

## Is Biology Destiny After All?

### *Three Clinical Conundrums: Homosexuality, Alcoholism, and Obesity*

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*Recent announcements about the discovery of a biological/genetic basis for homosexuality compel psychodynamically oriented therapists to reassess once again their understanding of mind/body relations. By comparing the claims made in regard to homosexuality, where a biological basis is pronounced evidence that this orientation is natural, with the claims of alcoholism researchers, where biological differences are cited as evidence of an underlying disease, we begin to see how the metaphorical use of biology often determines the use to which research findings are put. Recent anti-diet approaches to obesity and binge eating further illustrate the limits of the dominant disease/addiction metaphors that have hitherto been used to treat these problems.*

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At a time when progress in the neurosciences and the long-promised deciphering of the human genetic code seem poised to reveal the underlying physical determinants of psychiatric disorders, if not of personality itself, psychoanalysts face the dilemma of how to correlate these new levels of explanation with our clinical observations and our personal experience.

Psychoanalysts have had a long, complex, and ambivalent relationship with the biological sciences. Freud's own conception of psychoanalysis as a science assumed an essential congruity between the mechanisms of neuronal activity in the brain—then conceptualized in terms of a tension-reduction model—and psychic functioning, in which modulations of psychic “energy” could be considered a literal—that is, nonmetaphorical—transformation of the physical, energetic discharges of the nervous system. Freud<sup>1</sup> in 1900 looked forward to the eventual bridging of the gap between what was known of neurology and his emergent psychoanalysis:

The mechanisms of these processes [cathexes] are quite unknown to me; anyone who wished to take these ideas seriously would have to look for physical analogies to them and find a means of picturing the movements that accompany excitation of neurons. (p. 599)

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Again, the “picturing” he envisioned was not metaphorical, but rather a means of establishing a one-to-one correspondence between psychological and neuronal events. The concept of the drive served as a bridge between the biological and the psychological, ideally to create a metapsychological mind-body holism. Almost since the time Freud first postulated this concept at the turn of the century, its inadequacy to the task has been the impetus for much of the controversy, and much of the evolution, in the field.

Heinz Kohut,<sup>2</sup> one of many recent analysts determined to maintain psychoanalysis as the *scientific* study of subjective experience, lamented that “the psychobiological framework of analytic theory had brought about severe distortions of our perception of man’s psychological essence without yet achieving a true integration of analysis with biology and medicine” (pp. 564–565). Here Kohut still seems to imply that one could legitimately imagine an eventual “true integration” of psychoanalysis with biology, presumably at some future date when the biology was more sophisticated. However, at other times he himself seemed to make a powerful argument for the fundamental incommensurability of the two realms, as when he defined the physical sciences as those “that are accessible via extrospection...” as opposed to “the sciences which explore the fields that are accessible via introspection (plus vicarious introspection, i.e., empathy, our ability to think ourselves into the inner life of another) . . . psychoanalysis par excellence” (p. 565).

Some philosophers of science, notably Grunbaum,<sup>3</sup> would have us reserve the name of science precisely for those fields that rely on extrospection, maintaining that the repeatability and falsifiability of experiments are essential to the very concept of “science.” A strictly logical positivist view of the functioning of science asserts that only empirically verifiable and falsifiable statements are meaningful. But this view of science itself has increasingly been rejected as being too rigid and nondescriptive of how science actually

works. Furthermore, as Hilary Putnam<sup>4</sup> reminds us, “‘scientific’ is not coextensive with ‘rational’” (p. 143). And Jan Zwicky,<sup>5</sup> in her provocatively titled *Lyric Philosophy*, offers in a collage-like format both an argument for and a demonstration of other meaningful ways of knowing, distinct from those of linear logic. Ironically, logical positivism’s own equation of the meaningful with the verifiable is itself ultimately unverifiable, and thus by its own criterion meaningless. All scientific explanations invariably rely on extrascientific values such as simplicity, coherence, and elegance in adjudicating the claims of rival theories, where pure predictability cannot decide the issue.

Science itself has been in thrall to the myth of a fact/value dichotomy (much as classical analysis was in thrall to the myth of blank-screen neutrality) as a way of asserting its absolute objectivity. But this stance serves only to obscure the values that are always there, operating behind the scenes. Kohut argued, for example, that Freud’s overriding value of truth-seeking, an apparently neutral scientific stance, was in fact at odds with his capacity to immerse himself empathically in the patient’s subjective experience.<sup>6</sup> Kohut nonetheless opted for affirming the possibility of scientific objectivity within a science of subjectivity, whereby the analyst-observer is willing and able to assess the effect of “the influence of the observed on the observer, and especially, the influence of the observer on the field that he observes”<sup>7</sup> (p. 40).

One wonders what is ultimately being warded off by this resolute insistence on the scientific status of psychoanalysis—as if the alternative were to admit to some unbearably foundationless belief system or practice. Yet as Zwicky<sup>5</sup> argues that philosophy itself need not fear incoherence in not being identified with logical analysis, so might there be hope for a “nonscientific” psychoanalysis. She has suggested that philosophy is “thinking in love with clarity” (p. 18)—including the clarity that comes from art and music. Perhaps there is a clarity to psychoanalytic investigation, as

well, that need not so desperately proclaim itself as “scientific.”

In the meantime, in the absence of a clear account of the relation (or lack of one) between psychology and biology, the information coming to us from the neurosciences and from genetics has, I believe, been incoherently assimilated into our psychological discourse. In particular, I want to address the conceptual dilemmas that have arisen from the discovery or supposition of biological bases or factors in three socially charged syndromes—homosexuality, alcoholism, and obesity—and how the introduction of scientific “evidence,” in conjunction with metaphors drawn from biology (sometimes masquerading as evidence), has muddled rather than clarified our thinking about these subjects. These muddles arise not so much from the incompleteness of our knowledge of the underlying biological issues, or from our lack of an integrated language of biology and psychology—which I hold is a metapsychological chimera—as from our failure to be clear about the role we still want biological foundations to play in our conceptualizations, and our unwillingness to face—in the name of being scientific—the role that our values play in our theorizing.

To quickly frame the kind of conundrum I think we are facing, let’s contrast the ways in which biological discoveries and biological metaphors have been used in our thinking about alcoholism and homosexuality. In an effort to assimilate the behavior of heavy or addictive drinking into the medical model of alcoholism as a disease, attempts were made to demonstrate physiological differences in the metabolism of alcohol by alcoholics and nonalcoholics (see, for example, Lindros [1978], Schuckit [1984], cited in Fingar-ette<sup>8</sup>). Even though the evidence was inconclusive, if not often contradictory, the essential conceptual program was clear: the pathological behavior is based on an underlying biological abnormality. The victim of a metabolic disease, the alcoholic must manage his or her life with the vigilance of the diabetic.

The location of the root of the problem in a biological flaw/disease is intended to 1) destigmatize the behavior and transfer our judgments from the realm of morality to that of clinical medicine and 2) invoke a range of treatment options (and not incidentally, reimbursements) in its management. Though I will have more to say about this definition of the “disease” later on, let’s now contrast how biological evidence has been invoked in our understanding of homosexuality.

Simon LeVay,<sup>9</sup> in a widely publicized article in *Science*, reported finding that a particular group of cells in the anterior hypothalamus is twice as large in a group of heterosexual men as in homosexual men, and concludes that “sexual orientation has a biological substrate” (p. 1034). Rather than offer this finding to demonstrate that a biological difference is the hallmark of homosexuality as a disease, it has been widely hailed as a demonstration that homosexuality is *natural*. Here destigmatization is achieved not by invoking the medical model, but by escaping from it.

Since Freud, psychoanalysis has vacillated in its emphasis on biologically predetermined drives versus a dynamic interpersonal process in accounting for disruptions of normal development. But ingrained in the psychoanalytic investigation—as much as in LeVay’s research paradigm—is the assumption that a discovery of *origins* is synonymous with a discovery of *meaning*. For Freud, this meant that the two processes of scientific investigation and of psychoanalysis could be assumed always to be congruent, rather than in conflict. This conception dates back at least to 1880 with Breuer’s treatment of “Anna O.,” when he was able to make her symptoms disappear by painstakingly tracing them back to the exact circumstances of their first appearance.<sup>10</sup> But as I showed in an earlier discussion of Freud’s (1920) treatment of a homosexual woman,<sup>11</sup> it was precisely his single-minded pursuit of the origins of her “symptoms” that blinded him to the functions her homosexual identity played in her life—

particularly relating to her ability to find idealizable role models—and led him into a traumatically unempathetic treatment approach that resulted in her abrupt termination of the analysis. Freud's attempt to put psychoanalysis on a scientific footing, and his overly narrow concept of science as equated with the investigation of origins and causes, left psychoanalysis in an awkwardly indeterminate position vis-à-vis the hard sciences—a conflict often referred to ironically as “physics envy.”

For our purposes, the difficulty posed by these contrasting uses of the biological suggests that in each case Biology, whether in the guise of Disease or Nature, is being invoked as a *deus ex machina* to resolve from the outside a dilemma we cannot resolve from within a purely psychological discourse. The destigmatizing role of biology arises, it seems, when unacknowledged value judgments have worked their way into our psychological discourse. We then seek to further “neutralize” our language by shifting the explanation to a plane of presumptively even more “objective” language. But the different uses of Disease and Nature show that what we are really doing is invoking a new, value-laden metaphor of our own choosing to advance an inevitably value-laden agenda under the banner of objective, value-free Science. All of this makes it nearly impossible to assess the validity of the scientific data in question—an assessment that may, in any case, be irrelevant to the metaphorical use to which the data are put. The pragmatic efficacy of the disease model of alcoholism within Alcoholics Anonymous (AA) has, for some, rendered irrelevant the argument that alcoholism cannot be meaningfully shown to be based on a disease of metabolism; however, the efficacy of the model has also politicized attempts to research alternatives.

Paradoxically, as we try to integrate new scientific discoveries into our descriptions, the power of the facts comes from the metaphors into which we incorporate them. What does it mean to say homosexuality is natural? LeVay, himself homosexual, evidently in-

tends his findings to portray homosexuality as a naturally occurring, normal variant of human development.

In *The Sexual Brain*, LeVay<sup>12</sup> attempts to put the findings from his original article into a broader context and into language accessible to the lay reader. He surveys the now considerable literature on sexual development, ranging from the chromosomal abnormalities of Klinefelter's syndrome (men born with an extra X chromosome) through laboratory research on the effects of hormones on the intrauterine development of rat genitalia and subsequent mating behavior. For instance, it has long been known that the male and female external genitalia develop from a common embryonic precursor. In the presence of testosterone, these fetal tissues develop into a penis and scrotum; in the absence of this hormone, into the clitoris and labia. More recently, it has been discovered that circulating testosterone affects brain development as well. The area of the hypothalamus known as the sexually dimorphic nucleus, because it normally develops to different sizes in men and women, depends on the level of testosterone to determine its growth. LeVay's 1991 paper<sup>9</sup> showed that a size variation also occurs between heterosexual and homosexual men in this region, paralleling the difference previously seen between men and women. His recent book is, in effect, a survey of explanations that might account for the difference and an attempt to make sense of it.

Prenatal testosterone has, in fact, been shown to affect not only physical development, but behavior later in life as well. LeVay<sup>12</sup> cites studies by Goy (1981) correlating the amount of “rough-and-tumble play” characteristic of male monkeys to prenatal levels of the hormone and by Berenbaum and Hines (1992) on its effects on the toy preferences of 2- to 4-year-old human children.

They found that most boys had a preference for toys like model trucks over dolls, while girls tended to play with trucks and

dolls almost equally. Girls who had been exposed to unusually high levels of androgens prenatally, as a result of congenital [adrenal] hyperplasia, showed the toy preference typical of boys. (p. 86)

Normal prenatal testosterone has likewise been shown to be necessary, LeVay notes, for the adult sexual development of male rats:

Furthermore if these rats, as adults, are given estrogen and progesterone, mimicking the hormonal milieu of adult female rats, they will develop the lordosis [female mounting behavior] when paired with a stud male. (p. 87)

These types of data from laboratory animal studies and the study of human pathology clearly seem to link physical and behavioral sexual development to genetic and hormonal factors. But what does this mean for homosexuality? LeVay, on the one hand, urges caution in interpreting his own data.

To many people finding a difference in brain structures between gay and straight men is equivalent to proving gay men are "born that way." Time and again I have been described as someone who "proved that homosexuality is genetic" or some such thing. I did not. . . . It is not possible, purely on the basis of my observations, to say whether the structural differences were present at birth, and later influenced the men to become gay or straight, or whether they arose in adult life, perhaps as a result of the men's sexual behavior. (p. 122)

But in his presentation of the animal research, LeVay clearly indicates he believes the likelihood is that these differences prenatally organize subsequent development. At the same time, he just as clearly does not want a smaller hypothalamus to be thought of as equivalent to the missing X chromosome of Turner's syndrome. Why not?

To his credit, LeVay is quite forthright

about this dilemma and his own point of view. Reviewing a study showing that maternal stress in lab rats results in feminized sexual behavior in male offspring, and Dorner's (1980, 1983) hypothesis of a parallel etiology for human homosexuality, LeVay writes, "I may be biased away from believing something as cool as homosexuality could be caused by something as uncool as stress" (p. 126). Could we imagine our saying the same thing about Turner's syndrome, which includes short stature, scant pubic hair, stocky chest, and amenorrhea? If not, is it that the label "syndrome" records our foregone conclusion that that particular cluster of characteristics is "uncool"? To put this another way, our distinction between a "natural" or normal variation and a "syndrome" is inevitably going to be based on some value-laden, functional criterion from everyday life, not on what shows up under the microscope. LeVay maintains,

I do not know—nor does anyone else—what makes a person gay, bisexual or straight. I do believe that the answer to this question will eventually be found by doing biological research in laboratories and not by simply talking about the topic. . . . (p. 108)

But as should be clear by now, I am maintaining that how we talk about it turns out to be the far more difficult and important issue, and one that no research result can hope to simplify. As Wittgenstein<sup>13</sup> said, "The essence is expressed by grammar" (p. 116); "grammar" here means the role the concept is permitted to play in our form of life. For LeVay, talking about homosexuality as a disease "implies curability, and untold thousands of gay men have been subjected to psychoanalysis, hormone treatments, electric shock therapy, and brain surgery in attempts to 'cure' them of their unfortunate condition" (p. 110). But LeVay's own findings would be perfectly amenable to a disease model if we wished to employ one. What is

really crucial here is whether we think a homosexual's "condition" is "unfortunate" or not, and that is only going to be determined by how we talk about it, by whether we see it as having a value and a place in our collective lives.

What if further research uncovers a *preventable* hormonal or genetic variation that gives rise to dimorphic hypothalamic structure and adult sexual orientation? Would we then be justified in shifting *back* to a disease model? Or would preventing (or aborting) predisposed homosexual fetuses be morally equivalent to the actions of those Chinese families who use ultrasound scans to determine fetal gender and then abort the female (as reported in *The New York Times*, July 21, 1993)? The value that we place on homosexuality or gender seems to me a question that, inescapably, we will determine in our everyday life and language; and although biological metaphors can be enlisted in our arguments, the moral dilemmas cannot be solved by more research. When we are uncomfortable admitting that the issue is one of cultural values, we try to escape into objectivity.

The same intrusion of political and moral values is evident when we examine attitudes toward alcoholism as compared with addiction to cocaine and opiates. Whereas the usual disease model of alcoholism posits a core genetic vulnerability affecting only alcoholics, compared with "normal" people, who are able to drink socially, in the case of narcotics a model of universal vulnerability is presumed, such that anyone exposed will become addicted. This then justifies a kind of Prohibition against opiates that is now generally agreed to have been counterproductive when applied to alcohol. But the data emerging from the treatment of multiple substance abuse may support Berger's<sup>14</sup> contention that a range of characterological factors (best understood and treated psychotherapeutically), rather than genetic susceptibilities, might well be the appropriate focus of attempts to understand drug use and to dis-

tinguish those who use drugs recreationally from those whose lives become organized around their use or addiction. It is ironic that the seemingly more "scientific" disease model of addiction and alcoholism has led to such an increasingly one-dimensional picture of the victim. Any complexity of personality, any deficit or conflict, is ruled secondary to the acknowledgment of the disease; and one treatment, abstinence, is deemed to fit all.

The pendulum seems to have made similar swings regarding homosexuality. Within the clinical community, the depathologizing of homosexual behavior, *per se*, began with the recognition that homosexuality may be part of the personality at many levels of structural organization<sup>15</sup> and pathognomonic of no one diagnostic picture, if indeed of any. The recent attempt to destigmatize homosexuality as "natural" threatens to once again eliminate distinctions and posit a politically correct picture of a unitary homosexual development.

Similarly, the disease model/metaphor for alcoholism opens up a set of treatment options, but at the price of a conceptually rigid and one-dimensional model of the "disease." In his book *Heavy Drinking*, Fingarette<sup>8</sup> rejects the disease model entirely and claims that "alcoholism" is, in fact, a multifaceted psychologically and culturally determined *group* of phenomena: it has no single underlying cause and no single course of illness or outcome and, despite the claims of AA, is not a disease whose only treatment is abstinence. He writes:

Instead of looking at heavy drinkers as victims of some wayward gene or physical abnormality, we can now see them in a truer light: as a diverse group of people who for diverse reasons are caught up in a particularly destructive way of life.  
(p. 66)

Paradoxically, the medicalization of the syndrome has been accompanied by the demedicalization of the treatment in favor of

nonprofessional peer group support. Despite the efforts of some therapists to integrate AA tenets into dynamic exploration, for many alcoholics the disease model has stigmatized a psychodynamic treatment of their medical problem.

The destigmatization of alcoholism as a disease has not, however, eliminated all of the value judgments and moral dilemmas that result from the accompanying destructive behavior. Often a new moral judgment seems threatened against family and others as “enablers.” But the moral status of the alcoholic remains unclear. If an alcoholic is powerless over his or her disease until achieving AA-supported abstinence, and denial is one more “symptom” of the disease, where does responsibility for the behavior lie? Are we so beyond value judgments that we don’t want to consider such moralizing questions? Then why isn’t there an insanity defense for drunken driving?

The recognition of the problem of responsibility in the face of underlying determinism is very old. Zeno (333–261 B.C.) was once chastising a slave for stealing, and when the latter pleaded that it was his fate to steal, “Yes, and to be beaten too,” said Zeno (p. 135).<sup>16</sup>

A disease model for alcoholism perhaps suggests an analogy with diabetes, where the patient, although not responsible for having the disease, is still presumed to have responsibility for understanding its consequences and modifying his or her behavior accordingly. That is essentially what the Catholic church has tried to tell homosexuals: given the innate propensity to this sin, it is your responsibility not to act on it. A disease metaphor can allow us to sidestep moral questions only for so long, not banish them entirely.

The disease model of alcoholism is used to prescribe a constant vigilance and abstinence from all drinking in the face of an incurable underlying metabolic cause. Obesity seems to many to be another metabolic disorder with attendant compulsive be-

haviors. Indeed, Overeaters Anonymous attempts to transfer the alcoholism model directly to its treatment, even though “abstinence” from food can only take the form either of caloric control or abstinence from specific forbidden foods. However, both the addiction model and the attendant assumption that obesity is by definition a disease, rather than a normal variation from a culturally determined stereotype of ideal thinness, have provoked a feminist backlash, sometimes called the “anti-diet movement.” Although I won’t attempt to review that literature here (see, for instance, S. Orbach, “Fat Is a Feminist Issue”<sup>17</sup>), I will focus on the work of two therapists, Jane Hirschmann and Carol Munter, who have proposed addressing the problem with the aid of a very different biological metaphor and have come to radically different treatment conclusions about a seemingly similar compulsive behavior.

Hirschmann and Munter<sup>18</sup> see overeating as a self-soothing behavior with both unconscious and biological determinants, but their conclusion is not that obesity is a disease requiring constant monitoring; rather, invoking the biological metaphor of homeostasis and an analogy with the demand feeding of babies, they conclude instead that the pathologizing of the syndrome and the vigilance associated with diet “cures” iatrogenically worsen the problem. Their approach rests on three core assumptions that overturn the implications of “addiction” and “disease” models: 1) the mounting evidence is that diets simply don’t work: “90% regain their weight plus some more”; 2) “diets make you fat”: drastically reducing calorie intake can cause the body’s metabolism to slow down and store fat; and 3) “deprivation insures a fight-back response—the binge” (p. 82).

Although the medical consequences of obesity are comparable to those of alcoholism, Hirschmann and Munter show how the disease model can be avoided. Indeed, they suggest that the hypervigilant monitoring of one’s diet and the AA-like identification of oneself as an addicted overeater perpetuate

a vicious circle of self-loathing, restrictive diets doomed to failure, rebound binges, and further self-recrimination that leads to the pursuit of self-soothing via food. Taking the metaphor of “demand feeding” from Dr. Spock, they attempt to do away with a pathologizing of obesity and avoid any particular weight loss goal, allowing the body to “homeostatically” realign itself to an individual norm, even if it is one that is socially proscribed as “fat.” What is eliminated is not the fat, but the disease model of being fat, along with our culture’s equating of thinness with beauty and health.

If a woman who eats noncompulsively nevertheless has a body weight we see as “fat,” is she the victim of an innate metabolic disturbance or a cultural stereotype? In fact there may be considerable divergence between groups that are described as obese and those who are compulsive overeaters. Often no differences can be found in the eating habits of obese and thin people,<sup>19</sup> but culturally we are apparently reluctant to offer fat people the comfort of seeing their condition as “natural.” Anything that is that deviant from the cultural ideal will not easily be seen as natural, and there is as well an enormous investment being made by diet, food, and therapy industries in maintaining that ideal. But attempts to control weight by dieting may do nothing but disrupt what had been normal eating habits in favor of a compulsive regimen that only leads to rebound behaviors.

The difference in metaphor opens up a correspondingly different set of treatment options. For instance, in Hirschmann and Munter’s model overeaters are encouraged to stock the kitchen with large supplies of their previously forbidden favorite bingeing foods and to eat them to satiety, thereby eliminating any notion that they will be deprived in the future. It is important to note that the merits of this approach will need to be evaluated pragmatically by their outcome and that they would not necessarily be jeopardized by any new infant research that called into question the wisdom of “demand feeding” babies.

All that would change is the demand-feeding metaphor’s power to encourage us to adopt this particular approach.

Interestingly, Hirschmann and Munter’s model encourages their clients to return to the eating equivalent of “social drinking” that has been anathematized by AA. But Fingarette<sup>8</sup> cited studies (Cohen et al., 1971; Bigelow and Liebson, 1972) showing that when hospitalized alcoholics were allowed to “earn” as much alcohol as they wanted via various behavioral chores, most learned to limit themselves to moderate drinking. Within the AA model, however, moderation implies an ongoing degree of self-control that undermines the controlling metaphor of a disease-induced helplessness. Within the disease model of alcoholism, rebound bingeing, cravings, and “slipping” are all part of the natural history of the illness. Hirschmann and Munter’s work with overeaters tantalizes us with the possibility that many of these phenomena may be avoidable, iatrogenically induced artifacts of a treatment approach focused on hypervigilance and abstinence.

Fingarette attempted to relabel alcoholism “heavy drinking” precisely to emphasize many of the factors that Hirschmann and Munter ascribe to overeating. We might integrate their approaches to conclude that

1. There is no single entity that can be defined as alcoholism (or overeating).
2. Rebound and binge behaviors may be as much a function of the treatment as the “disease.”
3. There is no clear dichotomy, biological or characterological, between alcoholics (or the obese) and “normals.”
4. Neither heavy drinking nor overeating is a biologically predetermined behavior that precludes a return to normal social behavior.
5. Both heavy drinking and overeating are culturally available habits of self-soothing that can function as meaningful or destructive elements of individuals’ “struggle to live their lives.”<sup>8</sup> (p. 103)

Without attempting to adjudicate the claims made by these authors, we can nonetheless be impressed by how they have managed to break free of the dominant cultural and therapeutic metaphors governing talk about overeating and drinking and establish new paradigms for what happens both in the behavior and its treatment.

As biological foundationalism becomes more and more an unexamined part of our language and philosophy, attempts to medicalize moral dilemmas become commonplace, and the roles that we ask biological research and biological metaphors to play in our lives become blurred. These metaphors become part of what analysts Stolorow and Atwood<sup>20</sup> have termed the "prereflexive unconscious," which consists of the "organizing principles of a person's subjective world" (p. 29). We each have our own set of organiz-

ing principles unique to our personal life experience, but we also tacitly join them to the dominant cultural myths of our time in making sense of our broader experience. Although not the product of any defensive activity (and thus not considered essential to the dynamics of classical analysis), these cultural myths may be just as difficult to articulate or bring to consciousness as conflictual material, simply because they are part of the unspoken background of all our thinking.

The dilemmas raised by the behaviors of homosexuality, alcoholism, and compulsive eating are clinical, moral, and philosophical. To the extent that we allow ourselves to perceive these dilemmas exclusively through a narrow lens of science, we will deceive ourselves into thinking that what we lack is information, when what is really missing is clarity.

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