM period in which the dream was dreamed. Characteristically, this line of inquiry had been initiated by Dement himself in his Chicago years (33), and the question that generated the most interest was whether the dreamer's eye movements signalled either visual imagery itself or changes in the dreamer's imagined visual regard within the dream. Dement suggested, and a collaborative study with Roffwarg (34) seemed to confirm, that there was a frequent (if not invariable) association between the incidence and direction of preawakening eye movements and the dreamer's reported visual regard in dream experience, as recollected upon experimental awakenings. Later investigators suggested that these findings were methodologically flawed and hence irreproducible (35) and that the stereotyped nature of REMs belied any point-to-point correspondence with dream content variation (36). Interest in the so-called "scanning hypothesis" continued into the 1980s (37), but, in the meantime, the issue of eye-movement/dream relations largely had been reframed in terms of phasic vs. tonic differences in REM mentation.

Aserinsky (38), back in the field after a relatively long hiatus, had suggested that Moruzzi's (39) distinction between tonic (long-lasting state properties) and phasic (brief, intermittent burst properties) REM sleep might have more general implications for REM mentation than simply reflecting the dreamer's visual regard. Because it had become increasingly apparent since 1962 that eye movements were only the most accessible signs of a more generalized intermittent activation pattern (including both CNS and ANS variables), the suggestion seemed to have merit, although Aserinsky himself despaired of its testability. Molinari collaborated with Foulkes (40) in a study of phasic (eye movement burst) vs. tonic (no eye movement burst) REM awakenings; in light of the study's goals, subjects were asked to describe only the very last imagery or thought they could recall having experienced before their awakening. In a frankly post hoc analysis, the authors searched for whatever phasic vs. tonic dream content differences they could find. Visualization per se did not discriminate the two conditions; whether that visualization was accompanied by reflective cognitive analysis (tonic) or not (phasic) did. This finding proved only weakly replicable, however (41).

Meanwhile, in the same light, studies were under-

taken to determine newly measurable aspects of phasic activation [e.g. PIPs (phasic integrated potentials) (42); MEMA (middle ear muscle activity) (43); filtered cortical theta EEG activity (44)] and the relation of phasic activation, so measured, to concomitant dream experience as reported upon immediately subsequent awakening. Under the strictest methodological control, observed results tended to be weak or unreplicable (for reviews see 45,46). Different investigators probably reached different conclusions from such research [the wrong physiological variables were being measured; subjects' retrospective reporting abilities were being too severely taxed; there in fact were no strong underlying psychophysiological relationships; sleep psychophysiology is lawful, but it follows rules that are difficult to understand because they cannot be predicted from waking psychophysiology (47)]. For most, however, for whatever reason, the bloom definitely was off the psychophysiological association rose by 1980 (cf. 48).

From the outset (40), one goal of the newer, post-scanning-hypothesis dream psychophysiology had been to find phasic events or analogues that crossed the older REM vs. non-REM barrier and that thus were capable of serving as substrates of dreaming across the more traditionally defined sleep stages or states. Although, unlike REMs themselves, such variables were found (e.g. PIPs, filtered theta), it soon became apparent not only that none had a strong relationship with either the incidence or nature of non-REM reports but also that none had the kind of distribution across states requisite to explaining the known pervasiveness of dreaming during sleep [not to mention the problems posed by the finding of dreamlike hallucination during relaxed wakefulness (49,50)].

The discovery of REM sleep and the suggestion that it uniquely was associated with dreaming had made Ramsey's (2) review of the mere physiological *accompaniments* of dreaming look rather naive. In the heady early days of REM research, it appeared to many that physiological events might be used to forge relatively immediate *explanations* of dreaming and that the state of sleep afforded a possibly unique window on mind-body relationships more generally. By 1980, it began to look like Ramsey's more modest evaluation of the promise of psychophysiological research might, in fact, be appropriate after all. One unfortunate side effect here was that sleep-laboratory dream research no longer would be carried along on any wave of neuroscientific grant funding.

### A THOUSAND FLOWERS BLOOM

In the early 1960s, the loss of that source of funding might not have spelled the catastrophe that it later did.

Thanks to the Soviet satellite, Sputnik, America's priorities had turned in the late 1950s toward basic scientific research, and a new-generation political leadership in the early 1960s, sensitive to science and the arts, had yet to spend itself, and the nation's resources, in the senseless war in distant Vietnam. Research funding was not quite the zero-sum game it later would become; just as we could, for a while, have both "guns and butter", America could have basic sleep research, clinical research, and dream research, too. In his introduction (51) to a preliminary draft of what was to become Luce and Segal's (17) popular review of the field, Philip Saper, then Chief of the Research Grants Branch of NIMH, noted that, in 1964–1965, NIMH had "supported over 60 projects related in whole or in part to studies of sleep and dreams, with awards totaling over \$2,000,000" (p. i). (Because other federal agencies also were supporting the area, even this figure understates the government's overall commitment.) Relatively speaking, 1964–1965 must have been close to, if not at, the all-time highpoint at which any nation ever has supported basic dream and dream-related research (bearing in mind the more intimate relation of sleep research and dream research at that time).

It is difficult to describe to those few researchers new to the basic sleep or dream areas today just how intertwined and intense the sleep–dream research community was in the early days of APSS. The organization of the field would have made, and did make (52), a suitable object for study in the sociology of a developing science. At APSS meetings, everyone tried to keep up with everything, no matter how seemingly remote it might be from their own workaday interests. As a group, dream researchers probably were more conversant with the brain stem and neurotransmitters then than at any time before or since. Meetings were consistently well attended, although, contrary to a recurring joke, this was not because Rechtschaffen kept attendance charts. The group, despite the gradually increasing disparity of its interests, both worked and played well together. In many ways, then, the early and mid-1960s were the golden days of the new research paradigm in dream psychology. Researchers charged out in various directions, all of them hitherto relatively untouched—so that whatever result they found, it was bound to be novel and informative.

The main directions pursued were those that seemed—from the prevailing ethos of American psychology—to be the obvious ones. In terms of Cronbach's (53) discrimination of two scientific psychologies, one experimental and the other correlational, researchers tended to begin programs in which either manipulations were performed to attempt to influence dream content or correlations of dream variables were

obtained, either across individuals or within individuals over time, with various waking variables.

Major laboratories participating in this research in the 1960s and early 1970s included those at CUNY (Antrobus, Arkin), Montefiore (Herman, Roffwarg, Ellman), Downstate (Shapiro, Witkin, Goodenough), Mt. Sinai Hospital (Fisher, Kahn), Maimonides (Ullman, Krippner), Boston VA (Greenberg, Hartmann), Cincinnati (Kramer), Chicago (Rechtschaffen, Vogel), Illinois at Chicago (Cartwright, Monroe), Virginia (Van de Castle, Hauri), Stanford and UC Davis (Tart), Texas (Cohen), Wyoming (Foulkes), and Oregon (Bregger). In addition, persons later to figure in the development of dream research gained experience in laboratories devoted more specifically to sleep (e.g. Lavie at Webb's laboratory in Florida and Kripke's in San Diego).

Characteristically, Dement (33) was first to attempt to manipulate REM dream content both with stimulation of the dreamer during REM sleep (with a tone, light, or water sprays) and with presleep manipulations (fluid deprivation). Work in both veins was undertaken by others beginning in the 1960s, with relatively more emphasis on presleep than in-sleep manipulations. This preference was not clearly dictated by any greater success in demonstrating presleep than in-sleep stimulus incorporation; unmistakable effects of either stimulus class generally were on the weak side. Rather, it reflected the prevailing psychodynamic view of the dream among most of those first attracted to the field: given affectively arousing stimuli such as presleep films of vacuum-extraction childbirth or primitive rites including penile incisions (54), for example, one could best observe how the dreamer's ego coped with and/or defended against the primitive fears evoked by such stimuli. With few exceptions (e.g. 55), less attention was given to an information-processing sort of analysis, in which the dream incorporation, as response, could be compared with the known stimulus to try to model the intervening cognitive processing. Probably the most general conclusion to be reached from the wide variety of disparate stimuli employed and analyses undertaken (reviewed in 56,57) is that dreams are relatively autonomous, or "isolated" (58), mental phenomena, in that they are not readily susceptible to either induction or modification by immediate presleep manipulations, at least those within the realm of possibility in ethical human experimentation.

Also symptomatic of the prevailing psychodynamic interests of the times, personality, rather than cognitive, variables were of more interest in early correlational research. There was much initial interest, for example, in the REM dreams of patients undergoing dynamic verbal therapy (59,60). At the interindividual level, projective tests or personality inventories yield-

ing potential clues to psychopathology were the instruments of choice among "normal" adult populations (61,62). The most general conclusion that could be drawn from studies correlating dream content variables with personality at either the trait or state level (as reviewed, again, in 56,57) seemed to support the idea of continuity, rather than complementarity, between the experiences of dreaming and of waking life. [This might, at least, have given some pause to disciples of Jung, who predicted just the opposite outcome (63)]. In terms of the very incidence of dreaming itself, it had already been apparent, of course, from the work of Dement (13,64) that dreaming (i.e. REM periods with generally retrievable content on experimental arousal) was a cyclically recurring normal phenomenon in all humans, rather than a specific response to stress in the dreamer's waking life (and this was already the subject of much comment and reformulation in the Freudian community).

One of the founding hopes of the sleep-and-dream research movement had been that its data would cast light on the origin and nature of psychiatric disorders. Dement (65), again, was first on the scene, with observations both of sleep cycles and dream reports in hospitalized schizophrenics. But from the 1950s on, the biological/pharmaceutical revolution in the treatment of major mental illness made the study of basic (untreated) mental disorders increasingly difficult to justify or effect, so the evolution of the REM-monitoring technology came along too late to have the sort of impact it might have had in Kraepelin's or Bleuler's day. Also, on both the sleep and dream fronts, early data were not promising: neither sleep patterns nor, in much less extensive study, dream content seemed particularly revealing in either schizophrenia or depression. Although sleep's role in depression ultimately came to be perceived as potentially significant (66), dream-content studies in major mental illness never really got much off the ground before they fizzled in the early 1970s. Later, anxiety dreams in Vietnam veterans suffering from post-traumatic stress syndrome (PTSS) refocused attention, at least briefly, on dream content (67). The researcher with the strongest and longest commitment to studying dreaming in relation to mental illness was Milton Kramer of the Cincinnati group (68).

Dement's "dream-deprivation" experiments had been conducted with an obvious eye toward the role of REM sleep, and of dreaming, in psychosis. This led to a fair amount of study of the effect of REM deprivation on REM dream content variables in recovery periods and on non-REM mentation. Results were somewhat consistent in the direction of increased short-term dreamlikeness of recovery REM mentation (69-71), but the effects turned out to be so qualified

and subtle as not to occasion much ultimate excitement. Also, in the intervening years, REM deprivation itself had been shown to play more of a curative than causative role in major mental illness (72). Likewise, sleep anomalies such as sleep walking (73,74) and night terrors (75) had been shown to have no clear link to either REM sleep or dream content. Overall, then, dreaming and dream content were not demonstrated to provide any "royal road" to mental illness, earlier theories and hopes to the contrary notwithstanding. An inadvertent side effect here was that dream research no longer could be justified as fundable on the basis of its unique clinical relevance. By the 1980s, its perceived value pretty much had shrunk to encompass only situationally induced stress (PTSS, divorce) (76).

Only surprisingly late in the expansion of electrophysiological dream research was much attention given to simple description of REM dream content. There had been, of course, in relation to psychophysiological questions, some emphasis on comparative description of mental activity reported after awakenings from different sleep stages. There also had been study of the relation of a night's dreams to one another (77,78). But little detailed attention was given to simply describing the normative characteristics of adult dreams until an influential paper by Snyder (79). He made explicit what most researchers had already implicitly noted in their various laboratory dream studies: dreaming is not as bizarre or crazy a process as people generally have assumed from the few (probably) REM dreams that they spontaneously remember or as therapists have assumed from samples of such dreams volunteered by their patients. Rather, representatively sampled dream experiences, in both content and form, have a texture not so vastly different from, or unfamiliar to, waking experience (80). The same conclusion was reached about children's dreams in a study whose impetus, characteristically, had been manipulation rather than description (81). In like fashion, most early researchers eagerly sought determinants or correlates of a process whose baseline properties had never been systematically ascertained. It is difficult not to believe both that such choices were largely unconsciously made and that they reflected the low value accorded "sheer" description by American science.

The normative properties of REM dreaming had not been missed, however, by Calvin Hall and his associates. Hall's earlier summaries and interpretations of the nature of human dreaming (3,82) had been based on spontaneously reported ("home") dreams rather than on laboratory-sampled ("lab") dreams. In 1966, along with Van de Castle (7), he published a book describing a comprehensive and reliable categorization scheme for enumerating dream content, including norms based

on his old "home" dreams. Because of Hall's enormous stake in an older methodology for collecting dreams, it is not surprising that conflict erupted between Hall and his associates, on the one hand, and some lab researchers, on the other hand [see, for example, the review of Hall and Van de Castle's book by Monroe (83) and the authors's reply (84)].

One of Hall's associates, Domhoff (subsequently to become a social critic and theorist of some note) had collaborated with Kamiya (85) in comparing home-remembered and -recorded dreams with laboratory-elicited REM dreams for subjects in Kamiya's extensive normative sample. Hall himself even set up a laboratory at his private Institute of Dream Research in Miami for a study of home dreams and of lab dreams collected under different schedules (86). Domhoff summarized the conclusions this group seemed to reach in his 1969 paper (87) subtitled "Home dreams are better", better in the sense that they are richer, more affectively revealing, and just plain more interesting than lab dreams.

Foulkes responded by comparing home and lab dreams with sampling method held constant (88,89), i.e. with morning-only recall at both sites. His results, especially with children, suggested that home dreams seem richer precisely because the home setting invariably allows selective recall of human dreamlife, recall biased in the direction of the emotional or unusual dream. He pointed out that researchers had entered the lab in the first place precisely because it offered a chance to sample REM dreams people ordinarily would sleep through, and forget, and, thus, because it allowed, for the first time, a chance to representatively sample human dreamlife. From this perspective, the differences observed in the uncontrolled designs were entirely expectable.

The issue of home dreams was to arise again in the 1980s (90), under the banner of the concept of "ecological validity" (91). At that point, home dream study gained approbation from members of the relatively recently established Association for the Study of Dreams (ASD), a society with membership open to anyone with an interest in dreams, researcher or not. Not surprisingly, therefore, most of the members of ASD also had commitments to spontaneously recalled dreams, and particularly to their own spontaneously recalled dreams. With lab research at that time becoming moribund because of lack of external funding, home research was once again, as before 1953, in relative ascendancy. In such a climate, renewal of justification for home dream sampling could hardly be surprising, despite the demonstrated unrepresentativeness of such sampling.

If one is committed simply to studying typical "remembered dreams", then home dreams *are* better.

However, if one is interested in studying typical human dream experience, what the mind does in sleep, then home sampling is inadequate. And if one is committed to understanding the mental processes that underlie human dreamlife, rather than those that merely facilitate its recall, then one requires the lab [or other techniques in which the dreamer determines neither when an awakening for dream retrieval will occur nor precisely how the dream will be described (92)]. We admire rainbows in natural settings, but we legitimately study lights and prisms in somewhat "artificial" settings when we have established that the natural phenomenon is not changed thereby.

Another dimension of difference separating Hall from many laboratory researchers was preference in dream-scoring methodology. Hall obviously was committed to his own scoring system, which involved nominal counting of instances in various content categories that were meant, in total, to be entirely comprehensive. The system was also used, at least selectively, by some lab researchers (93). Lab researchers in the second-stage Chicago tradition tended to prefer ordinal rating scales, designed not to catalog dream contents comprehensively but rather to focus on variables of particular interest in a particular context and to capture, in one score, the forest rather than the trees of each dream. Still more complex scoring systems applicable to dream analysis (94,95) or specifically meant for dream and dream-association analysis (96) soon were to evolve. At the 10th annual meeting of APSS in 1970, a committee was formed to collate and coordinate the various dream-scoring procedures then available, and this later resulted in an excellent published compendium (97).

When one compares not scoring systems but the ideology surrounding them, two differences between the 1960s and subsequent years become apparent. First, the notion that measurement problems were retarding the field's growth seems less tenable: reliable scoring procedures either existed or could be created fairly readily for most research goals envisioned by experimenters. If the field was, in fact, retarded, it more likely was by those goals themselves rather than by the means available to implement them. Second, the notion that uniformity of scoring, on the model of that for sleep "stages", was desirable waned as it became increasingly apparent that appropriate methods would need to vary with the nature of the problem and the particular interests of the investigator. Although some comparability and sharing across studies and labs clearly remained useful, it no longer seemed that one faced either/or choices in dream assessment.

### THE INTERNATIONAL DIMENSION

In the early years of sleep-and-dream research, investigation of underlying mechanisms of REM sleep

production in experimental animals was conducted largely in France and Italy (98,99), with American investigators going abroad for requisite training. Laboratory dream research, on the other hand, developed rather slowly outside the United States, and the prevailing movement of traffic was in the other direction. Fully realizing the risk of ethnocentric judgment, and inviting the reader to bear this in mind, I now will briefly describe the spread of sleep-laboratory dream research from America to other parts of the world and attempt to place it in the context of the largely North American research discussed above.

Among the earliest non-American dream studies were those Jouviet carried out at Lyon to satisfy himself about the REM-dreaming connection (100). Both Jouviet and Michel retained some interest in dreaming throughout their careers, Jouviet from a neurobiological reductionist viewpoint (see, e.g. 101) and Michel from a neuropsychological perspective (102). Lairy, in Paris, also became interested in dreaming, although she primarily was studying sleep patterns in relation to psychiatric disorder (103). Perhaps her primary contribution to dream research was training Piero Salzarulo, who carried laboratory research methodology back with him to northern Italy, where he later collaborated both with the Bosinelli group in Bologna and with Cipolli, with whom he conducted what probably was the first dream research explicitly motivated by the cognitive sciences (104).

After its brief initial flash, laboratory dream research in France never did achieve much momentum. In England, one could say it never was born. Some of the earliest research with enduring significance for the field, however, was produced in Scotland, in Ian Oswald's laboratory at Edinburgh. Along with collaborators, including Ralph Berger [whose stimulus incorporation study (55) was noted above], Oswald supported both experimental [e.g. drug effects on dream content (105)] and psychophysiological studies (106), as well as the first published lab study of the REM dream experiences of the blind (107). As was also the case with many "big name" sleep researchers in America, Oswald's role in dream research diminished after its opening flourish: his attention turned to biological and pharmacological issues. Berger's interests, after emigration to the United States, also turned largely elsewhere.

At Bologna, the Bosinelli group began publishing papers on important issues that had arisen in American dream psychophysiology (27,108), often with a twist reflecting Bosinelli and Molinari's continuing commitment to psychoanalysis (cf. 109). Extended visits by both to American labs (Foulkes, Kramer, Rechtschaffen) served to articulate ideological and methodological relationships with mainstream American research.



As will be seen below, the Bologna lab was a leader in the elaboration, in the 1980s, of a "cognitive" approach to dreaming, to which Cipolli had made earlier (and was to make continuing) contributions. Overall, the Bosinelli group had the most consistent record of significant programmatic research carried on outside North America. Bertini (110), in Rome, by way of time spent at Downstate, was the first to publish in Italy. He has continued in the field ever since, although he has pursued a somewhat more eclectic line of research than the other Italian investigators mentioned here.

Strauch came from Germany to New York in the early 1960s to learn dream-lab technology. Her sustained commitment to dream research, however, did not start until her assumption of a professorship in Zürich in the 1970s. In her collaborative work with B. Meier (111), the dominant focus was on dream phenomenology. A recent book (112), newly translated into English (113), summarizes research from these authors's laboratory studies. Zürich had already seen sleep-laboratory dream research by C. A. Meier (of the Research Center for Jungian Psychology) and D. Lehmann (in the Department of Neurology, University Hospitals). In Germany, Baust was, for a brief time, a leading figure in psychophysiology, with studies of autonomic variables in relation to dream content (114,115). Otherwise, there was no commanding dream presence in Central Europe or Scandinavia. Fried (116) briefly had a dream lab in Jyväskylä, Finland, during the 1980s, and the Piagetian scholar Montanero (117) was starting one in Geneva at the end of the same decade.

In Eastern Europe, little work explicitly directed to dream content or the process of dreaming seems to have been done, and that which was had little impact on the West, and vice-versa (118). In Israel (to which Rotenberg later would emigrate), there was interest from the 1970s on at Tel Aviv (Giora) and Bar-Ilan (Lewin), and Lavie established a major all purpose sleep laboratory in Haifa at Technion. Israeli research articulated much more closely with trends in North America and Europe but generally in more of a responsive than stimulative fashion. Bose (119) did an interesting dissertation in India relevant to the home-lab problem (see 9). So far as can be told from translation, Japanese work has been mainly derivative; certainly it has not had much impact outside its land of origin.

The hotbed of dream research, almost 40 years after its debut, is America's northern neighbor. For obscure reasons (because the development of Canadian research has generally been highly similar to that in America and because the economies of the two countries generally ebb and flow in tandem), Canadian re-

searchers have been much better supported than those in the United States. Certainly per capita, they have become the prime national force in dream research in recent years: Baylor and des Lauriers in Quebec; De Koninck, Moffitt, Ogilvie, and Pivik in Ontario; Koulack in Manitoba; and Kuiken in Alberta (with his former student, Nielsen, now also in Quebec). At the possible risk, however, of giving umbrage to northern sentiment, I am going to insist that the work of these researchers is, for all intents and purposes, indistinguishable from that of their *baja* peers (some are, after all, Americans, and others trained there).

### MALAISE?

Several factors probably contributed to declining productivity and enthusiasm in dream research in the 1970s. First, many of the more obvious extensions of Dement's findings to new problems or groups had, by then, already been attempted, and few new trails had been illuminated thereby. Second, the increasingly confused picture in dream psychophysiology was beginning to make it look as if the promise that physiological recordings might have explanatory, as opposed to merely methodological, value was not going to be fulfilled. Third, in the United States, still the center of the field, federal funding was severely curtailed as a result, among other things, of the decision that only guns, and not butter, could be afforded, after all. These curtailments affected not only research projects as such but also the ability to recruit new scholars to the field. Fourth, I think that there was a growing sense that continuation of empirical activity along the lines of the previous decade was going to prove worthwhile only if it generated some sort of coherent theory or viewpoint to guide it. Earlier, researchers had, by and large, relied on the generalized psychodynamic tradition whenever theory seemed to be required (with each having his or her particular variant thereof). The 1970s became, then, both for extrinsic (funding) and intrinsic reasons, a somewhat less active and somewhat more introspective decade than the 1960s.

At the 1973 APSS meeting, Foulkes (120) argued that less physiologizing and more psychology would be required to further advance the field. He suggested more use of associative data and an attempt to exploit what could be salvaged of Freud's methodology for identifying dream processes. Ultimately, however, extensive experience in association gathering convinced him of its inherent arbitrariness (a matter that had much earlier troubled Wittgenstein), and his attempt at cognitivizing Freud (96) soon yielded to an effort to simply replace psychodynamic theory with a purely cognitive perspective (8,9). Antrobus (121), the long-term dream researcher with probably the best creden-

tials in cognitive psychology (122), moved directly toward the same goal, without the psychoanalytic way station.

Much stir was created by Hobson and McCarley's proposed replacement of Freudian theory with their "neurobiological" theory (123). Essentially, reductionism, which had failed at the empirical level, now was being elevated to the theoretical level. As was pointed out by many critics (e.g. 124), the new theory, which stressed the bizarreness of dream experience and traced this property to phasic activity unique to stage REM, flew in the face of three empirical observations already made by dream researchers: 1) that representatively sampled dream experience (contrary to stereotype) was not highly bizarre; 2) that dreaming was by no means limited to REM sleep; and 3) that it was very difficult to establish reliable empirical relationships between phasic activation and any particular dream quality [Brylowski et al. (125) later were to find that wakinglike reflectiveness was enhanced by phasic activity, and Antrobus (126) was to argue convincingly that such activity merely added to general cortical/cognitive activation, rather than having any specific properties of its own, such as increasing bizarreness]. Hobson and McCarley themselves produced no new independent evidence on dream correlates of phasic activation. Nonetheless, the theory was energetically promoted (e.g. 127) and ultimately became the prevalent "dream" psychology of many introductory textbooks and in the popular press. By the 1990s, Hobson was plumping for the kind of "home" dream study that generated dream stereotypes in the first place (128,129). Thus, paradoxically, a "neurobiological" theory ended up rejecting both the methods and the main empirical findings of REM dream research.

The 1970s also saw a proliferation of popular books on dreaming that suggested some relaxation of the magic spell that neuroscience had once cast with the initial Aserinsky and Dement findings. America was quite familiar, and perhaps a little bored, with "REM sleep", and breathless accounts of "a night in the dream lab" were becoming passé. In keeping with the spirit of the times, the newer popularizations were not only wholly psychological but also in the tradition of self-help and self-absorption (130-132). They can be viewed as ideological precursors of the ASD movement in the 1980s, which served as a nucleus not only for people interested in dreaming from a nonresearch perspective but also for laboratory researchers looking for a more humanistic or "softer" perspective than had been provided by dream psychophysiology (and would be provided by cognitive psychology).

### NEW LIFE?

The 1980s gave signs of some reinvigoration of dream research, with the following developments: 1)

the coalescing of cognitive dream theory as a third-force alternative to both neuroscientific and psychodynamic approaches, with accompanying support in significant new empirical findings; 2) the documentation of sufficient similarity between REM and non-REM dream reports to suggest a common system of dream production across different psychophysiological states and to demolish the hope that dreaming could ever be explained simply through REM sleep mechanisms or data; 3) the arrival on the scene of lucid dreaming, with accompanying sleep lab demonstrations and investigations; 4) the use of holistic neuropsychology, rather than reductionistic physiology, as a source of dream research objectives; and 5) the flowering of research in Europe. Paradoxically, these hopeful signs were accompanied, by decade's end, by the near collapse of laboratory dream research in the United States. By 1990, the area, which originally had been almost a purely American enterprise, was almost entirely a non-American one.

Cognitive dream theory—or, better, a cognitive approach to dreaming—had impetus from both empirical data and theoretical considerations. Empirical observation of (rather than everyday assumptions about) waking and sleeping thought had shown them not to be greatly dissimilar (49,50,133), and the realistic quality and contents of REM dreaming certainly pointed in the direction of unified waking-sleeping mind theory. And, although reductionist approaches seemed to be faltering in dream psychology, the great success story of 20th century psychology at large was cognitive-psychological or, more broadly, cognitive-scientific (i.e. including linguistics, artificial intelligence, neuropsychology, etc.). When the dream research movement started, there had been relatively little human experimental psychology on which dream psychologists could rely. With the "cognitive revolution", that no longer was true, and for those specifically trained in psychology (e.g. Antrobus, Foulkes), rapprochement with their home base began to seem both promising and overdue.

As noted, empirical groundwork for this development had already been laid by Salzarulo and Cipolli's work (104) in Europe in the 1970s, initiated with the goal of applying Chomsky's linguistic theories to dream reporting. Cipolli was to continue to concern himself with cognitive problems such as the structure of dream narratives (134), a problem to which Kuiken and Nielsen (135) also made an important contribution. There also were precursors at the theoretical level. Certainly, Hall (3-5) was a major one. Although there was little or no cognitive psychology in the modern sense when Hall wrote, he clearly wanted academic psychology to begin taking dreaming seriously (136). However, unlike later cognitive psychology and

cognitive dream psychology, Hall's approach was content rather than process oriented. Another important forerunner was Breger (137), who, although still tied to a psychoanalytic framework, was perhaps the first person to combine laboratory research [on the effects of stress on dreaming (138)] with explicit use of contemporary information-processing models.

The essence of the process-oriented cognitive approach to dreaming lies in several related assumptions: that dreaming is the product of the same mental systems operative in waking life; that it is feasible, therefore, to attempt to model dreaming, using knowledge of waking mental systems, as a form of information processing; that in common with the higher symbolic systems studied by cognitive psychology, dreaming is not present simply because an organism is sentient—it, too, must have cognitive prerequisites and a developmental history; and that one of the best ways of isolating the cognitive prerequisites and components of dreaming is examination of the dream capability of persons with specific cognitive deficits caused by selective brain damage. With the advent of the cognitive approach, there was a clear shift of focus from dreams, the product, to dreaming, the process (and even the ASD's generally high quality professional journal, initiated in 1991, was titled *Dreaming*).

Cognitive dream models came in several variants. Foulkes's were originally (8,96) based in either linguistics or psycholinguistics but later (9) became essentially agnostic on the role of linguistic systems in dream production. His models were not highly formalized but rather were more in the nature of frameworks for conceptualizing dreaming. He saw it as the conscious interpretation of diffuse, but still organized, cognitive activation accompanying any state—waking or sleeping, REM or non-REM—with reduced sensory input and relaxation of voluntary ideational control (139). Antrobus's models (133), on the other hand, were more in the style of contemporary cognitive science, i.e. relatively formalized and in more intimate connection with particular cognitive-scientific models (140). By the end of the decade (141), he was not only doing computer simulation articulating with connectionist models (142) but also attempting greater integration with neuroscientific models of REM sleep production.

Empirically, strongest support for the cognitive approach probably came from longitudinal (143) and corroborative cross-sectional (144,145) studies of dreaming in early childhood. This research provided perhaps the first major insight since the Dement era into the fundamental nature of dreaming itself. The child data suggested that dreaming is not a primitive process that automatically accompanies waking perceptual processing across the life span or across spe-

cies, but rather that it is a symbolic process with strong cognitive prerequisites and with a developmental history much like that of waking symbolic thought [and waking consciousness (146,147)].

The strong implication of the apparent failure of young children to experience REM dreams and of somewhat older children to have dreams as formally accomplished as ours is that children's ability consciously to reflect their experience in mental imagery initially is absent and then only slowly develops to adult form. Neuropsychological evidence, reviewed below, suggests that waking and dreaming imagery operate in parallel, thus the ontogeny of dreaming may offer an unparalleled window on the development in the child of conscious mental states.

Permitting, indeed necessitating, the elaboration of dream models independent of particular psychophysiological states was the accumulating evidence not only of the reality of non-REM dreaming but also of its essential content and processing similarities with REM dreaming. With controls for length of dream report (i.e. for the undoubted fact that the system producing dreaming generally is quantitatively more active in REM than in non-REM sleep), Antrobus (148) found little difference between REM and non-REM reports, a finding confirmed with different methods by Foulkes and Schmidt (149). These findings suggest that there is one dream system operative, at generally different levels of activation, across states. The Bologna group, moreover, established that even reports from the most profound stage of non-REM sleep, Stage 4, were not greatly different from typical REM reports (150), a particular embarrassment for Hobson's (127) account of dreaming as a REM-driven process.

Green's (151) book and Tart's (152) reader had helped to reawaken interest in lucid dreaming—the awareness that one is dreaming and the potential for some voluntary direction of dream content—as also had Garfield's (132) do-it-yourself book on dream control. Independently, Hearne (153) in England and LaBerge (154) at Stanford demonstrated that selected subjects who became aware during dreaming that they were dreaming could signal that fact with, for example, eye movements during REM sleep. The surprising revelation from lab studies was that lucidity was associated not with microawakenings but with the continuation of REM sleep, even with its intensification (125). Meanwhile, Gackenbach (155) had done a (non-laboratory) dissertation on lucidity and, in 1981, issued the first edition of *Lucidity Letter*, which in turn spawned, in 1985, the Lucidity Association. In short, another movement was born (156). Like ASD, the Lucidity Association was generally, but not exclusively, composed of persons without laboratory research backgrounds or interests. Although clearly giving dream psy-

chology something to ponder (and explain) and placing outer limits on the nonreflective quality (58) of dreaming, lucidity remained, at decade's end, both exotic in appearance and somewhat problematic in terms of its implications for dreaming more generally.

Neuropsychologists examine human cognitive functions by means of the effects of selective brain lesions. Before the discovery of REM sleep, there had been various self reports of dream loss or loss of dream visualization with such lesions (157,158), but it took surprisingly long for laboratory sleep researchers to turn their attention toward such special populations [the blind, as noted earlier, and the deaf (159) had received some study]. In the early 1970s, hemisphere-specialization hypotheses in various left vs. right guises (intellectual vs. emotional, verbal vs. visual, logical vs. intuitive) were very much in the air as a result of popularizations such as those by Ornstein (160) and Galin (161). Based on such theorizing, Bakan (162,163) proposed a right-hemisphere origin of dreaming. However, a small-scale lab study of commissurotomy ("split brain") patients suggested that that could not be the case for dreaming *per se* (164). Work comparing activation patterns of right and left hemispheres of normal subjects did not suggest that dream visualization was more right- than left-hemisphere mediated (for a review see 165).

Perhaps the more revealing dream-psychological findings from studies of special populations were those unrelated to questions of hemispheric dominance or anatomical locus [e.g. demonstration by the cognitive psychologist, Kerr, that subjects with sight but with defects in waking mental imagery may not have visual dreams (166) and that the visual imagery of late-blind subjects is wholly constructive, in that they can visualize people they have never known visually (167)]. Farah (168), a cognitive scientist, made observations of dream phenomena in relation to Kosslyn's (169) model of waking mental imagery and to site of brain lesion (170), although not, like Kerr, with laboratory dream data. "Mental imagery" was shown to covary with dream visualization, establishing one avenue of direct empirical contact between cognitive science and dream research and also one justification for using dream reports to index states of waking consciousness (as in young children).

Finally, no longer was it the case that European dream research played any sort of secondary role vis à vis its North American counterpart. The European Sleep Research Society, founded by Koella in Basel in 1972, hosted biennial meetings in which dreaming invariably played a role. By the 1980s, both the quantity and the scientific quality of dream presentations there were unsurpassed. Cipolli's early and continuing contributions to the cognitive approach already have been

noted. Heynick (171), in the Netherlands, initiated a program of systematic investigation of speech in dreams, finding it generally both grammatically and pragmatically well formed. At Bosinelli's laboratory in Bologna, Cavallero and Cicogna were investigating immediate sources of dreams in terms of Tulving's (172) cognitive-psychological differentiation of different memory systems (173,174).

## EPILOGUE

At present, the future of laboratory-based dream research in America, although perhaps not in Canada or Europe, looks rather bleak. It has been years, for instance, since an American has been able to secure external funding for laboratory dream research, and effective use of laboratory methodology does not come cheaply. Increasing demands that science be "practical" put an intolerable burden on the study of dreaming, the very prototype of impracticality. New positions for dream researchers simply are not available. A graduate student in psychology at a major university recently told me that his mentor advised him to hide his interest in dreaming until after he had secured a tenured position (and even that may not be enough, as more than one prominent American dream researcher has discovered to his chagrin). A recent coalition of mainstream psychologists at Arizona did show some interest in considering dream-related topics (175) but, unfortunately, without any evident conviction of the importance of representative dream sampling via laboratory methodology (176).

Because cognitive psychology and cognitive science have largely been American inventions, the idea of any sort of effective integration with them looks rather bleak as well. Musing in 1982 over the prospects of the ultimate acceptance of dreaming by the American psychological mainstream (i.e. "waking" psychology), Hall (136) was profoundly pessimistic. Back in 1953, Ramsey (2) wrote: "The topic of dreaming is largely shunned by current research workers and is given only the briefest of treatment or entirely omitted in most contemporary psychology textbooks" (p. 432). After the REM revolution, have things really changed all that much? Besides brief mention in the now obligatory introductory textbook chapter on "states of consciousness", did electrophysiological dream research really make much difference? From the perspective of (then "general", now "cognitive") psychology, the answer clearly is no. Yet psychology itself is a shifting creation, where topics, paradigms, and methods go in and out of favor, so in the long run, who knows?

It does seem clear that the clinical and, barring some unexpected discovery of the scope of that made by Aserinsky and Kleitman (1), the neurobiological jus-

tifications for studying dreaming are no longer compelling and that further funding on these grounds is, therefore, unlikely. If dreaming is to continue to be studied, it will have to be studied for what it is—a distinctive human cognitive process—rather than for what it can tell us about personality or mind-body relationships. But this puts the study of dreaming in the uncomfortable position of depending on both intellectual and financial support from the cognitive sciences.

But the two disciplines have quite different histories and current textures. In the cognitive sciences, highly complex and formalized models of cognition are the goal, and empirical observations sometimes seem almost an afterthought. Laboratory dream psychology, on the other hand, has been a creaky, and a paradoxically highly empirical, sort of venture, still open to finding out what its subject matter is really all about.

It is interesting to consider the question, then, despite their current wide difference in status and support, which discipline really has learned more about human experience in the past 40 years? Rather than assuming, as cognitive disciplines increasingly do, that we already know most of what we need to know about such experience, and that the only significant question now is how best to model the underlying "computations" (artificially and/or neurally), dream psychology has made genuine, significant *empirical discoveries* about human experience, about a pervasive phenomenon of the human mind and one that always has been a prime object of human self reflection. Compared with someone with Ramsey's (2) line of sight, for instance, not to mention Freud's (6), we now know infinitely more than we did then about dreaming's occurrence, its properties, and its nature. We also see the prospect of further dream study giving us unparalleled insights into the origin and nature of human consciousness. In 40 years, empirical dream psychology, using methods owed to Aserinsky and Kleitman (1), has given us not only more reliable knowledge of dreaming than we ever had gained before but also the chance to parlay that knowledge into better understanding of the basis and nature of the kind of consciousness that makes us distinctively human. Let us hope that it will get to take that chance.

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