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### Failed Explanations and Criminal Responsibility: Experts and the Unconscious

Stephen J. Morse

*University of Pennsylvania Carey Law School*

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# FAILED EXPLANATIONS AND CRIMINAL RESPONSIBILITY: EXPERTS AND THE UNCONSCIOUS\*

Stephen J. Morse\*\*

"There is no such thing as a bad boy."

Fr. Edward Joseph Flanagan

"Tout comprendre c'est tout pardonner."

French Proverb

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This paper was first presented to a faculty workshop at the Law Center. The paper was also presented at the Annual Meeting of the Massachusetts Psychological Association held in Boston in May 1981, and at a faculty workshop at the College of Law, University of Iowa, in March 1982.

Jack Zusman, Larry Simon, Alan Schwartz, Chris Stone, Charles Whitebread, John Monahan, Frederick Crews, Robert Steele, and Elliot Silverstein deserve grateful thanks for their gracious assistance. Adolph Grünbaum thoughtfully provided preprints of his seminal work on the topics discussed herein. Jan Harris and Lynn Skordal furnished able research assistance. Finally, Michael Moore deserves special mention for teaching me so much and for unflinchingly reading and commenting on my work.

In modified form, sections of this paper will appear in my forthcoming book, *The Jurisprudence of Crazyness*, to be published by the Oxford University Press.

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I.	INTRODUCTION: A PROJECT FOR SCIENTIFIC MENTAL HEALTH EXPERTISE	

THE causes of human conduct, including aberrant behavior, have forever been a source of interest, wonder, and puzzlement. At various times, Fate, humors, incubi, succubi, the gods, the devil, genetics, parents, unconscious conflicts and structures, the will, social structure, brain anatomy and physiology, contingencies of reinforcement, and combinations of the above have been advanced as explanatory factors.<sup>1</sup> In the past two centuries, under

<sup>1</sup> For standard histories of psychiatry, see generally F. Alexander & T. Selesnick, *The History of Psychiatry: An Evaluation of Psychiatric Thought and Practice from Prehistoric*

the influence of a scientific and medical *Weltanschauung* and sympathy for criminal defendants, Anglo-American criminal law has increasingly relied on the modern disciplines of behavior for explanations, especially psychiatry, psychology, and psychoanalysis. Behavioral sciences and scientists have promised to help rationalize ascriptions of criminal responsibility and the apportionment of punishment. But have they fulfilled their promises?

In *Crazy Behavior, Morals and Science: An Analysis of Mental Health Law*, a recent article that addressed mental health law generally,<sup>2</sup> I attempted to clarify the usefulness of behavioral science and scientists in understanding and deciding legal cases involving mental health issues. I argued that the degree of present acceptance or reliance on mental health experts is unwise, unjustified, and unnecessary, because such reliance often confuses social questions with scientific issues.<sup>3</sup> *Crazy Behavior* also claimed that, at least for legal purposes, there is much less mental health science than is commonly supposed, and that lay persons are generally but not exclusively capable of providing the type of information necessary for reasoned decisionmaking in mental health cases.<sup>4</sup>

I also argued in *Crazy Behavior* that the general supposition that the mentally disordered as a class are substantially less capable than "normal" persons of behaving rationally and controlling their behavior is an empirically unwarranted assumption. Consequently, it is difficult to support, on other than intuitive grounds, laws that treat the mentally disordered differently from normal persons, thereby depriving them of liberty and dignity.<sup>5</sup> I therefore suggested reforms of a set of typical mental health laws that would treat the mentally disordered properly, or at least clarify the essentially moral, social, and political nature of laws that did treat the disordered specially.<sup>6</sup> The legal system was enjoined to take re-

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Times to the Present (1966); G. Zilboorg, *A History of Medical Psychology* (1941). An excellent traditional history of psychology is E. Boring, *A History of Experimental Psychology* (1949). A complete review of various theories may be found in *Comprehensive Textbook of Psychiatry-III* (H. Kaplan, A. Freedman & B. Sadock 3d ed. 1980) [hereinafter cited as CTP-III].

<sup>2</sup> Morse, *Crazy Behavior, Morals, and Science: An Analysis of Mental Health Law*, 51 S. Cal. L. Rev. 527 (1978).

<sup>3</sup> Id. at 543-54 (what is mental disorder?).

<sup>4</sup> Id. at 554-60 (who is crazy?); 600-22 (the relevance of experts).

<sup>5</sup> Id. at 564-90.

<sup>6</sup> Id. at 626-54.

sponsibility for deciding as a *legal matter* the difficult issues presented by crazy behavior.<sup>7</sup>

More recently, Richard Bonnie and Christopher Slobogin, both of the University of Virginia School of Law, have argued in a thought-provoking and important article<sup>8</sup> that it is morally and socially proper for the criminal law to place great weight on the defendant's subjective psychological state for purposes of ascribing responsibility and imposing punishment. Bonnie and Slobogin claim that the law must accept the necessary imprecision that accompanies the investigation of subjective mental states when criminal punishment and stigma are at issue.<sup>9</sup> They also suggest that the law should not narrow the contributions of mental health experts, as I advised in *Crazy Behavior*, but should expand the influence of these experts as an integral part of criminal law adjudication. Bonnie and Slobogin especially advocate the use of psychodynamic psychological<sup>10</sup> explanations of criminal behavior.<sup>11</sup> Finally, they argue that the proper response to the admitted failures of mental health expertise in the criminal process is not to limit the role of the experts; rather, it is to train the experts to do the job right.<sup>12</sup> To state and support their claims on behalf of mental health experts, Bonnie and Slobogin use my views as a counterpoint to their own.

The debate involves both the means by which our society and the criminal justice system explain criminal behavior and ascribe responsibility, and the means by which we can make criminal justice decisionmaking most rational. Mental health experts at present provide information about incompetence to stand trial, diminished capacity, criminal insanity, sentencing, parole, and other

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<sup>7</sup> Id. at 600-04, 626. The word crazy is used for the sake of accuracy and to avoid question-begging labels. The author does not intend any disrespect toward disordered persons or mental health professionals. See id. at 529 n.1.

<sup>8</sup> Bonnie & Slobogin, *The Role of Mental Health Professionals in the Criminal Process: The Case for Informed Speculation*, 66 Va. L. Rev. 427 (1980).

<sup>9</sup> Id. at 431-52.

<sup>10</sup> Psychodynamic formulations and reconstructions refer to explanations of behavior based on the principles of psychodynamic psychology. This article uses the term psychodynamic psychology to refer to all modern psychological theories, beginning with Freudian psychoanalytic theory, that posit unconscious variables as the primary determinants of human behavior. See *infra* notes 40-190 and accompanying text for a full discussion of psychodynamic psychology.

<sup>11</sup> Bonnie & Slobogin, *supra* note 8, at 492-95 (summary).

<sup>12</sup> Id. at 496-522.

issues. They are the foremost experts used by defense counsel to support claims for mercy or compassion. In the narrowest legal terms, the debate is about the proper limits on mental health expert testimony in criminal trials. But the deeper issue involves fundamental conceptions of human behavior, the responsibility for actions, and the proper purposes and scope of punishment.

This article will first explore the importance of assessing subjective mental states in criminal law and the role of experts in assisting such assessments. I agree with Bonnie and Slobogin<sup>18</sup> that subjective mens rea requirements and general culpability are necessary preconditions of just criminal punishment. I shall argue, however, that the duty to be fair to criminal defendants does not justify the admission of expert testimony without regard to the reliability and validity of the data and opinions, or the needs of the factfinder for assistance.

The second section, the centerpiece of the article, considers the role of psychodynamic explanations in the criminal process by advancing four arguments. First, psychodynamic theory does not provide scientifically validated causal accounts for behavior; rather, it is better understood as a literary-interpretive account of the *meaning* of behavior. Second, in individual cases, no means exist at present to construct a reliable and valid dynamic formulation of the behavior of the defendant. Third, even if dynamic theory were sufficiently valid, and if mental health experts could formulate reliable and valid explanations of individual cases using the theory, unconscious motivation should not affect assessments of responsibility. Finally, the section argues that a psychodynamic approach to criminal cases yields no sensible, practical approach to dispositional questions.

The third section of the article shows that the scientific foundation for broad reliance on expertise is no stronger now than it was when I first proposed in *Crazy Behavior* to narrow the role of experts, and that training programs are an inapposite (and utopian) remedy for the defects in expert testimony. The fourth section analyzes intensively a representative sample of the cases that Bonnie and Slobogin offer as proof that their proposals will lead to the production of useful evidence in criminal law decisionmaking. Their analysis of the cases is parsed and compared with the ap-

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<sup>18</sup> Id. at 446.

proach set forth in *Crazy Behavior* and the previous sections of this article. The section claims that my approach will produce decisionmaking that is more efficient, scientific, and *fair* than the Bonnie and Slobogin system.

The article concludes by suggesting that the medicalization of behavior, exemplified by psychodynamic explanations, has eroded sensible, traditional notions of responsibility by compromising the belief that most persons are capable of rational behavior and reasonable self-control. To repair the damage, I call once again for a return to primary reliance on common sense in thinking about criminal law and adjudicating criminal cases.

## II. SUBJECTIVITY, RESPONSIBILITY, AND EVIDENCE: FORMULATIONS ON THE TWO THEORIES OF EXPERT TESTIMONY

A criminal conviction is a matter of great consequence, leading, as it almost always does, to some loss of liberty and to stigmatization. Substantive and procedural rules for adjudicating criminal liability must strike a just balance between societal and individual concerns. The law must determine the necessary preconditions for ascribing responsibility to and punishing a criminal defendant, and it must decide how much latitude the defendant must be permitted in his or her efforts to defeat allegations of guilt or to argue for mitigation of punishment after conviction.

In addition to an offensive act, the criminal law traditionally has had two basic criteria for responsibility and punishment: first, the actor must have been capable of reasonably rational, self-directed control over his or her behavior at the time of the offensive act; and second, the actor must have had the subjective state of mind, the *mens rea*, that was required for liability by the definition of the offense charged.<sup>14</sup> Although we agree on the wisdom of these

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<sup>14</sup> The policy behind the first criterion is straightforward. In a criminal justice system concerned with just punishment rather than with simply the efficient maintenance of order, it is unfair to punish persons for acts that are beyond their rational control. Small children and beasts are not considered responsible and are not *punished* for this reason (although they may be incapacitated in some fashion to protect themselves or others from the consequences of their ill-controlled behavior). The policies underlying the *mens rea* criterion are closely allied to those supporting the necessity of rationality and reasonable control. Just assessments of blame must consider the actor's state of mind when he or she performed the offensive act. Generally, for example, a person who acts intentionally is considered more blameworthy than a person who produces the same consequence negligently. For purposes

requirements, what divides observers of criminal justice such as Bonnie and Slobogin and myself is, first, the extent to which we believe that psychological factors or theories bear on, vitiates, or negate self-control, and, second, the best method for gathering evidence about control capacity and mens rea. I argue that the law should require a high degree of self-control from all persons and that psychological factors rarely so undermine the capacity for reasonable self-control and the ability to form mental states that the law should diminish the legal responsibility of the actor.<sup>15</sup> This argument is based on the belief that the data do not demonstrate that most people, including most disordered persons, are incapable of forming mens rea or acting with reasonable rationality or self-control. Bonnie and Slobogin believe, by contrast, that large numbers of offenders are either not fully responsible for their behavior or are not responsible at all. Let us call such persons "responsibility skeptics." They appear to believe that a very substantial percentage of defendants lack reasonable degrees of autonomy and dignity. As this article will demonstrate, the responsibility skeptics usually reach this view on the basis of an uncritical assessment of the scientific evidence or on the basis of a naive determinism.

My second point of departure from Bonnie and Slobogin is the usefulness of much mental health testimony. Bonnie and Slobogin claim that exclusion of such evidence is unfair to defendants because the experts have much to contribute to subjective assessments, and they label the critics of this testimony "method skeptics."<sup>16</sup> I shall try to demonstrate, however, that, unlike the "method skeptics," Bonnie and Slobogin make no systematic attempt either to analyze the questions allegedly calling for mental health expertise or to consider the reliability and validity of the scientific methods and data supporting the expertise they endorse.

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of blaming and punishing, the criminal law recognizes the moral distinctions between states of mind. There is virtual unanimity on the proposition that justice requires determinations of mens rea and the capability for reasonable control.

Notwithstanding the suggestions by some that questions about mental states should be resolved exclusively at the disposition phase, see, e.g., B. Wootton, *Crime and the Criminal Law* (1963), the criminal law shows no indication of excluding decisions about mental state and responsibility from the adjudicatory phase of criminal trials.

<sup>15</sup> Those who take this position are termed "moral skeptics," a faintly pejorative appellation. Bonnie & Slobogin, *supra* note 8, at 432. The label is also philosophically unsophisticated. See G. Warnock, *The Object of Morality* 125-38 (1971).

<sup>16</sup> Bonnie & Slobogin, *supra* note 8, at 433.



They appear simply to assume that mental health expertise is reliable, valid, and relevant to legal decisionmaking; they therefore may be termed "method votaries."

The method votaries admit that much of the "data" they would allow into evidence is scientifically soft or unverifiable, but they appear to justify its admission on three grounds. First, they uncritically assume the general scientific validity of theory and data.<sup>17</sup> Second, they correctly note that the modern trend in evidence law is to expand greatly the scope of expert testimony and the competence of experts to offer conclusions on ultimate issues.<sup>18</sup> Third, they argue that, in view of the first two assertions, the necessity for the assessment of psychological functioning in criminal law decisionmaking requires the admission of quite imprecise mental health testimony.<sup>19</sup> By contrast, I claim that although the criminal law must take subjectivity into account, this should not justify an "anything goes" criterion for admission of expert testimony. The law should exclude much mental health testimony from the criminal process because it is so unscientific that it cannot assist the factfinder. Indiscriminate admission of mental health testimony is inefficient, misleading, confusing, and sometimes prejudicial.<sup>20</sup>

Under modern evidence law, the primary criterion for admission of testimony is whether it will assist the trier of fact to resolve the relevant issues.<sup>21</sup> In order to deem scientific evidence admissible, courts require that the validity of the science be established.<sup>22</sup> Expert evidence is generally admissible, however, if it is based on matters beyond the ken of laypersons. The increasingly permissive standards for the admissibility of expert testimony are justified on the grounds that the factfinder should have before it all the information that might reasonably aid its determination.<sup>23</sup> Weaknesses or deficiencies in the scientific basis for expert testimony or in the testimony itself are considered matters of weight rather than ad-

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<sup>17</sup> *Id.* at 461-66.

<sup>18</sup> *Id.* at 452-53, 456.

<sup>19</sup> *Id.* at 461, 492-93.

<sup>20</sup> Morse, *supra* note 2, at 604-22, 625-26.

<sup>21</sup> E.g., Fed. R. Evid. 702; Cal. Evid. Code § 801(a) (West 1966).

<sup>22</sup> See, e.g., *Frye v. United States*, 293 F. 1013, 1014 (D.C. Cir. 1923); Gianelli, *The Admissibility of Novel Scientific Evidence: Frye v. United States, a Half-Century Later*, 80 Colum. L. Rev. 1197 (1980).

<sup>23</sup> McCormick's Handbook of the Law of Evidence § 203, at 490-91 (E. Cleary 2d ed. 1972).

missibility. It is alleged that effective cross-examination can expose the weaknesses of an expert's testimony.<sup>24</sup>

I generally agree with permissive standards for the admissibility of expert testimony, but the use of mental health testimony in the criminal justice system should be an exception to the general rule. The integrity of the criminal law, the protection of society, and fundamental principles of justice should require more for admissibility than the facial relevance of theories and methods or their use by some percentage of practitioners in a field.<sup>25</sup> It is not enough to claim, as Bonnie and Slobogin do, that "the professional literature reflects years of experimentation and close observation."<sup>26</sup> Expert testimony should be admitted only if it can truly assist the factfinder. Scientists in the past have accepted numerous doctrines and theories that later have proven preposterous. Before the modern era, the law might have forgiven "scientists" for their mistakes because the methods and instrumentation necessary to test empirical hypotheses were unknown. At present, however, it simply does not suffice to recognize evidence purportedly based on a scientific theory merely because that theory is accepted in the field, or because a proponent of that theory has had "years of experience." If a scientific discipline lacks validity, its "data" simply cannot assist the factfinder. For a science to be the basis of expert testimony, it does not have to be as precise or validated as the laws of motion, but specialized knowledge can assist the trier of fact only if it exists.

Before courts accept evidence from a scientific field, they should examine the evidence to determine its general reliability and validity. Otherwise, there will be no rational means for a court to decide if the seeming relevance of scientific testimony is outweighed by its tendency to be inefficient, misleading or prejudicial. If a particular theory is no more than a set of unverified hypotheses, courts should not accept "expert speculation" based on the theory, even if many practitioners subscribe to it. If there have been scientifically acceptable tests of the hypotheses, courts should still ask what the

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<sup>24</sup> Cf. Morse, *supra* note 2, at 626 (arguing that expert testimony is often inefficient, wasteful, and prejudicial because "too few attorneys are skilled at cross-examining psychiatrists, laypersons outweigh the testimony of experts, and . . . unrestricted use of experts promotes the incorrect view that the questions are primarily scientific").

<sup>25</sup> See *id.*

<sup>26</sup> Bonnie & Slobogin, *supra* note 8, at 464 n.120.

data show. Have propositions relevant to the legal questions in issue been confirmed? If not, courts should not accept testimony based on those propositions simply because a group of persons claim without scientific support that they are a valid account. A criminal defendant is not unfairly prevented from presenting the fullest defense possible if unproven, invalid speculations are excluded.

In a passage of admirable honesty, Bonnie and Slobogin admit that they would exclude in civil commitment cases much of the expertise they would allow so willingly in criminal proceedings.<sup>27</sup> By this admission they badly compromise their position. The inconsistency indicates that their guiding principle is not the validity of the expertise itself; were that the guiding principle, the testimony would be of equal worth in all contexts. Rather, the guiding principle for Bonnie and Slobogin appears to be that the law should restrain the state as much as possible from exerting control over a person in any context. Under this principle, their inconsistency in including or excluding mental health expertise evaporates, because its use in a criminal prosecution and exclusion in civil commitment proceedings both defeat the state's attempt to deprive a person of liberty.<sup>28</sup> Their guiding principle is clearly coherent, and I subscribe fully to their preference for liberty,<sup>29</sup> but there

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<sup>27</sup> *Id.* at 494-95.

<sup>28</sup> Bonnie and Slobogin chide me for overlooking the normative distinction between using expert testimony in civil commitment and criminal law cases. *Id.* at 468 n.133. They argue that in the former, it is the state that typically benefits and the individual who is involuntarily confined, while in the criminal law the defendant seeks to benefit by avoiding the clutches of the state. *Id.* But I have not overlooked this supposed normative distinction—I simply apply different guiding principles. The Bonnie & Slobogin principle is that the state should be prevented from exerting control over the liberty of an individual, whether that person allegedly committed a crime, acted crazily, or whatever. Consequently, they are extremely permissive in allowing the defendant to use mental health expertise to defeat a criminal prosecution, but are equally willing to exclude the same expertise in the civil commitment context.

<sup>29</sup> Although I, too, have a strong preference for liberty, see, Morse, *A Preference for Liberty: The Case Against Involuntary Civil Commitment of the Mentally Disordered*, 70 Cal. L. Rev. 54 (1982), I am not uncomfortable with convicting and punishing guilty persons. Almost all persons are capable of controlling their behavior and are responsible for it. Moreover, in all legal contexts, courts should only accept expert testimony that is reasonable and scientifically valid and that deals with scientific and not legal issues. Thus, evidence that is irrelevant because it is unhelpful on a particular issue in the civil commitment context is similarly irrelevant to the same issue in criminal proceedings. I see no reason why a criminal defendant, simply because he or she is a criminal defendant, should be allowed unjustified

is reason to doubt their commitment to the use of truly relevant evidence in legal proceedings. If the evidence is too weak and too prejudicial for use in commitment proceedings, the law should exclude it in all contexts. But if it is valid in criminal cases and if involuntary commitment is — at least in some cases — a justified exercise of state power, then mental health expert testimony ought to be freely admissible in involuntary commitment cases as well.

Questions of scientific validity aside, the law should also exclude much mental health expert testimony because mental health expertise is generally not necessary to answer the questions posed by criminal law. To clarify this contention, let me reiterate the position I took in *Crazy Behavior* concerning the nature of mental health questions in the law. Criminal courts confronted with mental health cases wish to know whether the defendant was mentally disordered or mentally abnormal at the time of the crime charged<sup>30</sup> and, if so, whether the disorder or abnormality interfered so substantially with the defendant's mens rea or capacity for rationality or self-control that the defendant should be acquitted or considered less than fully responsible.<sup>31</sup>

*Crazy Behavior* was largely devoted to an analysis and legal interpretation of these questions. The analysis and interpretation were the bases for my proposals about the use of expertise.<sup>32</sup> I argued that the law ought not be concerned with whether a person is mentally disordered; instead the crucial question is whether the person was so abnormally crazy that one might reasonably assume

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latitude to use questionable evidence to buttress claims of nonresponsibility. All defendants want to use evidence that may beat the rap, but unless the defendant has a reasonably creditable claim of lessened responsibility, the integrity of the system is compromised by taking invalid claims seriously; no sound normative principle is violated by preventing a defendant from using irrelevant evidence.

<sup>30</sup> Morse, *supra* note 2, at 541, 542-60.

<sup>31</sup> *Id.* at 541, 560-90. I also noted, see *id.* at 590-600, that in some instances factfinders wish to know what the person will do in the future. I demonstrated there that expert predictions, especially of the clinical variety, are likely to be highly inaccurate and therefore should be treated with great skepticism and caution when used as a basis for legal decision-making. Bonnie and Slobogin agree with my evaluation of the predictive ability of experts and the issue will not be addressed further. See Bonnie & Slobogin, *supra* note 8, at 457-61. The most recent authoritative treatment of this issue is J. Monahan, *Predicting Violent Behavior: An Assessment of Clinical Techniques* 41-89 (1981).

<sup>32</sup> Bonnie and Slobogin do not address directly my foundational analysis of mental health law questions, but I assume they are not in complete agreement with it because they reject proposals based directly on it. They do not present an analysis of these questions.

that the actor lacked the usual degree of rational control over his or her behavior.<sup>33</sup> Laws that treat crazy persons differently are bot-tomed on the assumption that such persons lack rational control or free choice. A diagnosis of mental disorder is simply an inaccurate and conclusory proxy for both abnormality and lack of self-control. Disorder or disease explanations stereotypically assume a lack of control, but supporting evidence is lacking. Therefore, I concluded, it is better to confront the question of craziness and lack of control directly. Finally, I contended that whether a person was suffi-ciently crazy to be subject to the application of special mental health laws is a social and moral judgment that can be made on the basis of lay assessment of thoughts, feelings, and actions. Neither knowledge of the causes of craziness nor speculations about disease processes answer the question of whether a particu-lar person is too crazy to be subject to the usual rules.<sup>34</sup>

On the question of the causal relationship between mental disorder and legally relevant behavior, I suggested in *Crazy Behavior* that the question of whether the relationship existed at all re-quired simply a commonsense assessment of whether crazy thoughts, feelings, or actions were the impetus for the legally re-levant behavior.<sup>35</sup> Furthermore, I argued that there is no scientific test for whether a person can control himself or herself, and that whether the causal relationship is so strong that the law should excuse the person is a question science cannot answer.<sup>36</sup> Science might provide data about the strength of the relative pressures craziness exerts on the person's choice to engage in legally relevant behavior, and clinicians might provide relevant evidence about the person's psychological experiences, but whether the pressure is too great to hold the actor responsible is a matter of social and moral judgment.

The implications of this analysis for the proper use of mental health experts are clear. Because these questions can be answered primarily on the basis of lay observations and judgments, expert testimony should be limited to domains where it adds relevant in-formation. I agree that the criminal defendant must be allowed to

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<sup>33</sup> Morse, *supra* note 2, at 542-54.

<sup>34</sup> *Id.* at 562-90.

<sup>35</sup> *Id.* at 554-60, 581.

<sup>36</sup> *Id.* at 585-90.

present a full defense, including the use of relevant expert testimony, but false, confusing, or misleading expertise must be excluded. If the integrity of the criminal process is to be preserved, the expertise of experts must be assessed and not assumed. On the basis of an extensive analysis of the knowledge and skills of mental health professionals, assessed in light of my interpretation of mental health law questions, I proposed the following conclusions concerning the proper contributions of mental health professionals. First, mental health professionals are acute observers of behavior and can therefore efficiently provide the rich behavioral data — observations about thoughts, feelings, and actions — that are necessary to decide mental health law questions.<sup>37</sup> Second, these professionals can present quantitative data based on empirical studies using reasonably sound methodologies to help triers of fact understand the effect craziness exerts on other behavior.<sup>38</sup> Third, the law should prohibit professionals from offering theoretical speculations, including psychodynamic speculations, that are unsupported by hard data. Unproven or invalid theoretical speculations do not help a factfinder properly to organize data, to ascertain states of mind, or to understand the “true” causes of behavior. Fourth, experts should not be allowed to offer opinions on nonscientific, ultimate legal issues. Imprecise but helpful data will be admitted under my proposal, but diagnoses, speculations, and ultimate conclusions provide nothing of value to the factfinder, lead to a conflation of legal and scientific issues, and are generally inefficient and misleading.<sup>39</sup>

### III. PSYCHODYNAMICS AND CRIMINAL LAW: THEORY AND ITS DISCONTENTS

This section will examine in detail the nature of psychodynamic psychology and its relationship to ascriptions of criminal responsibility and the imposition of punishment. Psychodynamic psychology is chosen for examination because it is the psychological theory that has most influenced legal theorists,<sup>40</sup> and because it is the pri-

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<sup>37</sup> *Id.* at 611-15.

<sup>38</sup> *Id.* at 615-22.

<sup>39</sup> *Id.* at 625-26.

<sup>40</sup> For examples of this influence, see A. Ehrenzweig, *Psychoanalytic Jurisprudence* (1971); J. Goldstein, A. Freud & A. Solnit, *Beyond the Best Interests of the Child* (1973); J.

mary theory Bonnie and Slobogin employ to aid criminal justice decisionmaking. The section will argue that, scientifically, psychodynamic psychology is not sufficiently valid or reliable to provide the basis for expert testimony. Moreover, it is not relevant to criminal responsibility and punishment, nor can it aid practical judgments about disposition.

Psychodynamic psychology offers a powerful and appealing theoretical and practical tool for conceptualizing and evaluating all behavior, criminal and noncriminal. Bonnie and Slobogin accept, with becoming caution, psychodynamic explanations as causal accounts of abnormal behavior. They suggest that these explanations should lead in many cases to the mitigation of the punishment due convicted criminals and, in some instances, should lead to complete absolution of criminal defendants. Bonnie and Slobogin implicitly accept the view that if one adduces an unconscious psychodynamic cause of behavior, one must also conclude that the actor's capability for rationality or self-control was compromised to some degree and that the actor's responsibility should be diminished to some degree.<sup>41</sup> If Bonnie and Slobogin are correct, however, our society's moral and practical approaches to criminal behavior are far more misguided than they seem to recognize. The same types of explanations that Bonnie and Slobogin advance to diminish responsibility for what they conceive to be aberrant criminal behavior can also be applied properly to all behavior, inexorably creating a need for a vast reconceptualization of all notions of personal accountability. This task Bonnie and Slobogin do not appear disposed to perform, largely, I believe, because they have not fully explored the psychodynamic tools with which they build their edifice of excusing and mitigating conditions.

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Katz, J. Goldstein & A. Dershowitz, *Psychoanalysis, Psychiatry and Law* (1967); Bienenfeld, *Prolegomena to a Psychoanalysis of Law and Justice, Part I*, 53 Cal. L. Rev. 957 (1965); Bienenfeld, *Prolegomena to Psychoanalysis of Law and Justice, Part II, Analysis*, 53 Cal. L. Rev. 1254 (1965); Goldstein, *Psychoanalysis and Jurisprudence*, 77 Yale L.J. 1053 (1968). Although most criminal law theorizing and decisionmaking makes no systematic, extensive use of any psychological theory, psychodynamic theory appears to be the only theory that has attracted systematic legal attention. For a discussion of the reasons for this attention, see *infra* notes 73-74, 134 and accompanying text.

<sup>41</sup> Bonnie & Slobogin, *supra* note 8, at 446-52.

### A. *The Appeal of Theoretical Romances*

Psychodynamic psychology is the theory of human behavior that posits unconscious variables and processes, especially psychological instincts, conflicts, anxieties and defenses, as the primary causes of behavior.<sup>42</sup> Poets, philosophers, scientists and others have always

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<sup>42</sup> Hereinafter, the terms psychodynamic psychology, dynamic psychology, and psychoanalytic psychology will be used interchangeably, although technically the latter term should be reserved for Freudian psychology. The term "unconscious" as applied to mental contents has two distinct meanings in psychodynamic psychology. A mental content is "descriptively" unconscious if a person is not aware of the content at any given moment, but can bring it to mind by trying to do so. For example, one's telephone number is at most times descriptively unconscious. Freudians also refer to such material as being in the preconscious. In contrast, mental contents are "dynamically" unconscious if they have been forced out of consciousness by uncontrolled and unperceived processes because the contents, if brought to consciousness, would produce too much emotional pain. For instance, Freudian psychology posits that human beings have bisexual instincts and desires, but the homosexual component of the sexual drive is usually dynamically unconscious because it creates anxiety for many persons to be aware of such impulses. Mental contents and processes that are dynamically unconscious constitute the prime determinants of behavior in psychodynamic psychology.

For a brief description of the history, theory, and practice of psychodynamic psychology, see Morse, *An Introduction to Dynamic Psychotherapy*, in *Psychotherapies: A Comparative Casebook* 16-42 (S. Morse & R. Watson eds. 1977). The best primer on Freudian psychology is P. Holzman, *Psychoanalysis and Psychopathology* (1970). A more technical "primer" is C. Brenner, *An Elementary Textbook of Psychoanalysis* (1974). A more historical treatment is R. Fancher, *Psychoanalytic Psychology: The Development of Freud's Thought* (1973). A complete, if somewhat outdated, treatment of the most important psychodynamic psychologies is R. Munroe, *Schools of Psychoanalytic Thought* (1955). Useful summaries are also found in 1 CTP-III, *supra* note 1, at 631-847. The best primary source introductions to psychodynamic psychology are still S. Freud, *Introductory Lectures on Psycho-Analysis*, in 15-16 *The Standard Edition of the Complete Psychological Works of Sigmund Freud* (1915-17) [hereinafter cited as *Standard Edition*]; and S. Freud, *New Introductory Lectures on Psycho-Analysis*, in 22 *Standard Edition* (1933).

The copious writings of Sigmund Freud originally appeared in various journals, as books, and in other forms. Through the prodigious effort of James Strachey, translator and general editor, all the psychological writings have been published in English by The Hogarth Press and The Institute of Psycho-Analysis in the *Standard Edition*. This 24 volume series was published seriatim beginning in 1953 and ending in 1974 with the publication of volume 24. This collection is the standard English reference for Freud. Each of Freud's writings therefore has two dates: the date of its original publication in German and the date of its English translation and publication in the *Standard Edition*. For purposes of simplicity and brevity, only the date of the original publication will be given for each reference.

Although the Strachey translations are by far the best available, allegedly they systematically mistranslate Freud's theory by using pseudoscientific or pseudomedical terms for words and phrases that Freud did not wish to have understood in those ways. For example, Freud used the German word for "soul" throughout his writings, but this was consistently mistranslated as "mental." Bettelheim, *Reflections: Freud and the Soul*, *The New Yorker*, March 1, 1982, at 52, 63-87. The upshot, in part, is that the nature of Freudian theory has



recognized the existence of unconscious determinants of human behavior,<sup>43</sup> but Sigmund Freud is credited with first describing systematically and comprehensively the alleged causal role played by unconscious factors. Over the course of about fifty remarkably productive years, Freud elaborated his views into a theory and mode of treatment that he called psychoanalysis. Freud's extraordinarily complex (and often vague<sup>44</sup> or contradictory<sup>45</sup>) theoretical and clinical writings include observations of behavior,<sup>46</sup> hypothetical inferences about the unconscious determinants of observable behavior,<sup>47</sup> and theoretical statements explaining the operation of unconscious processes and the structure of the mind (the metapsychology).<sup>48</sup> Freud's theory purports to explain almost all human

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been consistently misunderstood by readers of the English translation. See *id.* See also Gill, *Metapsychology Is Not Psychology*, in *Psychology versus Metapsychology—Essays in Memory of George S. Klein* 71, 75-83 (M. Gill & P. Holzman eds. 1976) (explaining Freud's increasing denial of the biological foundation of the theory); *infra* notes 103-106 and accompanying text. But see generally Freud, "The Question of a *Weltanschauung*," in *New Introductory Lectures on Psycho-Analysis*, *supra*, at 158-82 (arguing that psychoanalysis is a science).

<sup>43</sup> See H. Ellenberger, *The Discovery of the Unconscious* 3 (1970) (history and evolution of dynamic psychiatry); L. Whyte, *The Unconscious Before Freud* (1960) (study of the development of human self-awareness in Europe, 1680-1880).

<sup>44</sup> See, e.g., S. Freud, *Instincts and their Vicissitudes*, in 14 *Standard Edition* 117 (1915).

<sup>45</sup> This is accepted by all commentators on Freud. See, e.g., L. Breger, *Freud's Unfinished Journey* (1981); M. Jahoda, *Freud and the Dilemmas of Psychology* 3 (1977). Two examples of theoretical contradictions are discussed in Steele & Jacobsen, *From Present to Past: The Development of Freudian Theory*, 5 *Int'l Rev. Psycho-Analysis* 393, 408-09 (1978) (whether the ego or the id is the storehouse of libido); *id.* at 403-10 (theories on narcissism). An example of methodological contradiction is Freud's view of whether the reports of family members are trustworthy evidence for constructing psychological formulations. Compare S. Freud, *From the History of an Infantile Neurosis*, in 17 *Standard Edition* 14 n.2, with *id.* at 21 (1918). Compare also *id.* with S. Freud, *The Question of Lay Analysis*, in 20 *Standard Edition* 216 (1926).

<sup>46</sup> An example is the intense attachment that psychoanalytic patients develop toward their therapist. Freud termed this ubiquitous occurrence "transference." See S. Freud, *The Dynamics of Transference*, in 12 *Standard Edition* 97 (1912). See also J. LaPlanche & J. Pontalis, *The Language of Psycho-Analysis* 455-62 (1973).

<sup>47</sup> For example, Freud hypothesized that the motive behind almost all dreams was a wish, the so-called "latent content" of the dream, that was expressed in disguised form by the actual dream, the so-called "manifest content." See generally *The Interpretation of Dreams*, in 4-5 *Standard Edition* (1900).

<sup>48</sup> Based on his early training in the hard sciences, Freud constantly created and modified pseudomechanical and physiological models of the mind to explain behavior. Freud's earliest attempt to translate psychological concepts into a purely neurological model was his *Project for a Scientific Psychology*, in 1 *Standard Edition* 295 (1896). Although Freud abandoned this model quite early because it was unworkable, see 1 *Standard Edition* 285-86

behavior, including neuroses,<sup>49</sup> dreams,<sup>50</sup> parapraxes,<sup>51</sup> humor,<sup>52</sup> sexual aberration,<sup>53</sup> psychopathology in general,<sup>54</sup> and culture and religion.<sup>55</sup> Freud constantly modified the theory, and although he did provide masterful summaries from time to time,<sup>56</sup> he never provided a systematic, final exposition in a form that may be tested according to the standards of modern behavioral science.

The source of the data for Freud's observations and theoretical hypotheses was the human relationship that he termed psychoanalysis. Freud treated patients by having them lie down on the fabled couch and talk to him in a form known as "free association," a process where the patient tells the therapist without censorship whatever thoughts, feelings, or memories enter his or her mind.<sup>57</sup> As an avowed determinist, Freud believed that unconscious mechanisms governed by psychological laws produced the flow of free associations. Free associations are thus not truly free. It is necessary to understand the patient's unconscious wishes, impulses, conflicts, and defenses because they are the source of the patient's difficulties.<sup>58</sup>

(1896), prime examples of his later attempts include S. Freud, *The Interpretation of Dreams*, in 4-5 Standard Edition 1, 509 (1900) (Chapter VII, *The Psychology of the Dream-Processes*); S. Freud, *Papers on Metapsychology*, in 14 Standard Edition 105 (1915); S. Freud, *The Ego and the Id*, in 19 Standard Edition 12 (1923).

<sup>49</sup> The most famous early example is J. Breuer & S. Freud, *Studies on Hysteria*, in 2 Standard Edition 3 (1893-95). Extraordinarily complex later case studies are S. Freud, *Notes Upon A Case of Obsessional Neurosis*, in 10 Standard Edition 155 (1909); S. Freud, *From the History of an Infantile Neurosis*, in 17 Standard Edition 7 (1918).

<sup>50</sup> See, e.g., S. Freud, *The Interpretation of Dreams*, in 4-5 Standard Edition 1 (1900).

<sup>51</sup> S. Freud, *The Psychopathology of Everyday Life*, in 6 Standard Edition 1 (1901).

<sup>52</sup> S. Freud, *Jokes and their Relation to the Unconscious*, in 8 Standard Edition 9 (1905).

<sup>53</sup> S. Freud, *Three Essays on the Theory of Sexuality*, in 7 Standard Edition 130, 135-72 (1905).

<sup>54</sup> See, e.g., S. Freud, *Introductory Lectures on Psycho-Analysis*, supra note 42, at 358-77.

<sup>55</sup> See, e.g., S. Freud, *Totem and Taboo*, in 13 Standard Edition 1 (1912); S. Freud, *The Future of an Illusion*, in 21 Standard Edition 5 (1927); S. Freud, *Civilization and its Discontents*, in 21 Standard Edition 64 (1930).

<sup>56</sup> See, e.g., S. Freud, *Introductory Lectures on Psycho-Analysis*, supra note 42; S. Freud, *New Introductory Lectures on Psycho-Analysis*, supra note 42.

<sup>57</sup> Although Freud practiced psychoanalysis for almost five decades and produced an enormous corpus of writing during that time, he wrote surprisingly little about psychoanalytic technique. The primary sources are S. Freud, *Analysis Terminable and Interminable*, in 23 Standard Edition 216 (1937); S. Freud, *Constructions in Analysis*, in 23 Standard Edition 257 (1937); S. Freud, *The Question of Lay Analysis*, supra note 45; S. Freud, *Papers on Technique*, 12 Standard Edition 89-170 (1911-15).

<sup>58</sup> Morse, supra note 42, at 18-20.

During the five decades of his psychoanalytic work, the interchanges between Freud and his patients often produced data that did not fit Freud's current theoretical schema, forcing him constantly to revise the theory.<sup>59</sup> Despite the changes, some tenets of the theory remained constant: all behavior is meaningful and causally related to unconscious dynamic forces; childhood experiences are the source of all behavior; the unconscious is the repository of primitive instincts and of conflicting impulses, wishes and memories, all of which are concerned largely with sexuality and aggression. It is worth emphasizing that Freud's evidence was neither the observation of unconscious forces—they cannot be observed or measured directly—nor the observation or treatment of children. Almost the entire data base for his theory was the free associations of his adult, psychoanalytic patients, a group of predominantly middle-class Viennese citizens who consulted Freud from about 1890 until the 1930's.<sup>60</sup> Freud did not test his hypotheses outside the therapeutic encounter, nor did he encourage his followers to do so. Indeed, he was at best indifferent to external, independent validation of his speculations.<sup>61</sup>

From time to time in therapy, Freud would intervene, interpreting for the patient the unconscious meaning and causes of the patient's behavior. The purpose of such intervention was to provide insight—intellectual and emotional awareness and understanding by the patient of the unconscious determinants of his or her behavior. Freud and his followers believed that insight was the source of therapeutic change and, correlatively, that therapeutic change following insight confirmed the validity of an interpretation as a true account of the causal determinants of the patient's behavior.<sup>62</sup>

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<sup>59</sup> The most celebrated instance of this is when Freud abandoned the view that neuroses were caused by actual seductions by parents or caretakers and adopted the position that fantasized seductions were the causal culprits. This shift is chronicled in Ernest Jones' magisterial biography of Freud. See 1 E. Jones, *The Life and Work of Sigmund Freud* 321-26 (1953).

<sup>60</sup> Freud also used literature and history as evidence for his theories. See, e.g., S. Freud, *Leonardo da Vinci and a Memory of his Childhood*, in 11 *Standard Edition* 63 (1910); S. Freud, *Delusions and Dreams in Jensen's Gradiva*, in 9 *Standard Edition* 7 (1907).

<sup>61</sup> See, e.g., Letter from Sigmund Freud to F. Rosenzweig, *quoted in* MacKinnon & Dukes, *Repression*, in *Psychology in the Making—Histories of Selected Research Problems* 662, 702-03 (L. Postman ed. 1962).

<sup>62</sup> See *infra* notes 93-101 and accompanying text. This was a crucial assertion on Freud's part because only if it were true could psychoanalysis defeat its critics' claims that both the data that patients produced and their behavioral changes were actually the products of the

The other major source of confirmation of Freud's theory was the patient's reaction during the therapeutic hour. For instance, interpretations that led to breakthroughs of feelings or memories or to strong resistance were believed to be accurate, and flows of associations that fit expected patterns were also considered confirmatory.

Freud's psychoanalysis has many descendants, direct and indirect, legitimate and illegitimate. Most psychoanalysts consider themselves direct, legitimate heirs of Freud, building on the original Freudian theoretical corpus while constantly revising it.<sup>63</sup> Many, however, have revised Freud's theory so extensively that it is questionable whether the revisionists are true Freudians.<sup>64</sup> Some theorists, such as Jung<sup>65</sup> and Adler,<sup>66</sup> were clearly schismatics<sup>67</sup> who denied fundamental aspects of Freud's theory (e.g., the centrality of sexual instincts) and formed their own schools of theory and therapy. Others gave sexuality its due, but placed more emphasis on the importance of social and cultural factors in explaining behavior.<sup>68</sup> All the psychodynamic psychologies adhere to the

therapist's suggestion.

<sup>63</sup> Examples are A. Freud, *Normality and Pathology in Childhood: Assessments of Development* (1958); A. Freud, *The Ego and the Mechanisms of Defense* (1936); H. Hartmann, *Essays on Ego Psychology* (1965); H. Hartmann, *Ego Psychology and the Problem of Adaptation* (1958). Because the literature of psychoanalysis is so extensive, this article uses representative examples from the major theories.

<sup>64</sup> E.g., W. Fairbairn, *An Object-Relations Theory of the Personality* (1954); H. Guntrip, *Schizoid Phenomena, Object Relations and the Self* (1968); H. Guntrip, *Personality Structure and Human Interaction* (1961); M. Klein, *Contributions to Psycho-Analysis 1921-45* (4th impression 1968); M. Klein, *The Psycho-Analysis of Children* (1948); H. Kohut, *The Analysis of the Self* (1971); H. Kohut, *The Restoration of the Self* (1977); R. Schafer, *A New Language for Psychoanalysis* (1976); H. Segal, *Introduction to the Work of Melanie Klein* (1964); D. Winnicott, *The Maturation Process and the Facilitating Environment* (1965); D. Winnicott, *Collected Papers: Through Paediatrics to Psycho-Analysis* (1958).

<sup>65</sup> Jung's theory is known as "analytical psychology." The collected works of Jung are published by Princeton University Press. See *The Collected Works of C.G. Jung* (W. McGuire ed. 1973). A good secondary primer is J. Jacobie, *The Psychology of C.G. Jung* (1942).

<sup>66</sup> Adler's theory is known as "individual psychology." The best primary source on Alfred Adler is *The Individual Psychology of Alfred Adler* (H. Ansbacher & R. Ansbacher eds. 1956).

<sup>67</sup> Freud relates his view of the splits with Jung and Adler in S. Freud, *On the History of the Psycho-Analytic Movement*, in 14 Standard Edition 7, 42-66 (1914). Jones chronicles the splits in 2 E. Jones, *The Life and Work of Sigmund Freud* 84-88, 128-34, 137-51 (1955). A portrait more sympathetic to Jung is drawn in R. Steele, *Freud & Jung: Conflicts of Interpretation* (1982).

<sup>68</sup> Termed the neo-Freudians, this latter group included figures such as Harry Stack Sullivan, Erich Fromm, Erik Erikson, and Karen Horney. See, e.g., E. Erikson, *Identity: Youth and Crisis* (1968); E. Erikson, *Childhood and Society* (2d ed. 1963); E. Fromm, *Escape from*

central hypothesis that unconscious forces are the primary determinants of behavior. They differ enormously, however, on the hypothesized development and content of the unconscious forces and on how they influence human behavior.<sup>69</sup> If a mental health clinician says simply that he or she is a psychoanalyst or is psychodynamically-oriented, one cannot be certain what theoretical views the clinician holds—except that dynamically unconscious processes affect behavior.

I believe Freud's theory was successful among clinicians of his time and for sometime thereafter because, in addition to being brilliantly written and interesting, the theory entered an explanatory vacuum. At the turn of the century, there was virtually no scientific knowledge about human behavior in general and about crazy behavior specifically. Freud's theory seemed generally to make sense of crazy behavior for the first time by revealing its apparently understandable hidden determinants. Moreover, the theory was broad and flexible enough to explain all behavior.<sup>70</sup> Freud wrote exquisitely and often narratively about inherently compelling factors—sex, aggression, death, and the deep, dark unconscious—and the theory therefore captured the popular imagination of educated lay persons.<sup>71</sup> Most heartening to clinicians, psychoanalysis appeared to produce a reasonable degree of success in ameliorating crazy behaviors at a time of therapeutic impotence.<sup>72</sup>

In modern behavioral science there are literally thousands of theories competing for recognition. In the face of this competition, I believe there are three basic reasons for the continued success of dynamic theory: dynamic theory is interesting, comprehensive, and

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Freedom (1941); K. Horney, *The Neurotic Personality of Our Time* (1937); H. Sullivan, *The Interpersonal Theory of Psychiatry* (1953).

<sup>69</sup> Morse, *supra* note 42, at 41-42.

<sup>70</sup> This does not mean, as some critics claim, that the theory is nonscientific because it is unfalsifiable. Grünbaum, *Is Freudian Psychoanalytic Theory Pseudo-Scientific by Karl Popper's Criterion of Demarcation?*, 16 *Am. Phil. Q.* 131 (1979). It simply means that hypotheses are available to explain almost all behavior. The scientific validity of dynamic theory will be discussed in detail at *infra* notes 75-135 and accompanying text. It might be noted, however, that some friendly observers believe that psychoanalysis can never become a natural science. See, e.g., McIntosh, *The Empirical Bearing of Psychoanalytic Theory*, 60 *Int'l J. Psycho-Analysis* 405, 408 (1979).

<sup>71</sup> See generally, Burnham, *The Influence of Psychoanalysis upon American Culture*, in *American Psychoanalysis: Origins and Development* 52 (J. Quen & F. Carlson eds. 1978).

<sup>72</sup> The therapeutic success of psychoanalytic therapy will be discussed at *infra* notes 91-101 and accompanying text.

literary-interpretative.<sup>73</sup> Most other theories address only restricted aspects of behavior and, unlike dynamic theory, do not deal with the nitty-gritty of human life. Sex, aggression, and the unconscious are simply more catchy than, for example, unconditioned and conditioned reflexes as explanations for behavior. Most important for lawyers, historians, literary critics and others who seek understanding of behavior and whose primary task is the interpretation of language and events, dynamic theory can be understood as offering an interpretative rather than a mechanistic account of behavior. Freud treated behavior as a language that needed to be interpreted, and he attempted to provide both the grammar and the dictionary. Dynamic theory attempts to make sense of all of human life by explicating its meaning, much as literary criticism makes sense of a poem or historical scholarship makes sense of past events.<sup>74</sup> Lawyers therefore appreciate psychodynamic theory because it speaks comprehensibly to their concerns.

Psychodynamic theory has now been in existence for nearly a century. Despite its continued popularity, however, the law must ask fundamental questions of it. Is it in principle a scientific theory? How much of it has been scientifically validated? And what is its relevance to the criminal law? These are the questions to which this section now turns.

### *B. Does Psychodynamic Psychology Provide a Valid Causal Account of Human Behavior? Science and Its Vicissitudes*

This subsection considers systematically the general scientific validity of psychodynamic psychology. The goal is not to prove conclusively that it is generally invalid; rather, I hope to demonstrate that its validity as a causal, mechanistic account of behavior is so questionable that criminal courts should disallow testimony

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<sup>73</sup> Another major reason for its general success is that psychodynamic theory and practice have been taught and propagated by a formal set of institutions known as psychoanalytic institutes.

<sup>74</sup> In large measure, this is why dynamic theory can be so attractive to lawyers, historians, and literary critics. They are constantly interpreting language and behavior in order to make sense of them. If a psychodynamic theorist is asked to explain a human action, he or she will tell a story about a human life, explaining the behavior by reference to motives—sex, aggression—that we can all understand. It will be a human story about human beings, rather than an account that treats behavior as nothing more than the mechanistic effects of biological, psychological, or sociological variables.

based upon it. At the very least, I hope to demonstrate that large portions of the theory are invalid or unvalidated and should not be accepted as the basis for expert testimony. In sum, I wish to shift the burden of persuasion, to force proponents of psychodynamic psychology to demonstrate—not simply to assert—that the theory in general—or at least some specific propositions of the theory—are sufficiently valid to serve as the basis for expert opinion.

In assessing the scientific validity of psychodynamic psychology, it is important to remember that there is no systematic, coherent, accepted series of principles grouped under the rubric “psychodynamic psychology.”<sup>76</sup> There is only one common principle—that unconscious determinants affect behavior. There are an enormous number of hypotheses, many of which are contradictory, all competing in the intellectual and clinical marketplace.<sup>76</sup> New hypotheses rarely replace the old ones; instead, they co-exist in varying states of complementarity.<sup>77</sup> How, then, is the puzzled observer to

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<sup>76</sup> McIntosh, *supra* note 70, at 408 (theory has no significant formal structure); Morse, *supra* note 42, at 41-42.

<sup>76</sup> A recent example is the divergent and widely influential views of Heinz Kohut and Otto Kernberg on borderline or narcissistic personalities. See Robbins, *Current Controversy in Object Relations Theory as Outgrowth of a Schism between Klein and Fairbairn*, 61 *Int'l J. Psycho-Analysis* 477 (1980) (views of Kohut and Kernberg seem “fundamentally antagonistic”; the roots of the antagonism are found in a theoretical split between earlier thinkers).

<sup>77</sup> See S. Fisher & R. Greenberg, *The Scientific Credibility of Freud's Theories and Therapy* 6-7 (1977). An example is the treatment accorded a phenomenon called the “transitional object.” See Brody, “Transitional Objects”: Idealization of a Phenomenon, 49 *Psychoanalytic Q.* 561 (1980).

To understand why this is so, it is necessary to examine how psychodynamic psychology developed and has been tested. Almost all the data gathered by the first generation of psychodynamic psychologists, including Freud, Jung, and Adler, came from therapeutic consultations. The theories were constantly revised in light of the communications emanating from the consulting room, but there never was an attempt to set down in systematic, testable form the theoretical propositions derived from the data. Psychodynamic psychology was veiwed from the first as a constantly evolving theory that was always open to revision, but most of its schools, especially the still pre-eminent Freudians, developed a cult around the works of the founder. Treated as holy writ, the founder's writings are the subject of constant exegesis and heated dispute about their meaning. No theoretical advance within a school is possible without ritual obeisance to what the Master had to say about a particular topic. Moreover, the schools tend to be quite hostile to one another. Freud himself was especially dogmatic and pitiless to those who strayed too far from the received wisdom. Heretics—a word that is none too strong—such as Jung and Adler were banished, besmirched, and even vilified for daring to question hypotheses, such as the centrality of sexual instincts, which Freud considered crucial to the theory. See *supra* note 67.

In the psychoanalytic movement, disputes are solved by assertions of authority rather than according to acceptable methods of scientific investigation. S. Fisher & R. Greenberg,

determine if dynamic psychology has been scientifically validated to some reasonable extent?<sup>78</sup> Which theorist or which hypotheses

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supra; Holzman, *The Future of Psychoanalysis and Its Institutes*, 45 *Psychoanalytic Q.* 250, 269 (1976). At most, a theorist will use anecdotal and unchecked case reports to buttress assertions of theoretical superiority. G. Klein, *Psychoanalytic Theory: An Exploration of Essentials* 64 (1976). For instance, orthodox Freudians are challenged today by the theoretical deployments of a psychoanalyst named Heinz Kohut. This new teacher, lately deceased, preached that the Oedipus complex, so central to the orthodox theory, is perhaps less important for explaining the problems of many patients than psychological disturbances arising from an earlier developmental period when the self is being organized. H. Kohut, *The Analysis of Self*, supra note 64. A comparison of Kohut's views with those of more orthodox Freudians is contained in Kohut, *The Two Analyses of Mr. Z*, 60 *Int'l J. Psycho-Analysis* 3 (1979). The Oedipus complex refers to the simultaneous sexual, rivalrous, and aggressive feelings that all children between the ages of three and six are alleged to experience toward their parents. These feelings are repressed because they are too threatening to be confronted consciously. The repression of the Oedipus complex is responsible for the child's sexual identity, for the formation of conscience, for patterns of loving and hating, and for other crucial aspects of personality and psychopathology. In orthodox Freudian theory, the resolution of the Oedipus complex is considered the crucial psychological event of childhood. See, e.g., O. Fenichel, *The Psychoanalytic Theory of Neurosis* 91-98 (1945). In modern psychodynamic psychology, the Oedipus complex seems to be declining in importance as a crucial construct. See Loewald, *The Waning of the Oedipus Complex*, 27 *J. Am. Psychoanalytic A.* 751 (1979).

Although Kohut's decade old theory is enormously influential, my search disclosed no empirical test of the theory. But cf. Silverman & Wolitzky, *Toward the Resolution of Controversial Issues*, in *Psychoanalytic Treatment Factors in Dynamic Psychotherapy* 321, 329-39 (S. Slipp ed. 1982) (some ongoing research not based on Kohutian theory may bear on it). Indeed, the evidence considered crucial by Kohutians to prove the theoretical and clinical validity of Kohut's theory is purely uncontrolled case study material. See, e.g., *The Psychology of the Self: A Casebook* (A. Goldberg ed. 1978); Kohut, *The Two Analyses of Mr. Z*, supra (clinical validity of Kohut's theory demonstrated by successful re-analysis using Kohutian methods on a patient formerly analyzed by more traditional methods). The cases collected in the Goldberg volume, supra, are criticized intensively and extensively in Gedo, *Reflections on Some Current Controversies in Psychoanalysis*, 28 *J. Am. Psychoanalytic A.* 363 (1980). Gedo is especially harsh on the "scientific" quality of the Kohutian case studies. He claims that Kohutian analyses are incomplete and that "theoretical inferences derived from observations made in the course of such incomplete analyses fail to carry scientific conviction." *Id.* at 382. He also complains that "there is no inductive reasoning on this work," citing one case study author who was "unable to hear the significance of what the patient was saying until I [the author] had the conceptual assistance of a new analytic discovery." *Id.* at 367. Although I am sympathetic to Gedo's viewpoint, it is clear that his critique is based on exactly the same sort of soft case study material and vague, suspect theoretical inferences for which he takes the Kohutians to task. One wonders what Gedo means by "scientific." Within the psychoanalytic movement, this kind of debate and evidence are prototypical of disputes about psychodynamic theory and practice.

<sup>78</sup> A theory is scientifically validated if it is confirmed by reasonable deductive or inductive procedures. In the behavioral sciences, such as psychodynamic psychology, reasonable validation includes subjecting clear hypotheses to possible disconfirmation, and using reliable and valid procedures that make the observational data available to outside observers



are particularly important? It is a mark of the scientific confusion surrounding psychodynamic psychology that there are no truly acceptable answers to these questions.<sup>79</sup> Given these difficulties, how do psychodynamic psychologists and behavioral scientists assess the relative merits of the wealth of propositions that flow from innumerable therapeutic consultations?

It is necessary first to discuss briefly whether psychodynamic theory is sufficiently scientific in form to be worthy of serious scientific consideration.<sup>80</sup> Many eminent philosophers of science such as Popper<sup>81</sup> have claimed that psychoanalytic theory is so vague and internally contradictory that it is impossible to confirm it and, more importantly, to disconfirm it. It is true that the various psychodynamic theories are often vague and contradictory; however, if one carefully considers the writings of Freud and other dynamic psychologists, one can derive testable and disconfirmable hypotheses. In a series of exhaustive and penetrating analyses, the eminent philosopher of science Adolf Grünbaum has demonstrated beyond reasonable doubt that in principle some of Freud's theory is scientific and that the criticisms made by Popper and others are the result of an insufficiently close reading of the theory.<sup>82</sup> This is not to say that all psychodynamic psychologists would agree on the particular statement of a testable hypothesis derived from the the-

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and control for the influence of extraneous and confounding variables and alternate hypotheses. See Silverman & Wolitzky, *supra* note 77, at 322 (delineating "minimal requirements for any discipline that aspires to be a science").

<sup>79</sup> It is utterly extraordinary that although psychoanalytic psychology has existed for nearly a century, its scientific status is "equivocal." Bowlby, *Psychoanalysis as a Natural Science*, 8 *Int'l Rev. of Psycho-Analysis* 243 (1981). Indeed, psychoanalysis has probably been more self-consciously "preoccupied . . . with its status as a science" than any other discipline that purports to be a science. Silverman & Wolitzky, *supra* note 77, at 321-22. This is entirely unsurprising, however, if one remembers the utterly unscientific manner in which psychoanalytic "research" has been carried on within the psychoanalytic movement. See *supra* note 77; *infra* notes 84-135 and accompanying text.

<sup>80</sup> The most useful general collection on this issue is *Psychoanalysis, Scientific Method and Philosophy* (S. Hook ed. 1959).

<sup>81</sup> *Science: Conjectures and Refutations*, in K. Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge* 33-39 (1962). Another example is Cioffi, *Freud and the Idea of a Pseudo-Science*, in *Explanations in the Behavioral Sciences* 471 (R. Borger & F. Cioffi eds. 1970). See also H. Eysenck & G. Wilson, *The Experimental Study of Freudian Theories* 1-13 (1973).

<sup>82</sup> Grünbaum, *supra* note 70; Grünbaum, *Can Psychoanalytic Theory Be Cogently Tested on "the Couch"?*, *forthcoming in Explanation and Evaluation in Psychiatry and Medicine* (L. Laudan ed. 1982).

ory, nor is it to say that they would agree that a particular test of a hypothesis is valid. The claim is more limited: simply that one can reasonably derive from the various theories some testable hypotheses. That the theory can be tested scientifically does not, of course, guarantee or even suggest that it is valid.<sup>83</sup>

Psychodynamic psychology has been tested using three basic methods—by observing the patient's response during therapeutic consultations, by assessing the efficacy of dynamic therapy, and by external, independent, controlled quantitative studies of hypotheses derived from the clinical and theoretical writings. An examination of the validity of these methods or their results reveals that psychodynamic theory in general—or, at the least, great portions of it—cannot withstand the rigors of scientific scrutiny.

Psychodynamic psychologists historically have relied almost entirely on the couch as the primary means for testing as well as deriving their theoretical propositions. If a patient does not produce expected data (associations, memories, affects), especially in reaction to a theory-based therapeutic intervention, a piece of theory is tentatively disconfirmed; conversely, if a patient does produce expected data, the piece of theory is tentatively confirmed.<sup>84</sup> This

<sup>83</sup> Nagel, *Methodological Issues in Psychoanalytic Theory*, in *Psychoanalysis, Scientific Method and Philosophy*, supra note 80, at 38, 55; Salmon, *Psychoanalytic Theory and Evidence*, in *Psychoanalysis, Scientific Method and Philosophy*, supra note 80, at 252, 265. Indeed, Grünbaum's reading of the evidence is that Freud's theory is partially scientific in form, but that it is not validated. Grünbaum, supra note 82, at 54, 197, 206; Nagel, supra (agreeing with Grünbaum).

<sup>84</sup> See S. Freud, *Constructions in Analysis*, supra note 57, for an early explanation of this view. For example, therapist *A* may claim: "My patients behave such and such a way, predicted and explained by *Y*'s theory; therefore, *Y*'s theory is correct." Therapist *B* says in response: "Nonsense. My patients do not behave in that way; furthermore, if there is any merit to *Y*'s observations, they can be explained by the theory of *Q*." And so on.

An examination of the training that psychodynamic psychologists receive reveals how the unusual testing system developed. Full training is reserved mainly for physicians with psychiatric training. If the candidate is accepted by a psychoanalytic institute, his or her training consists of three experiences. First, the candidate undergoes psychoanalysis, a process referred to as a "training analysis." The training analysis, the sine qua non of the process, will not be considered successful unless the candidate accepts the account of his or her behavior interpreted by the analyst. Glover, *Research Methods In Psycho-Analysis*, 33 *Int'l J. Psycho-Analysis* 403, 403-04 (1952). Second, the candidate engages in academic training, but only in the school or schools of dynamic psychology adhered to by his or her training institute. The candidate is taught that the proper data base for deriving and testing theory is primarily the therapeutic dialogue. Finally, the candidate conducts some analyses under the supervision of senior members of the training institute, who naturally supervise the candidate according to the theoretical and therapeutic biases of the supervisor and the

process sometimes results in the discarding of hypotheses, but it rarely challenges the major tenets of the system. Moreover, external evidence seldom causes modifications of the theory.<sup>85</sup> The psychoanalytic environment is a sealed cosmos that communicates far too little with the outside world.<sup>86</sup>

For dynamicists, the couch produces many types of data that dynamic clinicians believe confirm their theory: the relationship with the therapist develops in familiar ways predicted by the theory; a patient's series of free associations can be understood only according to the theory; a patient responds to an interpretation based on the theory with a flood of affects or a group of associations that appear to confirm the validity of the interpretation. Yet the behavior of almost all patients "confirms" the diverse and often contradictory theories of their various therapists, and thus validates no single theory.<sup>87</sup> The patients of Freudians dream dreams, respond to interpretations, and understand themselves as Freud's theory suggests. But so do the patients of Jungians, Adlerians, existential-

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institute.

<sup>85</sup> Some notable counter-examples do exist, however. Freud believed, for instance, that females experienced two distinct orgasms depending on whether the source of the excitation was primarily clitoral or vaginal. He had no physiological evidence for this assertion, however, and recent research on sexual physiology has thoroughly disproved it. M. Sherfey, *The Nature and Evolution of Female Sexuality* (1966). The psychodynamicists must therefore revise at least some aspects of their theories of female psychology. See Stoller, *Overview: The Impact of New Advances in Research on Psychoanalytic Theory*, 130 *Am. J. Psychiatry* 241, 244 (1973) (Freud's theory of development of female character is incorrect). Stoller also notes that the impact on psychoanalytic theory of the new advances in sex research has been "mild," but that the impact on analysts "may be considerable." *Id.* at 249. No support is cited for either proposition.

An example of the sweeping use of external evidence to modify dynamic theory is found in the noted work of John Bowlby, who has used ethology and information-processing models extensively to reconceptualize dynamic theories of attachment, separation, and loss. See 1 J. Bowlby, *Attachment and Loss, Attachment* (1969); 2 *id.*, *Separation* (1973); 3 *id.*, *Loss* (1980).

<sup>86</sup> See, Bowlby, *supra* note 79, at 250. I recently examined the references to all the articles that appeared in the 1979 volume year of the two leading psychoanalytic journals — 60 *Int'l J. Psycho-Analysis* (1979) and 27 *J. Am. Psychoanalytic A.* (1979). The examination revealed that in 38 articles of 60 *Int'l J. Psycho-Analysis* (1979), 96% of the references cited were to psychoanalytic sources. Of the remaining 4%, 34% were to nonscientific sources and 66% were to social or behavioral science sources. In 27 *J. Am. Psychoanalytic A.* (1979), 84% of the references were to psychoanalytic sources. Only four of the 29 articles in the volume accounted for 62% of the nonpsychoanalytic sources. In a supplement on psychotherapy published in 27 *J. Am. Psychoanalytic A.* (1979), 98.5% of the references were to psychoanalytic sources.

<sup>87</sup> Marmor, *Limitations of Free Association*, 22 *Archives Gen. Psychiatry* 160, 161 (1970).

ists, and all the others.

One might sensibly ask whether the couch is an epistemologically valid means of validating dynamic theory. Can unchecked (or checked) observations by therapists of the behavior of patients during therapeutic encounters properly test dynamic theory? This is a matter of great dispute.<sup>88</sup> The most persuasive analysis suggests, however, that the patient's behavior in the consulting room is badly contaminated as a data source.<sup>89</sup> The intense relationship that develops between therapist and patient creates and continuously enhances the patient's disposition to accept the authoritative therapist's (mostly) unwitting suggestions. Patients learn and produce the appropriate responses to please the therapist. The patient's behavior then naturally conforms to and confirms the therapist's theoretical expectations—expectations that are themselves the source of the therapist's suggestive behavior.

This process is readily explicable: confirmation of the therapist's theory is, after all, terribly important to both patient and therapist. The patient is in difficulty and has consulted an expert upon whom he or she is quite dependent; the patient naturally wants to please the therapist and to have a successful therapeutic experience. The therapist strongly needs to have the worth and validity of therapy substantiated. It is unremarkable, then, that patients behave in accord with the theoretical expectations of their ther-

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<sup>88</sup> Analysts have always assumed that the couch is a proper laboratory. See, e.g., S. Fisher & R. Greenberg, *supra* note 77, at 7. Indeed, Freud himself felt that this type of evidence, and the success of psychoanalytic therapy, were the only necessary methods of confirmation, but we need not accept assumptions and assertions that are unsupported by data or convincing analysis.

<sup>89</sup> Grünbaum, *Can Psychoanalytic Theory be Cogently Tested on "the Couch"?*, *supra* note 82; Grünbaum, *Epistemological Liabilities of the Clinical Appraisal of Psychoanalytic Theory*, 14 *Nous* 307 (1980) (expanded version of Grünbaum's 1979 article with the same title); Grünbaum, *Epistemological Liabilities of the Clinical Appraisal of Psychoanalytic Theory*, 2 *Psychoanalysis & Contemp. Thought* 451 (1979). See Eagle, *Psychoanalytic Interpretations: Veridicality and Therapeutic Effectiveness*, 14 *Nous* 405 (1980) ("So decisively has Grünbaum shown that therapeutic outcome and other clinical data cannot validate psychoanalytic propositions, that there is little more one can say directly on the question."). See also Kubie, *Problems and Techniques of Psychoanalytic Validation and Progress*, in *Psychoanalysis as Science* 46, 118 (E. Pumpian-Mindlin ed. 1952) (data from therapeutic interactions "are hardly the stuff out of which fundamental scientific advances can be fashioned"). As George Klein put it: "Among the sorriest clichés I have heard in psychoanalytic circles are the view that doing therapy *is* research and . . . that treatment *is* experimentation." G. Klein, *supra* note 77, at 64 (emphasis in original).

apists.<sup>90</sup> The associations of Freudian patients confirm Freudian theory, and the associations of Jungian patients confirm Jungian theory. Such results from the couch cannot sustain the validity of dynamic theory.

A second method of testing dynamic psychology is to assess its efficacy as a therapy. If psychodynamic theory cannot be confirmed by evidence from the consulting room, can it be proven valid because the therapy based on it is successful? After all, psychoanalysis was born as, and remains, primarily a method of treating behavioral disorders, and Freud thought that the success (or failure) of psychoanalytic therapy was critical evidence of its validity (or invalidity).<sup>91</sup> For decades this process was carried out entirely unscientifically and uncritically,<sup>92</sup> leading to claims and counterclaims that were equally invalid. After almost four decades of scientific study of psychotherapeutic outcomes, however, it is now possible, despite many pitfalls, to reach reasonably valid conclusions about therapeutic efficacy.

Freud and all the dynamicists who followed him believe that repressed unconscious factors are the mechanisms that distort our lives and produce psychopathology. People experience difficulties because they react not only to the stimuli appropriate to a situation, but also to the irrational and unconscious forces, mechanisms,

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<sup>90</sup> A further reason rests upon the reciprocal nature of free association and the therapist's expectations. Suppose a patient begins associating to an element of a dream. At what link in a theoretically endless chain of associations will a therapist intervene to help the patient discover the meaning of the element? If a person begins to free associate to any stimulus, at some point associations connected to almost all important aspects of human existence will arise. D. Taylor, *Explanation and Meaning* 28-31 (1970). Clearly the therapist will intervene at the link that the theory holds is critical and will consider the theory confirmed. By intervening at that point, the therapist conveys the information to the patient that *this* association is the crucial one, thus encouraging the patient to produce similar associations and to believe that they are in fact crucial. But unless there is some external confirmation of the therapist's theory, there is no reason to believe that the association confirms the theory, because the patient was certain to produce associations that would confirm other theories as well.

<sup>91</sup> S. Freud, *Introductory Lectures on Psycho-Analysis*, *supra* note 42, at 452.

<sup>92</sup> By this I mean that at the end of therapy, therapists would make global assessments concerning how much the patient had improved, but there were no checks on the accuracy of the therapist's observations during therapy nor were there controls to insure that the simple passage of time (spontaneous remission, maturation) or other variables were not the causes of the alleged therapeutic effect. These issues will be discussed in greater detail at *infra* notes 98-101 and accompanying text.

and processes within.<sup>93</sup> Because the actor does not have access to the distorting unconscious stimuli, he or she acts without all the data necessary to behave maturely and reasonably. According to dynamic therapists, the corrective is *insight*: the patient must become aware at an intellectual *and* emotional level of the unconscious impulses, wishes, memories, affects, conflicts, and defenses that are so troublesome. Armed with self-knowledge, the person will supposedly perceive himself or herself and others in an undistorted, adult fashion and will no longer be buffeted by unknown, irrational forces.<sup>94</sup> The method of dynamic therapy, then, is for the therapist to help the patient discover his or her own unconscious motives and mechanisms.

Freud believed that *accurate* insight was the only agent of enduring and deep therapeutic success, and that such success was therefore the guarantee of the validity of psychoanalytic theory against the charge that therapeutic results were the products of suggestion. Freud wrote:

The doctor has no difficulty, of course, in making him [the patient] a supporter of some particular theory and in thus making him share in some possible error of his own. In this respect the patient is behaving like anyone else — like a pupil — but this only affects his intelligence, not his illness. After all, his conflicts will only be successfully solved and his resistance overcome if the anticipatory ideas he is given tally with what is real in him. Whatever in the doctor's conjectures is inaccurate drops out in the course of the analysis; it has to be withdrawn and replaced by something more correct.<sup>95</sup>

Freud viewed correct insight, the necessary condition for therapeu-

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<sup>93</sup> For instance, a student may misinterpret and feel unnecessarily badly about a communication from a teacher because the student may unwittingly react to the teacher in light of continuing unconscious conflicts that developed in the early relationships with his or her parents.

<sup>94</sup> This is still an article of faith among many psychodynamic practitioners. See, e.g., Miller, *The Drama of the Gifted Child and the Psycho-Analyst's Narcissistic Disturbance*, 60 *Int'l J. Psycho-Analysis* 47 (1979) ("[T]here is only one weapon available against mental sickness: emotional discovery and acceptance of the truth in our individual and unique childhood history.").

<sup>95</sup> Freud, *Introductory Lectures on Psycho-Analysis*, supra note 42, at 452. In later life, when Freud was wiser, he recognized that psychoanalysis was often ineffective and that other experiences such as religious conversion could be quite effective. S. Freud, *New Introductory Lectures of Psycho-Analysis*, supra note 42, at 152.

tic success, as possible only through the uncovering work of analysis.<sup>96</sup> Consequently, when a patient improved, Freud concluded that the insights provided by the analyst tallied with what was causally real, and that only psychoanalysis could have produced therapeutic success in this manner.<sup>97</sup> Freud's bold conclusion was that therapeutic success vouched for the accuracy of the insights in an individual case and confirmed the validity of his theory in general.

Confirming the validity of psychodynamic psychology by reference to its therapeutic success raises three distinct questions: (1) is psychodynamic therapy more successful than spontaneous remission? (2) is it more successful than therapies based on quite different theories of behavior? and (3) if dynamic therapy is successful, is its success produced by accurate insight into the causes of behavior, as dynamicists claim?<sup>98</sup> If the therapy is not successful or if dynamic insight is not the agent of change, dynamic theory is disconfirmed according to Freud's own standards.

There have been hundreds of "outcome" studies, of differing methodological rigor, which have examined various therapies. The

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<sup>96</sup> Grünbaum, *Can Psychoanalytic Theory be Cogently Tested on "the Couch"?*, *supra* note 82, at 72, and Grünbaum, *Epistemological Liabilities of the Clinical Appraisal of Psychoanalytic Theory*, *supra* note 89, at 321, refer to the crucial point as the "Necessary Condition Thesis."

<sup>97</sup> Grünbaum, *Can Psychoanalytic Theory be Cogently Tested on "the Couch"?*, *supra* note 82, at 73, and Grünbaum, *Epistemological Liabilities of the Clinical Appraisal of Psychoanalytic Theory*, *supra* note 89, at 323, refer to this whole line of argument as the "Tally Argument."

<sup>98</sup> In more credulous times, a therapist would note that some patients improved and the therapist would then claim that the therapy was successful. Behavioral scientists recognize that patient improvement simpliciter is invalid evidence of efficacy; more careful, controlled studies must be performed before one can make claims for the efficacy of a therapy. For a thorough cataloguing of the objections to scientific study of psychotherapy outcome and a persuasive response to the objections, see M. Smith, G. Glass & T. Miller, *The Benefits of Psychotherapy* 24-35 (1980) [hereinafter cited as *Benefits*]. Good outcome studies should include the following methodological parameters: the group of patients receiving the therapy (the "experimental" group) should be compared to control groups that receive either no therapy or a placebo, and therapeutic change ought to be judged by independent, external observers who have no stake in the outcome. Useful treatments of research design in psychotherapy outcome studies are J. Gottman & H. Markman, *Experimental Designs in Psychotherapy Research*, in *Handbook of Psychotherapy and Behavior Change: An Empirical Analysis* 23 (S. Garfield & A. Bergin 2d ed. 1978); J. Meltzoff & M. Kornreich, *Research in Psychotherapy* 3-60 (1970); Epstein & Vlok, *Research on the Results of Psychotherapy: A Summary of Evidence*, 138 *Am. J. Psychiatry* 1027, 1027-28 (1981) (briefly listing the criteria for good outcome research).

overall conclusion of these studies is remarkable: all therapies conducted under all types of conditions seem to offer a greater chance of improvement in short-term emotional feeling than spontaneous remission, but, with a few minor exceptions, no type or condition of therapy seems more successful than any other.<sup>99</sup> It does not seem to matter which therapy a patient chooses; simply being in therapy works. The dynamic therapies are no more efficacious than other therapies that are not based on dynamic theory and insight. Making the unconscious conscious does not, as Freud and his theory predict, offer a unique route to true and durable emotional improvement.<sup>100</sup> Although the finding of no special efficacy

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<sup>99</sup> Benefits, *supra* note 98, at 183-89. The meta-analysis provided in this work is a comprehensive, useful and persuasive integration of the vast psychotherapy outcome literature. These findings have recently been replicated by a re-analysis of its data. See Andrews & Harvey, *Does Psychotherapy Benefit Neurotic Patients? A Reanalysis of the Smith, Glass and Miller Data*, 38 *Archives Gen. Psychiatry* 1203 (1981). The methodology of Benefits has been harshly criticized. See S. Rachman & G. Wilson, *The Effects of Psychological Therapy* 248-56 (2d ed. 1980); Eysenck, *An Exercise in Mega-Silliness*, 33 *Am. Psychologist* 517 (1978). For the answer to their critics by the authors of Benefits, see Glass & Smith, *Reply to Eysenck*, 33 *Am. Psychologist* 517 (1978). For a highly favorable review of Benefits that discusses the issues, see Abeles, *Psychotherapy Works*, 26 *Contemp. Psychology* 821 (1981). I have chosen to rely on the Benefits conclusions because they are the most favorable to psychodynamic psychology and therefore least favorable to my thesis. See generally Epstein & Vlok, *supra* note 98.

The only exception to the statement in the text applies to treatment of specific phobias or habit disturbances such as smoking. For such problems, behaviorally oriented therapies are almost certainly more effective. See S. Rachman & G. Wilson, *supra*, at 257.

<sup>100</sup> Indeed, a well-respected, although behaviorally biased, pair of scholars have recently concluded from an intensive review of the literature that "there still is *no* acceptable evidence to support the view that psychoanalysis is an effective treatment." S. Rachman & G. Wilson, *supra* note 99, at 76 (emphasis added). These authors also dispute the conclusion that all therapies work equally well. They believe that behavior therapy has clearly shown itself to be more effective in general than other forms of psychological treatment. *Id.* at 255-77. See generally *id.* at 117-94.

No review of the research literature has concluded that dynamic therapy is more successful than other therapies in general or for specific problems. See Luborsky, Singer & Luborsky, *Comparative Studies of Psychotherapies: Is it True That "Everyone Has Won and All Must Have Prizes"?*, 32 *Archives Gen. Psychiatry* 995 (1975). At best, reviewers conclude that it is no less effective than other therapies. One might argue, however, that because psychoanalysis and long-term dynamic psychotherapy are more expensive and time consuming than other therapies, an overall cost-benefit analysis leads to the conclusion that they are the least desirable. Proponents of dynamic therapy have attempted to meet such objections by developing quite brief and relatively inexpensive methods of psychotherapy based on psychodynamic principles. See, e.g., D. Malan, *The Frontier of Brief Psychotherapy* (1976); J. Mann, *Time-Limited Psychotherapy* (1973). There is no evidence that these brief methods are more or less effective than other types of psychotherapeutic treatment, including their more extensive ancestors.



does not mean that Freud's theory is invalid, it does mean that, on the basis of current evidence of therapeutic results, neither psychodynamic theory in general nor any particular school can claim greater validity than any other psychological theory.

Moreover, dynamic theory cannot claim that it is at least one valid theory among others or that it is partially valid because it achieves some therapeutic success. The issue is whether improvement under dynamic therapy is the result of insight, the asserted agent of change, or whether it is the result of a placebo or an as yet unidentified therapeutic agent. If Jungian patients improve with Jungian insights, Freudian patients improve with Freudian insights, and so on, and all improve equally, it is difficult to claim credibly that all the theories are equally correct. If the widely diverse and often contradictory insights of the various dynamic theories and the wide range of psychological interventions employed by the other therapies are all equally efficacious, it appears very unlikely that the specific insights or interventions are the true change agents. There is simply no evidence that it is the specific content of these different insights that cure. We need not accept the conclusion of the Dodo at the end of the Caucus Race in *Alice's Adventures in Wonderland*: "Everybody has won and all must have prizes."<sup>101</sup> Unless one is willing to make the incredible assumption

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Finally, one investigator has concluded that in ratings of the success of dynamic therapy, symptomatic improvement is a highly significant component of the rating. See Mintz, *Measuring Outcome in Psychodynamic Psychotherapy: Psychodynamic vs. Symptomatic Improvement*, 38 Archives Gen. Psychiatry 503, 506 (1981) (also questioning the need for an expert clinician's "dynamic assessment" in order to rate improvement).

<sup>101</sup> L. Carroll, *Alice's Adventures in Wonderland* 33 (Signet Classics ed. 1960). The full text is:

"Why," said the Dodo, "the best way to explain it is to do it." (And, as you might like to try the thing yourself some winter-day, I will tell you how the Dodo managed it.)

First it marked out a race-course, in a sort of circle ("the exact shape doesn't matter," it said), and then all the party were placed along the course, here and there. There was no "One, two, three, and away!", but they began running when they liked, and left off when they liked, so that it was not easy to know when the race was over. However, when they had been running half an hour or so, and were quite dry again, the Dodo suddenly called out "The race is over!" and they all crowded round it, panting, and asking "But who has won?"

This question the Dodo could not answer without a great deal of thought, and it stood for a long time with one finger pressed upon its forehead (the position in which you usually see Shakespeare, in the pictures of him), while the rest waited in silence. At last the Dodo said "Everybody has won, and all must have prizes."

that *all* the psychological theories upon which the different therapies are based, including the diverse dynamic theories, are all valid, and equally so, then one must acknowledge that studies of therapeutic outcome do not confirm the validity of dynamic therapy.

The third method of testing psychodynamic theory is by external, empirical studies of theoretical propositions. In contrast to validation from the couch or consulting room, many behavioral scientists, struck by the rich comprehensiveness of psychodynamic psychology and by its impact on psychiatry and culture, have attempted to test dynamic propositions externally by controlled, quantitative, correlational and experimental studies. Almost always they have used the works of Freud himself: they have attempted to restate Freud's hypothesis in forms suitable for testing according to the methods of modern behavioral science. Freud is the most sensible theoretician to test, for he is the fount of psychodynamic psychology and there is little reason to test a disciple's theoretical offspring rather than the works of the teacher.<sup>102</sup> The

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Id. (emphasis in original)

The more likely explanation for equal therapeutic success is that all the various psychotherapies share certain, nontheoretically related characteristics that are the source of behavioral melioration. Many commentators have suggested that this is the case, and it certainly is the most parsimonious account of the empirical evidence. J. Frank, *Persuasion and Healing* 1, 2 (rev. ed. 1973); Hobbs, *Sources of Gain in Psychotherapy*, 17 *Am. Psychologist* 741 (1962); Strupp, *On the Basic Ingredients of Psychotherapy*, 41 *J. Consulting & Clinical Psychology* 1 (1973); Strupp, *The Interpersonal Relationship as a Vehicle for Therapeutic Learning*, 41 *J. Consulting & Clinical Psychology* 13 (1973); Strupp & Hadley, *Specific vs. Nonspecific Factors in Psychotherapy: A Controlled Study of Outcome*, 36 *Archives Gen. Psychiatry* 1125, 1135 (1979). See also Cross, Sheehan & Khan, *Short- and Long-Term Follow-Up of Clients Receiving Insight-Oriented Therapy and Behavior Therapy*, 50 *J. Consulting & Clinical Psychology* 103, 111-12 (1982). But see Garfield, *Basic Ingredients or Common Factors in Psychotherapy?*, 41 *J. Consulting & Clinical Psychology* 9 (1973).

Two eminent theoreticians have offered unifying, theory-based explanations for the success of the diverse psychotherapies. Bandura, *Self-Efficacy: Toward a Unifying Theory of Behavioral Change*, 84 *Psychological Rev.* 191 (1977) (cognitive/behavioral theory); Eysenck, *A Unified Theory of Psychotherapy, Behavior Therapy and Spontaneous Remission*, 188 *Zeitschrift für Psychologie* 43 (1980) (classical conditioning theory). See also Bandura, *Self-Efficacy Mechanism in Human Agency*, 37 *Am. Psychologist* 122 (1982) (broad ramifications of theory of self-efficacy). One might claim that particular therapies would be more successful if they were matched with the appropriate cases. See S. Garfield, *Psychotherapy An Eclectic Approach* 225-37 (1980). But empirical evidence fails to support this view and, in any event, it would show only that a particular theory accounted for a restricted portion of reality.

<sup>102</sup> This is especially so because the validity of the Freudian corpus is hardly settled. Disciples and schismatics all derive their data from therapeutic consultations, which, I argued

question is whether the validity of the theory, as a scientific, causal theory, can be established by external, controlled, quantitative studies which, after all, would provide stronger direct verification than uncontrolled clinical evidence from the consulting room or studies of therapeutic outcomes.

Freud's writings (and those of most other psychodynamic clinicians) contain at least two major types of statements: observations of behavior and theoretical explanations of varying orders of inference.<sup>103</sup> Empirical observations include data such as slips of memory,<sup>104</sup> and the apparently regular concurrence of certain clusters of personality traits such as orderliness, obstinacy, and parsimony.<sup>105</sup> Empirical observations can be confirmed directly, but confirmation proves little, if anything, about the validity of an allegedly explanatory theory. Freud's theoretical explanations for his observations are the interesting and relatively unique aspects of his work. The most important types of causal, theoretical explanations for observed behavior include genetic (developmental) and dynamic processes.<sup>106</sup> For an instance of a genetic explanation, Freud

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at supra notes 84-90 and accompanying text, are a contaminated data source, much as were Freud's consultations. Therefore, in light of Freud's incomparable brilliance, there is little reason to test systematically the hypotheses of the offspring until the Freudian underbrush is cleared and pacified. The eminent British psychiatrist, Sir Aubrey Lewis, wrote that "the accessions to psychoanalytic theory since Freud's death have been of little weight in comparison with the colossal edifice of ideas and hypotheses that he reared." *The Later Papers of Sir Aubrey Lewis* 66 (1978) (also noting significant revisions).

The empirical literature testing analytic theory and studying psychotherapy is by now enormous, but it is almost totally ignored by most psychodynamic clinicians who are evidently content to remain sealed in their analytic cocoon. See supra note 86. Moreover, those in the analytic movement have failed to pay attention to advances in social science methodology that may have aided their endeavor. G. Klein, supra note 77, at 64 ("[I]s it not astonishing that, except for a few recent faint stirrings . . ., there has not been, since Freud's time, a *single* advance in the investigative or research methodology used by psychoanalysts?" (emphasis in original)).

<sup>103</sup> This, of course, is a vast oversimplification, but for the purposes of this paper it is a useful categorization. Psychodynamicists often confuse data and theory, and evidence and inference, much to the conceptual detriment of their enterprise. Esman, *On Evidence and Inference, On the Babel of Tongues*, 48 *Psychoanalytic Q.* 628 (1979) (examples given). Nevertheless, one can distinguish the two.

<sup>104</sup> S. Freud, *The Psychopathology of Everyday Life*, supra note 51.

<sup>105</sup> S. Freud, *Character and Anal Eroticism*, in 9 *Standard Edition* 169 (1908).

<sup>106</sup> In the most famous attempt to systematize the structure of psychoanalytic theory, David Rapaport described the genetic point of view as follows:

All behavior is part of a genetic series, and through its antecedents, part of the temporal sequences which brought about the present form of the personality . . . . [M]uch of what "exists" here and now in the subject can only be known through a

termed the personality triad of orderliness, obstinacy and parsimoniousness "anal" traits because, he hypothesized, they were acquired as a result of the conflict engendered by toilet training—the anal phase of psychosexual development—when the child wished to enjoy his or her feces and play with them, but was forced to renounce such wishes by parental pressure. As an example of a dynamic explanation, Freud attributed lapses of memory to the process of repression, whereby a particular recollection would be unconsciously prevented from reaching consciousness because, for any number of reasons, it would produce conflict and anxiety if the person were to remember it.

Genetic and dynamic hypotheses can be tested. Although it is difficult for even longitudinal, prospective research to confirm ge-

genetic exploration of its antecedents. This implies that descriptively identical behaviors may differ in their psychological significance, depending on their genetic roots.

D. Rapaport, *The Structure of Psychoanalytic Theory* 44-45 (Psychological Issues Monograph 6, 1960). Freud described the dynamic point of view as follows: "We seek not merely to describe and to classify phenomena, but to understand them as signs of an interplay of forces in the mind, as a manifestation of purposeful intentions working concurrently or in mutual opposition. We are concerned with a *dynamic view* of mental phenomena." S. Freud, *Introductory Lectures on Psychoanalysis*, supra note 42, at 67 (emphasis in original).

In addition to the genetic and dynamic points of view, psychoanalytic theory contains four other points of view. The *topographic* view hypothesizes that mental life occurs at different levels of consciousness. The *economic* point of view is concerned with the distribution and discharge of hypothesized psychic energy, e.g., libido. The *structural* point of view posits that the mind can be divided into three units — e.g., the id, ego, and superego — each of which has different functions. The *adaptive* point of view holds that behavior is determined in part by the person's external reality. See D. Rapaport, supra, at 57-61.

Freud posited many highly inferential explanatory constructs, such as psychic instincts (life and death), energies (e.g., libido), and psychic structures (id, ego, superego), to explain the processes that caused behavior. He termed such theorizing "metapsychology." The genesis of many of these constructs was Freud's background in the physical sciences and his desire to appear to have his psychology approach the precision of a physical science. Indeed, Freud believed chemistry and physiology ultimately would explain all his discoveries. See generally F. Sulloway, *Freud: Biologist of the Mind: Beyond The Psychoanalytic Legend* (1979) (interpreting Freud's work as an attempt to be a natural science); Bettelheim, supra note 42 (interpreting Freud's work not primarily as an attempt to be a natural science and demonstrating how mistranslation has led readers in English astray). Creative as these constructs are, however, many of them are not theoretically or scientifically sound — they are better understood as metaphors than as explanatory variables. See, e.g., Rubenstein, *The Problem of Confirmation in Clinical Psychoanalysis*, 28 *J. Am. Psychoanalytic A.* 397, 409 (1980) ("Freud's energetic hypotheses have been thoroughly discredited physiologically."). The main dispute among current mainstream Freudians is concerned with whether the metapsychology is a useful part of psychoanalytic theory. G. Klein, supra note 77, at 41-71; R. Schafer, supra note 64; Gill, supra note 42; Holt, *The Death and Transfiguration of Metapsychology*, 8 *Int'l Rev. Psycho-Analysis* 129 (1981).

netic personality explanations, well-performed studies nevertheless can furnish strong correlational evidence of an association between a developmental experience and a personality variable. For instance, if Freud was right about the roots of the anal personality, there should be major differences of the expected type between the toilet training experiences of "anals" and "non-anals." Indeed, if all other possible explanatory variables are distributed randomly among the anals and non-anals, one might cautiously conclude that toilet training experiences are a causal variable in the development of the anal personality.<sup>107</sup> Even so, such a study would not demonstrate that toilet training produced the personality trait constellation as an effect of the unconscious psychological dynamic processes that Freud suggested.

Dynamic explanations are equally interesting because they purport to explain how the person's mental processes that have been produced by his or her experiences in turn produce observable behavior. Such processes can be tested experimentally to determine if predicted behavioral consequences ensue. For instance, one can try to induce conflict and anxiety in one group of subjects by exposing them to those stimuli that the theory predicts will produce anxiety. Then one can determine if the anxious group, compared to a non-anxious control group, evidences significantly greater theoretically predicted defects in memory, attention, perception or the like. If the study is properly done, positive results will substantiate the theory in the absence of a better explanation for the results.

Although Freud never attempted to validate his theories by systematic, controlled tests, others have. Thousands of experimental and correlational studies of various aspects of psychodynamic theory have been performed outside the consultation room.<sup>108</sup> The now standard text for assessing the empirical evidence for dynamic theory is a critical compendium by two psychologists, Fisher and Greenberg, entitled *The Scientific Credibility of Freud's Theories*

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<sup>107</sup> Investigators cannot perform closely controlled experimental tests of genetic hypotheses because ethical principles and families will not allow experimenters to manipulate critical child-rearing practices to such an intrusive degree.

<sup>108</sup> A useful collection of these studies is *Freud and Psychology* (S. Lee & M. Herbert eds. 1970). A collection of some of the "strongest" such studies, each followed by highly critical commentaries, is H. Eysenck & G. Wilson, *supra* note 81. A useful primer on the scientific testing of psychodynamic theory is I. Sarnoff, *Testing Freudian Concepts* (1971).

*and Therapy*.<sup>109</sup> Although this work is highly valuable, it suffers from serious substantive and methodological constraints. First, despite the vast number of dynamic theorists and theories, Fisher and Greenberg reviewed only some aspects of one theory—Freud's.<sup>110</sup> Second, as is too often the case in behavioral science, the studies relied upon have rarely been replicated, and the studies of single topics are often not comparable to one another. Third, empirical tests of the validity of dynamic theories are prone to circularity.<sup>111</sup> For example, a test will be devised that is based on the very dynamic theory that is to be tested. If the results fail to disprove the theory—the null hypothesis is rejected—both the theory and the test are considered validated. But building validation into a test is a bootstrapping operation of the first order. Convincing validation requires tests whose foundation and prior validation are external to the theory.<sup>112</sup> Fourth, many of the tests used in the studies reported by Fisher and Greenberg do not have sufficient reliability and validity to meet modern, behavioral science standards for tests.<sup>113</sup> Fifth, many of the studies, especially the earlier ones, do not conform to present criteria for methodological soundness. Sixth and last, even when a reasonable study produces results

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<sup>109</sup> S. Fisher & R. Greenberg, *supra* note 77. Fisher and Greenberg attempted to review all the pre-1974 published and much of the unpublished (dissertation) empirical literature bearing on Freud's therapy and on five aspects of his therapy. These are (1) the dream theory; (2) personality types and oral and anal characters; (3) oedipal dynamics and consequences; (4) the origins of homosexuality; and (5) paranoid defensive projection of homosexual impulses. The book also reviews the empirical evidence concerning psychoanalytic therapy, but this topic has already been examined at *supra* notes 91-101 and accompanying text. (Fisher and Greenberg agree with the conclusions reached *supra*.) Another work that attempts a similar review is P. Kline, *Fact and Fantasy in Freudian Theory* (1972).

<sup>110</sup> As indicated at *supra* note 102 and accompanying text, the choice of Freud makes eminent sense—indeed, any other choice would be foolish: only Freud's theory, and only some aspects of it, have received reasonable amounts of empirical attention. It is remarkable that much of the rest of Freud's theory and all the other dynamic theories have not received substantial empirical testing. See *supra* note 77. Nevertheless, the Fisher and Greenberg review, extensive as it is, assesses only a limited portion of the enormous numbers of hypotheses that may be derived from the wide range of dynamic theories.

<sup>111</sup> Studies use such disparate subjects and methods that it is difficult to aggregate them to reach general conclusions. See Eysenck, *The Experimental Study of Freudian Concepts*, 25 *Bull. Brit. Psychological Soc'y* 261, 265-66 (1972) (outlining valuable methodological criticisms of empirical studies confirming psychodynamic theory and concluding that "there is no evidence at all for psychoanalytic theory").

<sup>112</sup> Eysenck, *supra* note 111, at 266.

<sup>113</sup> E.g., the Dynamic Personality Inventory. See Sells, *Dynamic Personality Inventory*, in *The Sixth Mental Measurements Yearbook* §§ 86-87 (O. Buros ed. 1965).

consonant with theory, behavioral scientists, like lawyers, need to apply Ockham's razor liberally or, as psychologists put it, the principle of parsimony. Behavioral scientists should try to determine if there are alternative explanations of the results that are simpler and less inferential.<sup>114</sup>

Fisher and Greenberg take note of some of these points, but they rarely give them their due. They do not consider the problem of aggregating studies in sophisticated detail, nor do they employ any of the reasonable methods of aggregation that methodologists have devised.<sup>115</sup> The problem of circular validation is ignored and alternative explanations are either not considered or are rejected a priori. The problem of methodological rigor is addressed but, in my opinion, is not sufficiently considered. The criterion for the inclusion of a study in the review is extremely loose and permissive.<sup>116</sup>

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<sup>114</sup> Eysenck, *supra* note 111, at 261-63.

<sup>115</sup> Light & Smith, *Accumulating Evidence: Procedures for Resolving Contradictions Among Different Research Studies*, 41 *Harv. Educ. Rev.* 429 (1971); Rosenthal, *Combining Results of Independent Studies*, 85 *Psychological Bull.* 185 (1978). See also Cooper & Rosenthal, *Statistical Versus Traditional Procedures for Summarizing Research Findings*, 87 *Psychological Bull.* 442 (1980).

In addition, Fisher and Greenberg use very vague terms to describe the strength of findings, e.g., "results demonstrate fairly convincingly," S. Fisher & R. Greenberg, *supra* note 77, at 148, or, "Freud's theory concerning this matter is moderately well affirmed," *id.* at 195.

<sup>116</sup> Fisher and Greenberg describe their methodology for reviewing and evaluating the literature on a topic as follows:

When we decided to collect and evaluate all of the existing scientific information bearing on Freud's models, we were confronted with having to set standards concerning the meaning of the term "scientific." We decided to apply the term to any study in which observations had been gathered according to rules more exacting than "This is what I personally witnessed or experienced." We will . . . cite information only when it has been secured through procedures that are repeatable and involve techniques that make it possible to check on the objectivity of the reporting observer. We did not decide in advance to rule out studies that had defects in their experimental designs or that were based on oversimplistic notions concerning Freud's models. It seemed more sensible to make a sweep of the total empirical data, flawed or otherwise, and to draw conclusions from overall trends.

S. Fisher & R. Greenberg, *supra* note 77, at 15.

Thus, Fisher and Greenberg do not give differential weight to studies of varying rigor, nor do they discuss the rigor of particular studies very often. They conclude that it is "sensible to make a sweep of the total empirical data, flawed or otherwise," but they do not provide reasons for why this is sensible. Their criteria for inclusion are very permissive, to say the least.

Developing and employing methods for aggregating data and reporting aggregated results precisely is admittedly difficult and time-consuming. Compare, for example, the methods used in *Benefits*, *supra* note 98, at 55-84, with the methodology of Fisher and Greenberg.

Finally, a unique point in behavioral science research should be noted. For various reasons, negative research results are published far less frequently than positive results, producing a bias in support of validity if one considers only published literature. Recognizing this, Fisher and Greenberg have commendably sought to remedy this bias by consulting unpublished dissertation literature, but for unavoidable reasons their search of this literature must be incomplete.<sup>117</sup> Although Fisher and Greenberg have produced an enormously useful work, it is nonetheless a review of limited aspects of one theorist that uses a permissive inclusion criterion. Its conclusions must therefore be viewed with caution.

With these methodological constraints and cautions in mind, one can draw three broad conclusions from the Fisher and Greenberg volume.<sup>118</sup> First, Freud was a fine, acute observer of behavior. Many of his observations of behavior and his associations between behaviors are reasonably valid. The anal personality constellation, for example, does seem to exist,<sup>119</sup> and anals appear generally to behave consistently as one predicts anals would (e.g., they are more likely to be stamp collectors than non-anals are).<sup>120</sup> Second, although studies appear to have validated a number of observational theorems, these studies have not confirmed a majority of the causal genetic and dynamic hypotheses, especially those pertaining to women.<sup>121</sup> And most of what has been confirmed has not been

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Although Fisher and Greenberg are more readable than *Benefits*, it is patent that one prefers the difficulty and rigor of *Benefits* when one is trying to reach convincing conclusions.

<sup>117</sup> For example, Fisher and Greenberg failed to discover my doctoral thesis, which systematically tested a number of crucial Freudian and post-Freudian hypotheses derived directly from the primary literature. See S. Morse, *Anxiety, Ego and Death* (1973) (unpublished doctoral thesis on file in Widener Library, Harvard University) (disconfirming almost all hypotheses tested). Fisher and Greenberg cannot be faulted for this failure — it would be impossible to identify every relevant thesis. Nevertheless, the omission indicates that there might be a great deal of negative evidence that Fisher and Greenberg did not canvass.

<sup>118</sup> Tests of Freudian hypotheses have mainly addressed observations and genetic and dynamic explanations. To review the extensive array of topics covered by Fisher and Greenberg would be beyond the purpose of this paper; therefore, only representative conclusions will be discussed.

<sup>119</sup> S. Fisher & R. Greenberg, *supra* note 77, at 142-45.

<sup>120</sup> *Id.* at 163.

<sup>121</sup> *Id.* at 394-95, 408-09. For example, the anal personality is probably not related to toilet training, *id.* at 147-49; it is unclear whether female paranoia is the result of a projective defense against unacceptable homosexual wishes, *id.* at 269, 409 (see also S. Freud, *A Case of Paranoia Running Counter to the Psycho-Analytic Theory of the Disease*, in *14 Standard Edition* 263 (1915)); the manifest content of a dream is not a "disguise" produced by de-



the uniquely Freudian developmental and genetic hypotheses.

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fenses against the unacceptable wishes of the latent dream, S. Fisher & R. Greenberg, *supra* note 77, at 394; love, not fear, is probably the major cause of the boy's oedipal identification with his father, *id.* at 395, 404-06. The very core process of repression itself, as Freud conceptualized it, has not been scientifically validated. Holmes, *Investigation of Repression: Differential Recall of Material Experimentally or Naturally Associated with Ego Threat*, 81 *Psychological Bull.* 632 (1974). Indeed, the logical and evidentiary basis for the concept in Freud's own work is problematic. Grünbaum, *Logical Foundations of Psychoanalytic Theory*, *forthcoming in Festschrift for Wolfgang Stegmüller* (W. Essler & H. Putnam eds. 1982).

Projection is another core concept not discussed by Fisher and Greenberg that has not been empirically confirmed as Freud identified it. Holmes, *Dimensions of Projection*, 69 *Psychological Bull.* 248 (1968); Holmes, *Projection as a Defense Mechanism*, 85 *Psychological Bull.* 677 (1978). Holmes' conclusions, however, have been criticized. See Sherwood, *Self-Serving Bias in Person Perception: A Re-examination of Projection as a Mechanism of Defense*, 90 *Psychological Bull.* 445 (1981). As Holmes' answer demonstrates, however, classical projection is still unconfirmed. Holmes, *Existence of Classical Projection and the Stress-Reducing Function of Attributive Projection: A Reply to Sherwood*, 90 *Psychological Bull.* 460 (1981). But see Sherwood, *Consciousness and Stress Reduction in Defensive Projection: A Reply to Holmes*, 91 *Psychological Bull.* 372, 373 (1982) (disagreeing with Holmes but admitting that the Freudian conceptualization is idiosyncratic, unsystematic, incomplete, and "inconsistent with contemporary thinking and unlikely to advance our understanding of the phenomenon, if it indeed exists").

More recent evidence on some of the genetic hypotheses—the family histories of male homosexuals, for example—has failed to confirm hypotheses that Fisher and Greenberg consider confirmed. See A. Bell, M. Weinberg & S. Hammersmith, *Sexual Preference: Its Development in Men and Women* 50, 61-62, 189-90 (1981); Zuger, *Homosexuality and Parental Guilt*, 137 *Brit. J. Psychiatry* 55 (1980).

The genetic (developmental) hypotheses of the theory, which are at the very core of psychoanalytic explanation, are now considered by many eminent observers to be incorrect. See, e.g., J. Kagan, R. Kearsley & P. Zelazo, *Infancy: Its Place in Human Development* 60 (1978) ("The original psychoanalytic variables of interest . . . are no longer of interest today. By the late 1940's . . . the research record was not in accord with prediction."); Bowlby, *supra* note 79, at 249 ("[A]ll of his [Freud's] developmental psychology came to be founded in principles long since abandoned by biologists. If . . . psychoanalysis is to become the natural science . . . that Freud intended, there are compelling reasons for drastic changes in some at least of its basic assumptions. . . . Although psychoanalysis is assuredly a developmental discipline, it is nowhere weaker . . . than in its concepts of development."). Even if psychoanalysts today would focus on different developmental explanations from those posited by Freud, the original Freudian hypotheses are still vastly influential, Bowlby, *supra* note 79, and to the best of my knowledge, newer developmental hypotheses, such as those of Kohut, have not been systematically tested. See *supra* note 102 and accompanying text. These conclusions do not contradict the assumption that parent-child relations are important in the child's development; they simply mean that psychodynamic theory does not offer a validated account of the influence of parent-child relations on development and adult behavior.

Many phenomena explained by Freud in his distinctive fashion have been explained according to other theories that seem more parsimonious and require fewer inferential, imaginative leaps. See, e.g., R. Nisbett & L. Ross, *Human Inference: Strategies and Shortcomings of Social Judgment* 228-48 (1980); S. Timpanaro, *The Freudian Slip: Psychoanalysis and Textual Criticism* (1976); Chapman & Foot, *The Psychology of Humour*, 4 *Trends in*

Third, where Freud's observations and explanations have been confirmed, they are only partial accounts of reality. Confirmation refers only to the weight of the evidence. Some studies of all propositions are negative, and even positive studies do not yield perfectly consistent results. For example, anal personality traits do not always cluster together; there are non-anals who behave in anal ways (e.g., collect stamps) and vice versa. Not all paranoids respond to homosexual stimuli. Persons with particular experiences and dynamics do not always behave in the predicted ways, while some persons who lack those experiences or dynamics do behave in those predicted ways. In sum, it must be acknowledged that Freudian observations are incomplete, and that Freudian causes are predisposing, not necessary or sufficient, causes of behavior. Moreover, the evidence from the pro-dynamic Fisher and Greenberg treatise does not strongly support Freudian theory.<sup>122</sup>

Unlike Freud, other dynamic psychologists have recognized the necessity to perform methodologically adequate studies of dynamic theory in an attempt both to rehabilitate and to validate it. The work of Lloyd Silverman, a leading spokesman for academic dynamic psychology, provides the best example. The work of Silverman and his students appears to provide two distinct types of confirming evidence. First, it purports to demonstrate that dynamic theory explains the success of one standard form of behavior therapy, systematic desensitization, better than the behavioral the-

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Neurosciences IV, V (1981); Erdelyi, *A New Look at the New Look: Perceptual Defense and Vigilance*, 81 *Psychological Rev.* 1 (1974); Norman, *Categorization of Action Slips*, 88 *Psychological Rev.* 1 (1981).

<sup>122</sup> Fisher and Greenberg are optimistic about the success of tests of Freudian hypotheses, but their optimism is difficult to fathom. Although a large number of studies are positive, many of the most important Freudian hypotheses are disconfirmed and few genetic and dynamic hypotheses — the core of the theory — receive substantial support. Others take an even dimmer view of the outcome of empirical tests of Freudian hypotheses. See, e.g., H. Eysenck & G. Wilson, *supra* note 81, at 392 (the studies most strongly "confirming" Freudian hypotheses "give little if any support to Freudian concepts and theories"); Crews, *Analysis Terminable, Commentary*, July, 1980, at 25, 28 ("[M]uch of psychoanalytic thought is by now a palimpsest of hazy, mutually jostling notions, not one of which has been shown by an adequately designed empirical study to be the most likely explanation of a given phenomenon."). Crews also believes that even if one accepts in total the claims of Fisher and Greenberg, only "marginal credence" would be lent to the theory. *Id.* at 28 n.11. See generally Slater, *The Psychiatrist in Search of a Science: III—The Depth Psychologies*, 126 *Brit. J. Psychiatry* 205 (1975) (denying the scientific validity of psychoanalysis). But see Kelk, *Is Psychoanalysis A Science? A Reply to Slater*, 130 *Brit. J. Psychiatry* 105 (1977) (asserting the continued validity of Freudian hypotheses).

ory upon which the treatment is based, and that an unusual form of therapy derived from particular psychodynamic hypotheses is successful in treating a wide range of problems.<sup>123</sup> Second, Silverman and others have conducted numerous experimental studies of dynamic processes that attempt to enhance psychopathology, many of which appear to confirm psychodynamic theory.<sup>124</sup> More recent work, however, has severely undermined the optimistic conclusions that dynamicists would wish to draw from Silverman's work. One investigator has shown that Silverman's proffered psychodynamic explanation for the success of systematic desensitization is vitiated by multiple methodological flaws.<sup>125</sup> Another behavioral scientist has tried rigorously in three recent separate experiments to replicate Silverman's experimental findings on dynamics.<sup>126</sup> When the first test failed, the experimenter twice

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<sup>123</sup> E.g., Silverman, *Psychoanalytic Theory: "The Reports of My Death are Greatly Exaggerated,"* 31 *Am. Psychologist* 621, 629-31 (1976); Silverman, Frank & Dachinger, *A Psychoanalytic Reinterpretation of Systematic Desensitization: Experimental Data Bearing on the Role of Merging Fantasies,* 83 *J. Abnormal Psychology* 313 (1974). Systematic desensitization is a treatment wherein a patient is first taught to relax and is then exposed to a series of graded stimuli, which increasingly provoke anxiety. As each stimulus is presented and the patient feels anxious, he or she is instructed to relax. This procedure is repeated until that stimulus is presented and the patient feels no anxiety. Then, the next, more anxiety-producing stimulus is presented and the procedure is repeated. The process continues until the last stimulus, typically the real cause of the patient's concern, such as a fear of flying, is mastered. The classic text on this method is J. Wolpe, *Psychotherapy by Reciprocal Inhibition* (1958).

The unusual therapeutic intervention used by Silverman is to present the subject with the subliminal written message, "Mommy and I are one." This message appears to be successful in ameliorating a range of problems, including schizophrenia.

<sup>124</sup> E.g., Silverman, *supra* note 123, at 626-29; Silverman, Ross, Adler & Lustig, *Simple Research Paradigm for Demonstrating Subliminal Psychodynamic Activation: Effects of Oedipal Stimuli on Dart-Throwing Accuracy in College Males,* 87 *J. Abnormal Psychology* 341 (1978).

<sup>125</sup> Condon & Allen, *Role of Psychoanalytic Merging Fantasies in Systematic Desensitization: A Rigorous Methodological Examination,* 89 *J. Abnormal Psychology* 437 (1980). But see Palmatier & Bornstein, *Effects of Subliminal Stimulation of Symbiotic Merging Fantasies on Behavioral Treatment of Smokers,* 168 *J. Nervous & Mental Disease* 715 (1980) (Silverman's theory apparently confirmed, but test is of a trivial behavior). Two other recent studies appear to demonstrate the efficacy of subliminal messages in reducing psychopathology, but some results were surprising or not predicted by Silverman's earlier work. See Fribourg, *The Effect of Fantasies of Merging with a Good Mother on Schizophrenic Pathology,* 169 *J. Nervous & Mental Disease* 337 (1981); Mendelsohn, *The Effects of Stimulating Symbiotic Fantasies on Manifest Pathology in Schizophrenics: A Revised Formulation,* 169 *J. Nervous & Mental Disease* 580 (1981).

<sup>126</sup> Heilbrun, *Silverman's Subliminal Psychodynamic Activation: A Failure to Replicate,* 89 *J. Abnormal Psychology* 560 (1980) (also citing other failures to replicate).

modified the experimental design, each time making it *easier* to replicate Silverman. Both further attempts failed.<sup>127</sup>

Generally, then, empirical, external investigation has failed to confirm substantially, and in many cases to confirm at all, the validity of Freudian theory.<sup>128</sup> Nevertheless, there are myriad other psychodynamic theories, both Freudian and non-Freudian, in use today by mental health clinicians. Is there reason to believe that some or all of them might be more valid than their Freudian an-

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<sup>127</sup> Id. Silverman has recently commented on the studies by Condon and Allen, *supra* note 125, and Heilbrun, *supra* note 126. See Silverman, A Comment on Two Subliminal Psychodynamic Activation Studies, 91 *J. Abnormal Psychology* 126 (1982). The responses to Silverman's comment are Allen & Condon, Whither Psychodynamic Activation? A Reply to Silverman, 91 *J. Abnormal Psychology* 131 (1982), and Heilbrun, Reply to Silverman, 91 *J. Abnormal Psychology* 134 (1982). See also Silverman, Rejoinder to Allen & Condon's and Heilbrun's Replies, 91 *J. Abnormal Psychology* 136 (1982). For a recent, popular review of Silverman's work, see Adams, Mommy and I Are One, *Psychology Today*, May 1982, at 24. Silverman's work has been positively replicated in about three-quarters of the studies using his methods, many of which were conducted by him and his students. There is no reason to doubt this work, but in light of the failure consistently to replicate the work, it is not clear what conclusions can be drawn from it. I believe Silverman's work is the most interesting and encouraging attempt to validate core psychoanalytic hypotheses, but the results are not obviously predicted by psychoanalytic theory, and indeed they may even be psychodynamically counterintuitive. Something is happening in these studies, but it is not clear what. Silverman's work is not generally accepted yet, but even if it should be, it does not validate the extraordinarily broad range of psychodynamic hypotheses that are used to explain behavior. For other recent empirical investigations by dynamically oriented researchers, see Spence & Gordon, Activation and Assessment of an Early Oral Fantasy: An Exploratory Study, in 8 *Psychological Issues* 11-28 (M. Mayman ed. 1973) [hereinafter cited as *Psychological Issues*]; Luborsky, Forgetting and Remembering (Momentary Forgetting) During Psychotherapy: A New Sample, in 8 *Psychological Issues*, *supra*, at 29-55; Shevrin, Brain Wave Correlates of Subliminal Stimulation, Unconscious Attention, Primary and Secondary-Process Thinking, and Repressiveness, in 8 *Psychological Issues*, *supra*, at 56-87. Cogent and compelling criticisms of these studies are made by Holzman, Some Difficulties in the Way of Psychoanalytic Research: A Survey and a Critique, in 8 *Psychological Issues*, *supra*, at 88-103. See generally Meehl, Some Methodological Reflections on the Difficulties of Psychoanalytic Research, in 8 *Psychological Issues*, *supra*, at 104-17. For more recent evidence confirming observations, see Greenberg & Fisher, Freud's Penis-Baby Equation: Explanatory Tests of a Controversial Theory, 53 *Brit. J. Med. Psychology* 333 (1980); Masling, O'Neill & Katkin, Autonomic Arousal, Interpersonal Climate, and Orality, 42 *J. Personality & Soc. Psychology* 529 (1982).

<sup>128</sup> That hypotheses derived from Freud's theory should fail to receive substantial confirmation is not surprising given the method of his investigation. Freud consistently constructed post hoc explanations of the antecedents of behavior based on the material that his adult patients provided in the consulting room. Freud never really tested these constructions, however. A brilliant exposition of the difficulties and contradictions that Freud's method produced is Jacobsen & Steele, From Present to Past: Freudian Archaeology, 6 *Int'l Rev. Psycho-Analysis* 349 (1979).

cestor? It is possible that the first dynamicist, Freud, although admittedly a genius, made only a hesitant and often wrong beginning. This is unlikely, however. Freud's powers as an intellectual and theoretician far surpassed those of his contemporaries and successors; and, unlike the situation in most sciences, there have not been validated advances in the instrumentation and methodology of psychodynamic psychology. The therapist-observer and the consulting room couch are still the tools of discovery and theoretical advance. Simply put, it seems fair to conclude that, in general, the theories of other dynamic psychologists are no more valid than those of Freud.<sup>129</sup> In any case, the burden of persuasion should be on the dynamic psychologists to demonstrate by acceptable scientific methods that their alternatives to and emendations of Freud are valid.

The conclusions drawn from the evidence concerning the scientific validity of dynamic psychological theory are these: (1) the theory cannot be tested properly by the patient's responses to therapeutic interventions in the consulting room; (2) the outcome of psychodynamic therapy suggests that it is not uniquely successful and that the theoretically posited variables are not the agents of the therapeutic change that does occur; (3) external, empirical investigations have produced, at best, only equivocal and pallid confirmations of Freud's theory; (4) alternative theories often can explain the results of studies supportive of Freudian theory, and, in any case, psychodynamic theory is only a partial account of reality even in those areas where it may be valid; and (5) a great proportion of Freud's theories, and almost all of the theories of those who followed him, have never been tested by reasonably scientific means.<sup>130</sup>

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<sup>129</sup> This is not to deny that some theorists have usefully amended the Freudian corpus. For instance, Hartmann's attention to the real world, although put too abstractly, was a useful corrective. See H. Hartmann, *Ego Psychology and the Problem of Adaptation* (1958).

<sup>130</sup> A caveat to these conclusions is necessary. Even though a great part of dynamic psychology as a scientific, causal account of behavior is not valid, *descriptively* unconscious determinants do affect behavior. To reiterate a distinction explained in *supra* note 42, *descriptively* unconscious determinants have not been forced into unconsciousness because they are too painful to recognize—they are simply not in awareness. Much behavior can be explained only if one posits such variables. See R. Nisbett & L. Ross, *supra* note 121; Shevrin & Dickman, *The Psychological Unconscious: A Necessary Assumption for All Psychological Theory?*, 35 *Am. Psychologist* 421 (1980). The issue then is the validity of psychodynamic accounts of why mental contents are unconscious and how they affect behavior.

As a purportedly scientific causal account of human behavior, dynamic theory has had nearly a century to prove itself, and in large part it has failed to do so. Given its broad claims and comprehensive nature, the empirical support psychodynamic theory has received simply is not sufficient to support the conclusion that it is generally valid: other theories explain the most supportive studies, and many core concepts have been disconfirmed. Psychoanalysis has always had its critics, especially in academic psychology, but today, as a scientific psychology, it is not only losing support in its traditional home in psychiatry but is also beset by critics from within.<sup>131</sup> Moreover, attempts to employ dynamic psychology to explain behavior in other fields such as history and literature are receiving increasing criticism or are being abandoned.<sup>132</sup>

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Other theories and models, concerned with information-processing in general, explain the data more parsimoniously and satisfyingly. E.g., R. Nisbett & L. Ross, *supra* note 121; S. Timpanaro, *supra* note 121; Norman, *supra* note 121. With such models, there is no need for recourse to the many layers of inference required by dynamic theory and one is not forced to confront and then rationalize the large body of negative evidence about dynamic propositions that has accumulated.

<sup>131</sup> Frank, *Two Theories or One? or None?*, 27 *J. Am. Psychoanalytic A.* 169 (1979).

An important indication is that psychodynamic concepts were not influential in the development of the American Psychiatric Association's new Diagnostic and Statistical Manual of Mental Disorders (3d ed. 1980) [hereinafter cited as DSM-III]. See Spitzer & Williams, *Classifications of Mental Disorders and DSM-III*, in 1 CTP-III, *supra* note 1, at 1035, 1053, 1055 (system is atheoretical, and neither text nor diagnostic criteria contain psychodynamic material, despite disapproval by psychoanalysts).

Dynamic psychology has rarely found a comfortable niche in American academic psychology because Freudian theory is seen as unscientific, as so often vague and reliant on unobservable variables, and because the evidence for it is weak. See MacKinnon & Duke, *supra* note 61, at 670-73 (also noting some attention paid to psychoanalysis but little scientific impact). See generally D. Shakow & D. Rapaport, *The Influence of Freud on American Psychology*, in 4 *Psychological Issues*, *supra* note 127 (1967). Some percentage of academic and practicing clinical psychologists are dynamically oriented, but dynamic psychology is increasingly removed from the mainstream of scientific psychology. Among clinical psychologists, only 19% now consider themselves dynamically oriented, compared to 41% according to a 1961 survey. Garfield & Kurtz, *Clinical Psychologists in the 1970's*, 31 *Am. Psychologist* 1 (1976). See generally Garfield, *Psychotherapy: A 40-Year Appraisal*, 36 *Am. Psychologist* 174 (1981).

<sup>132</sup> J. Barzun, *Clio and the Doctors* (1974); F. Crews, *Out of My System: Psychoanalysis, Ideology, and Critical Method* (1975); D. Stannard, *Shrinking History: Freud and the Failure of Psychohistory* (1980). On the other hand, psychoanalysis seems to have taken on new life in France. S. Turkle, *Psychoanalytic Politics: Freud's French Revolution* (1978); French Freud: *Structural Studies in Psychoanalysis*, in 48 *Yale French Studies* (1972). I leave it to the intrepid reader to consult some of the primary French sources in psychoanalysis to determine whether this result is a benefit to intellectual life.

The law, too, should reject all, or at least most, of psychodynamic theory as a scientific, causal account of behavior. Michael Moore has shown convincingly that dynamic theory is not motivational in character, in that it does not provide reasons for behavior that rationalize that behavior and causally explain it.<sup>133</sup> Nor, as we have seen, is it likely that dynamic wishes and other processes and mental events are causes of behavior in the mechanical or functional sense.

All of us want explanations for behavior that seems to make no sense. Psychodynamic psychology will continue to be seductive to legal factfinders (who are not dismayed by its fanciful excesses) because it offers a story about an actor's behavior, a story that is wrapped in the mantles of medicine, psychology, and science, and that seems to explain the actor's behavior. One wants to believe the story because no other theory offers such an apparently full, causal account. Indeed, one might argue that psychoanalysis is a good "social hermeneutic" because it tells compelling stories of social dis-eases:<sup>134</sup> psychodynamic tales allow us to accept that some persons act in unacceptably antisocial ways for reasons beyond their control and not because of moral weakness, moral failure, or the like. Psychodynamic psychology portrays aberrant conduct as the product of inexorable, dark, primitive, unconscious forces. Thus, the wrongdoer is not like the rest of us, whose conflicts are of a lesser order and a milder nature. Finally, psychodynamic stories help us rationalize our desire to be merciful.

But the law should resist the sirens of dynamic psychology. Therapeutic meaningfulness is not the law's concern, and we must confront directly the hard social choices involved in excusing a criminal defendant or mitigating punishment. If we wish to hear tales about why people behave cruelly, evilly, or monstrously, let us do so honestly and have them told by novelists and poets. The difficulty with having dynamic clinicians tell these tales is that the tales are then enshrouded in the white coats of science and

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<sup>133</sup> Moore, *The Nature of Psychoanalytic Explanation*, 3 *Psychoanalysis & Contemp. Thought* 459, 537 (1980). Accord Eagle, *A Critical Examination of Motivational Explanation in Psychoanalysis*, 3 *Psychoanalysis & Contemp. Thought* 329, 362-64 (1980). For a different account of the nature of psychoanalytic explanation also rooted in the philosophy of science, see M. Sherwood, *The Logic of Explanation in Psychoanalysis* (1969).

<sup>134</sup> Christopher Stone and other readers of this article have suggested that this is the primary function of dynamically-oriented expert testimony.

medicine. Rather than understanding them as attempts to interpret the awful aberrance of which humans are sometimes capable, the law mistakenly accepts the tales as scientifically-based causal accounts of behavior. As a consequence, the law then misunderstands and misapplies the tales. But the tales should not be told in courts at all—not by doctors or by novelists. The integrity of the criminal trial process is too important to permit it to be compromised by the admission of dynamic speculations.

The significance of the evidence is clear: psychodynamic psychology in general is not sufficiently scientifically verified for the law to accept it as relevant in the criminal process.<sup>135</sup> Speculations

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<sup>135</sup> Dynamic psychology is best understood, I believe, as a method of interpreting and giving meaning to behavior, rather than as a mechanistic, causal explanation of it. See P. Ricoeur, *Freud and Philosophy: An Essay on Interpretation* (1970); Steele, *Psychoanalysis and Hermeneutics*, 6 *Int'l Rev. Psycho-Analysis* 389 (1979). Cf. M. Sherwood, *supra* note 133, at 244-57 (noting the narrative nature of psychoanalytic explanations and suggesting criteria for evaluating the adequacy of such explanations). Freud rejected this account of dynamic psychology: although the process of interpretation was crucial for Freud, the purpose of interpretation was not simply to provide meaning; rather, the provision of meaning was hypothesized to cause specific modifications in the hypothesized unconscious causal mechanisms that primarily determine behavior. See S. Freud, *The Question of Lay Analysis*, *supra* note 57; S. Freud, *An Outline of Psycho-Analysis*, in 23 *Standard Edition* 144, 179 (1940). Nevertheless, the author of a theory is not the exclusive arbiter of its validity or usefulness. Professor Moore, too, has argued that treating psychodynamic psychology as simply an interpretive system, much like literary criticism, trivializes the theory. Moore, *Psychoanalytic Explanation*, *supra* note 133, at 529. But this is true only if one insists on maintaining that psychodynamic psychology is a causal, scientific account of behavior. Even if it is not a causal account, however, the provision of meaning can be crucial in peoples' lives.

It is often said, unfairly, that psychoanalysis is the modern secular religion and that analysts are the latter day priests. Dynamic therapists are probably able to impart meaning authoritatively to skeptical modern patients because the therapists are trained in psychiatry or psychology, which are, in a sense, modern religions. For millenia, religions have had the vitally important effects of comforting people and of giving an account of and imparting meaning to their lives. This has been true even though the spiritual doctrines of the world's religions are not scientifically verifiable. Dynamic therapists give meaning to the lives of patients, and meaning counts.

This is a reasonable point at which to confess that when I don my clinical hat, I am a psychodynamically-oriented clinician. I no longer believe, however, that dynamic theory is a causal account of behavior. Rather, I view it, as I have suggested in this article, as an interpretive story that provides meaning and may therefore be comforting to persons. I also believe that traditional psychoanalysis is never indicated as a psychotherapeutic measure, but that it may be useful for aspiring dynamic clinicians as a means of "scrubbing" their unconscious. Finally, I also confess to having written years ago theoretical and clinical psychoanalytic articles: Morse, *Perfecting the Parents: A Family Romance Resistance*, 27 *Am. J. Psychotherapy* 410 (1973); Morse, *The After-Pleasure of Suicide*, 46 *Brit. J. Medical Psychology* 227 (1973); Morse, *Structure and Reconstruction: A Critical Comparison of Michael*



based on psychodynamic psychology, judged as scientific causal accounts of behavior, are not "informed" but idle. At most, only speculations based on the few verified propositions should be admitted into evidence. That a significant number of mental health clinicians accept psychodynamic psychology does not in any way prove its validity. The burden of persuasion should be on proponents of dynamic psychology to demonstrate that it is more verified than I claim. The criminal law should demand more than mere acceptance; it should require a reasonably solid scientific foundation before admitting the testimony of dynamicists. It is currently impossible to lay this foundation, for instead of concrete there is only faith.

*C. Are Psychodynamic Formulations Reliable? A Difficulty in the Path of Expert Testimony*

Even if one concludes that some or all of psychodynamic theory is sufficiently valid as a causal account of behavior to support expert testimony, the question remains whether clinicians can give accurate and reliable explanations in individual cases. If they cannot, such clinical testimony does not aid rational decisionmaking by the factfinder.

A good dynamicist collects data about a person primarily by extensive interviewing. The dynamicist typically takes a comprehensive history of the person and gives the person enormous freedom to talk about whatever he or she wishes. From the person's own historical account and from the person's behavior during the clinical interviews, the dynamicist draws inferences about the person's developmental processes, personality structure, and dynamics. The dynamicist constructs speculations about the person's unconscious infantile impulses, wishes, conflicts, and feelings, and about the person's characteristic unconscious means of denying and expressing those impulses, wishes, conflicts, and feelings. The dynamicist does not deny the influence on behavior of biological and sociological variables or conscious thoughts and feelings, but the primary explanatory tool is unconscious motivation.<sup>136</sup> Thus, it is dynamically unconscious motivation that the psychodynamic cli-

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Balint and D.W. Winnicott, 53 *Int'l J. Psycho-Analysis* 487 (1972); Morse, *The Psychological Theory of Michael Balint*, 3 *Psychiatry in Medicine* 407 (1972).

<sup>136</sup> See S. Freud, *Analysis Terminable and Interminable*, *supra* note 57, at 219-30, 234-40.

nician seeks to understand and, in therapy, to elucidate. The dynamicist applies the same process of explanation and understanding in terms of unconscious variables to both life patterns and individual events. Indeed, understanding an individual event is impossible without understanding the person's general personality structure and dynamics.

Let us take a simple example: an employee is late for an appointment with a superior. The employee explains truthfully that he or she was so engrossed in prior activities that the time was forgotten. This is an ordinary enough occurrence that most persons would understand and attribute it to inadvertence. By contrast, the dynamicist would assume that the person unconsciously wished to be late and that the tardiness was not a matter of inadvertence. It would be assumed further that becoming engrossed in prior activities was a means the employee unconsciously employed to provide a conscious excuse. But why did the employee wish to be late, and why was the wish so unacceptable that the employee was unconsciously unwilling to become aware of it? Based on an understanding of the employee's usual dynamics and the associations to the particular incident, the dynamicist might conclude that the cause of the tardiness was hostility toward the superior, or the masochistic desire to be chastised by the superior, or some other irrational, unconscious reason or combination of reasons. Furthermore, recognition of these irrational wishes is psychologically threatening; consequently they cannot be allowed into consciousness. At a deeper level yet, the dynamicist would seek the root of the wish in the employee's childhood relations with other significant authority figures, such as parents. The dynamicist might explain the employee's seemingly nonsensical and unjustified unconscious hostility towards the superior by hypothesizing that the employee unconsciously reacted to the superior as if the superior were the employee's parent. The employee unconsciously behaved towards the superior in the passive-aggressive, hostile manner that, as a child, the employee unconsciously used to express hostility toward his parents. Such hostility would have been a source of great unconscious anxiety for the child for many reasons—"what if my parents retaliate or stop loving me," and "children who feel hostility towards parents are bad." The hostility would therefore be defended against and kept from consciousness in many ways. Moreover, the resulting punishment would unconsciously be desired to

expiate the guilt engendered by having bad feelings.<sup>137</sup>

Note the extraordinary level of complexity that even this oversimplified dynamic story gives to an ordinary incident. At the first level, the dynamicist interprets the incident in terms of an unacceptable wish. Then the clinician hypothesizes about the substance of that irrational wish. And finally, a genetic explanation is given for the activation of the wish and the conflict-based reaction to it in that particular context. This is what is known as a psychodynamic formulation of the incident. It does not purport to be solely an interpretation of the meaning of the incident; rather, it is offered as a causal account, with the causal chain beginning in childhood, mediated by an adult (wish-conflict-anxiety-defense) dynamic, and ending in an expressive behavioral effect—tardiness. Note, too, how much more interesting the dynamic story makes this otherwise pedestrian incident.

When a psychodynamic clinician proffers testimony either to help the factfinder understand why a criminal defendant lacked self-control or to try to convince the factfinder that the defendant did not have the requisite *mens rea* at the time of the charged offense, the clinician will offer a formulation of the defendant's psychodynamics at the time of the offense and relate it to a conclusion about control capacity or *mens rea*. This testimony will be of no aid, however, if the formulation provided is not an accurate, causal account of the defendant's behavior.

As should be clear from the example given above, it is impossible to verify an individual formulation objectively: there are not, for example, substantiated and accepted physiological or psychological criteria to judge the accuracy of the clinician's hypothetical formulation. Dynamicists claim, however, that the person's reaction to the formulation, when the person receives it as an interpretation, is the standard means of verifying the accuracy of a formulation/interpretation. If the person reacts to the interpretation with a loosening of affect, a stream of confirming associations, or even by an

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<sup>137</sup> Dynamic theory also predicts that the patient will act out his or her personality dynamics with the therapist, toward whom the patient unconsciously transfers the feelings he or she has toward significant figures from childhood, especially parents and siblings. Indeed, much of clinical psychoanalysis is devoted to analysis of the "transference," the recreation with the therapist of the patient's characteristic and unconsciously mediated behavior patterns. See generally R. Greenson, *The Technique and Practice of Psychoanalysis* 151-356 (1967).

“Aha, so that’s it” response, then the interpretation is deemed accurate.<sup>138</sup> This method is unavailing for several reasons, however. First, criminal defendants being evaluated are not the patients of their evaluator. There is no therapeutic contract—defendants are simply the subjects of a psychological investigation. Consequently, they are not being offered interpretations on a regular basis, if at all, and the usual set of reactions are unavailable as a source of validation. More important, even if dynamic psychology is generally valid, it cannot be tested on the couch. The defendant’s reactions cannot serve to validate formulations. Even a criminal being evaluated will often desire to please the expert; or the expert, who does not have the temporal luxury of doing psychoanalysis, will intervene actively to hasten the evaluation and, by so doing, will shape the communications produced. Finally, there is simply no persuasive objective evidence that the subject’s reactions validate an interpretation, and there is much reason to believe that they do not.<sup>139</sup> At present, therefore, there is no external source to test the validity of an individual formulation.

The only remaining means to assess the *probable* validity of a formulation is to examine its reliability. Reliability here means the degree to which independent observers agree about the categorization of a phenomenon.<sup>140</sup> One may ask whether a dynamic formulation is reliable in the following specific ways: What is the likelihood that two or more dynamicists interviewing the person separately will independently construct the same formulation? Or, what is the likelihood that they will independently construct the same formulation based on the same interview (conducted either by one of them, both of them, or a third interviewer)? If dynamic science is generally valid, and experienced practitioners can inde-

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<sup>138</sup> See S. Freud, *Constructions in Analysis*, *supra* note 57, at 261-65.

<sup>139</sup> See *supra* notes 88-90 and accompanying text.

<sup>140</sup> Reliability generally refers to the degree to which a test or measurement system produces the same response under similar conditions. W. Mischel, *Personality and Assessment* 13 (1968). There are many types of reliability. See L. Cronbach, *Essentials of Psychological Testing* 156-73 (3d ed. 1970); C. Selltiz, L. Wrightsman & S. Cook, *Research Methods in Social Relations* 181-94 (3d ed. 1976). In the current “testing” of psychodynamic hypotheses, however, virtually the only “measuring instrument” is the clinician-observer applying dynamic theory. Thus, the primary type of reliability measurement is to determine if independent clinician-observers agree on the formulation of the case. This is also the primary method for measuring the reliability of psychiatric diagnoses. R. Kendell, *The Role of Diagnosis in Psychiatry* 29-30 (1975). See also sources listed in *infra* note 141.

pendently agree on a formulation, then one has some indirect evidence for the accuracy of the formulation.<sup>141</sup>

Because there are no acceptable criteria for determining the validity of a formulation, at the very least one would expect—indeed, one should require—that proponents of psychodynamic theory would perform reliability studies of psychodynamic formulations carefully and constantly. But they have not done so. The very few studies of the reliability of dynamic formulations that exist are mostly impressionistic and suggest that these formulations are unreliable. Independent dynamicists do not agree on the explanations of individual cases.<sup>142</sup> Thus, if a dynamicist offers a formulation at

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<sup>141</sup> The degree of agreement can be expressed numerically as a reliability coefficient. See L. Cronbach, *supra* note 140; House, House & Campbell, *Measures of Interobserver Agreement: Calculation Formulas and Distribution Effects*, 3 *J. Behav'l Assessment* 37 (1981); Janes, *Agreement Measurement and the Judgment Process*, 167 *J. Nervous & Mental Disease* 343 (1979). Modern studies of the reliability of psychiatric diagnosis typically use a reliability statistic called Kappa (K), which corrects for the amount of chance agreement that may be expected between judges. R. Kendall, *supra* note 140, at 36; Spitzer & Williams, *supra* note 131, at 1042. Although there are no universally agreed-upon levels of acceptable reliability, see Grove, Andreasen, McDonald-Scott, Keller & Shapiro, *Reliability Studies of Psychiatric Diagnosis: Theory and Practice*, 38 *Archives Gen. Psychiatry* 408, 412 (1981), in clinical psychiatry, diagnostic reliability of .80 is considered "quite good" and .50-.60 is considered "at least fair." See Spitzer & Williams, *supra* note 131, at 1061. An eminent psychometrician has suggested, however, that in applied settings, the reliability of a measure may make a great deal of difference and therefore higher reliability — .90 as a minimum — should be required. J. Nunnally, *Psychometric Theory* 245-46 (2d ed. 1978). See generally Grove, Andreasen, McDonald-Scott, Keller & Shapiro, *supra*.

<sup>142</sup> Marmor, *Validation of Psychoanalytic Techniques*, 3 *J. Am. Psychoanalytic A.* 496, 500, 502-04 (1955); Seitz, *The Consensus Problem in Psychoanalytic Research*, in *Methods of Research in Psychotherapy* 209, 215 (L. Gottschalk & A. Auerbach eds. 1966). Recent reviewers once again concluded that "in the few formal studies . . . [citing Seitz] of this 'consensus issue,' the results have been most discouraging." Silverman & Wolitzky, *supra* note 77, at 325. See also Mischel, *On the Empirical Dilemmas of Psychodynamic Approaches: Issues and Alternatives*, 82 *J. Abnormal Psychology* 335, 337 (1973) (dynamically-based clinical judgments do not predict behavior well, nor do they enable clinicians to tailor treatments to the individual client's needs and circumstances, because clinicians cannot identify inferred generalized cross-situational predispositions).

Seitz's review revealed only two unpublished studies, including his own. Seitz's study, carried out by senior analysts at the Chicago Psychoanalytic Institute, is a fascinating report of disagreements about interpretations and interpretive processes, and of the squabbles and narcissistic investments in the interpretations offered by the individual analysts. There were heated disagreements, and finally the group disbanded because of an "inability to make progress in developing a reliable interpretive method, i.e., a method that would yield greater consensus among a group of analysts in making independent formulations of the same case material." Seitz, *supra*, at 210. If such eminently qualified analysts from the same training institute cannot achieve reliable formulations, there is no reason to believe the average forensic mental health professional will do so. See also S. Fisher & R. Greenberg, *supra* note

a professional meeting, in a courtroom, or in a consulting room, it is virtually certain that another colleague would have explained the case differently on the basis of the same data.<sup>143</sup> As an intuitive matter, it would be extraordinary to claim that all are correct causal accounts. The measuring instrument of psychodynamic psychology—the clinician-observer applying his or her theory—is simply unreliable.

There are several explanations for the unreliability of dynamic formulations. First, there are many theoretically divergent schools of dynamic psychology. There is no reason to expect the formulation of a Jungian to sound anything like the formulation of a Freudian, or the formulation of a Sullivanian to sound like that of an Adlerian. Even within a particular school, there can be major theoretical differences. A Kohutian-Freudian, for example, will not sound like a Brenner-Freudian.<sup>144</sup> Second, the process of investi-

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77, at 294-97 (describing other relevant studies).

In the last decade, some researchers have attempted to measure the reliability of judgments of various aspects of the transference during psychoanalysis, using selected portions of transcripts and rating scales of one to five to rate the degree to which the aspects of transference were present. These investigations report that statistically significant, but poor, reliability was achieved. Luborsky, Graff, Pulver & Curtis, *A Clinical-Quantitative Examination of Consensus on the Concept of Transference*, 29 *Archives Gen. Psychiatry* 69 (1973). See also Graff & Luborsky, *Long-Term Trends in Transference and Resistance: A Report on a Quantitative-Analytic Method Applied to Four Psychoanalyses*, 25 *J. Am. Psychoanalytic A.* 471 (1977) (no reliability determination, but investigators assumed that the same reliability obtained as in the earlier study because the same judges were used). These studies are a far cry, however, from studies of the reliability of formulations or interpretations, which are the core process of psychodynamic clinical work. In another study, judges were able to make reliable estimations from psychotherapy process notes, of which themes, previously identified, had been warded off in therapy. Horowitz, Sampson, Siegelman, Wolfson & Weiss, *On the Identification of Warded-Off Mental Contents: An Empirical and Methodological Contribution*, 84 *J. Abnormal Psychology*, 545, 549-50 (1975). Again, however, this finding is quite different from discovering high reliability about formulations.

<sup>143</sup> Of course, if the patient had been interviewed by a different clinician, different data would have been elicited. Marmor, *supra* note 142, at 504. Even if the same data are used, however, differences would be great.

<sup>144</sup> See, e.g., Unger, *A Program for Late Twentieth-Century Psychiatry*, 139 *Am. J. Psychiatry* 155, 158 (1982):

The great scandal in the use of psychological models—Freudian or not—in contemporary psychiatry is what I have called their indeterminacy. By indeterminacy I mean the overabundance of plausible but only ambiguously successful responses to the same explanatory or therapeutic problems. There are just too many alternative explanations and treatments based on too many incompatible pictures of what is in fact the case. The variety of meaningful interpretations in turn puts pressure against the diagnostic categories. It makes them seem more or less arbitrary.

gating an individual's psychodynamics and constructing a formulation is entirely subjective and based on exceedingly complex inference chains for which no decision rules exist and no external checks are available.<sup>145</sup> Two interviewers examining the same patient will elicit very different types of reactions and information.<sup>146</sup> Even if the clinicians constructing the formulation use a single data base, obviating subjectively caused differences in the production of the data base, the inferential process of constructing the formulation will still eliminate reliability.<sup>147</sup> When a clinician uses loose, vague criteria, unchecked by external validity constraints, and when the clinician's individual training, experience, and personality affect the application of those criteria, it is hardly remarkable that the highly inferential process of dynamic reasoning will lead to markedly different formulations.<sup>148</sup>

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Cf. McIntosh, *supra* note 70, at 408 (psychoanalytic theory has no predictive force because there are no nomic universals and hence no scientific laws in the theory).

A brilliant evocation of the internecine disputes of the psychoanalytic schools may be found in J. Malcolm, *Psychoanalysis: The Impossible Profession* (1981), which describes the present workings of the psychoanalytic profession by focusing on one analyst in New York City. One of the most bizarre aspects of the book is that its protagonist, Dr. Aaron Green (a pseudonym), retained a debilitating symptom that interfered with his career after two psychoanalyses that had lasted a total of 14 years!

<sup>145</sup> D. Rapaport, *supra* note 106, at 113 ("The extensive clinical evidence . . . fails to be conclusive in terms of the usual criteria of science, because there is no established *canon for the interpretation of clinical observations* (emphasis in original)). See Glover, *supra* note 84, at 405-07; Ramzy, *How the Mind of the Psychoanalyst Works: An Essay on Psychoanalytic Inference*, 55 *Int'l J. Psycho-Analysis* 543, 543 (1974) ("Unbelievable as it may sound, in the whole vast library of psychoanalysis — clinical, theoretical, technical or applied — there are hardly any references which outline the logical guidelines or the methodological rules which the analyst follows in order to understand his patient."). See generally Ramzy & Shevrin, *The Nature of the Inference Process in Psychoanalytic Interpretation: A Critical Review of the Literature*, 57 *Int'l J. Psycho-Analysis* 151 (1976).

<sup>146</sup> Marmor, *supra* note 142, at 504. See Bowlby, *supra* note 79, at 252 ("[W]hat an analyst subsequently reports his patient to have said [is] probably influenced as much or more by the analyst's preconceptions as by anything the patient may in fact have said or done.").

<sup>147</sup> Moreover, from the time of Freud to the present, therapists describing cases wittingly or unwittingly elaborate the material to fit their own preconceptions. M. Sherwood, *supra* note 133, at 71; S. Fisher & R. Greenberg, *supra* note 77, at 276; Glover, *supra* note 84, at 407.

<sup>148</sup> Assume now, however, that, contrary to the claims of its proponents (and those who argue that it is relevant to the ascription of responsibility), dynamic psychology does not offer causal accounts of behavior and assume instead that it offers an account of the meaning of behavior. See *supra* notes 70-74 and accompanying text. If the interpretation of psychodynamic psychology as a hermeneutic is correct, then the lack of reliability of formulations is neither surprising nor troubling. One does not expect the interpretations of a literary work by different critics to be the same, even if the critics were trained in the same

It might be objected that the argument about verifying the accuracy of a formulation proves too much. Establishing states of mind is central to assessments of culpability, and if this cannot be accomplished reliably then the bedrock of guilt determination is blasted away. There is a major distinction, however, between assessing a defendant's conscious states of mind (such as purpose, or knowledge or awareness of risk), and assessing a defendant's unconscious dynamics. All persons can verify by ordinary experience that conscious mental states such as purpose exist, and we can often infer easily and correctly whether in particular circumstances these mental states were present in others by examining those circumstances, including the actor's utterances and actions. There is, however, no correspondingly easy access to dynamically unconscious processes, and there are no generally available and acceptable means for inferring their presence or absence in others. Evaluating ordinary mental states is not simple, but it is within the realm of ordinary experience; evaluating unconscious dynamic processes is entirely within the realm of speculation.<sup>149</sup>

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academic department and according to the canons of a particular theory of criticism. Moreover, the validity or accuracy of an interpretation is not assessed in the same fashion as the solution to a mathematical problem or other empirical question.

In large measure, interpretations are judged by how satisfying and internally coherent they are, by whether they "work" by helping us to understand the artistic creation under consideration. This, I suggest, is directly analogous to the process of psychodynamic formulation and interpretation. Formulations and interpretations are neither ultimately right nor wrong: either they work to help the patient and the therapist create a coherent story about the patient's life, or they do not work. Whether that story is an accurate causal account is irrelevant, and, as has been recognized for a long time, an inexact interpretation may be therapeutic. Glover, *The Therapeutic Effect of Inexact Interpretation: A Contribution to the Theory of Suggestion*, 12 *Int'l J. Psycho-Analysis* 397 (1933). Thus, Freudian stories work for Freudian patients, Jungian stories work for Jungian patients, and the new criticism works in many departments of English. Stories that work are satisfying, and vice versa.

I am not suggesting that, among psychologies, dynamic psychology is the best interpretive system, or that all interpretive systems are equally valuable. Indeed, Professor Frederick Crews suggests that dynamic theory may be pernicious because it is particularly guilt-inducing and because its therapy is unjustifiably expensive and time-consuming. Nevertheless, all the dynamic theories and therapies do appear to be able to impart meaning.

<sup>149</sup> A similar point can be raised about assessing the influence of drugs on behavior. Bonnie and Slobogin try valiantly to rehabilitate their unverifiable speculations based on dynamic psychology by comparing them to opinions about the effects on behavior of alcohol and other drugs. They are surely correct when they propose that retrospective inquiries about the effects of drugs on mental states in criminal cases are important because drugs affect behavior, and they are right to suggest that such inquiries will usually lead to imprecise conclusions. Nevertheless, they are far wide of the mark in their claim that retrospective knowledge of the effects of drugs is no more imprecise than retrospective psychody-



Dynamic formulations present a dilemma to the factfinder. At present, no acceptable means exist to externally verify an individual formulation, and it is entirely likely that any particular formulation will be unreliable. An unseemly battle of the experts is all but inevitable, for neither the proponent nor opponent of a psychoanalytic formulation can point to external validity criteria to buttress the accuracy of a causal account of the defendant's behavior. Psychodynamic formulations are post hoc interpretive rationalizations of behavior, not explanations of it. In sum, a psychodynamic formulation is an unverifiable and unreliable causal account of an individual case; it provides the factfinder with little more than a false sense of security based on the incorrect assumption that a reasonably accurate scientific explanation has been provided. Psychodynamic formulations are so inherently unreliable that they cannot aid decisionmaking in the criminal justice system. They should not be admitted at trials, at sentencing hearings, or at any other stage of the criminal process.<sup>150</sup> Cross-examination will not be an effective tool for exposing the inaccuracy of psychodynamic formulations because factfinders will have no means to resolve disputes. Moreover, the unseemly battle of the experts which will result will be costly, confusing, and inefficient.

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dynamic formulations. The existence of chemicals as variables that cause changes in behavior is at least verifiable, in complete contradistinction to the existence of psychodynamic variables. Moreover, countless rigorous studies have examined the effects of recreational and therapeutic drugs on behavior, and even more extensive clinical experience has added to the store of knowledge about drug effects. See generally E. Abel, *Drugs & Behavior: A Primer in Neuropsychopharmacology* (1974); E. Brecher, *Licit and Illicit Drugs* (1972); R. Julien, *A Primer of Drug Action* (2d ed. 1978). Finally, most adults in our culture have had some experience with one drug or another. Indeed, experience with alcohol is so common in our culture that intoxication is one subject on which lay witnesses are usually allowed to offer an opinion. Lilly, *An Introduction to the Law of Evidence* § 29, at 85, 87 (1978). Even if factfinders are faced with an imprecise inquiry about drug effects, there is simply infinitely more hard data and common wisdom with which to assess such claims compared to the causal claims of psychodynamic psychology. At least we know beyond a reasonable doubt that drugs exist and do affect behavior.

<sup>150</sup> It is interesting to consider why psychodynamic psychology retains its appeal in the face of disconfirming evidence. Let me offer a few speculations. First, if one treats the theory as an interpretive rather than causal account, disconfirming evidence is not problematic. Second, psychodynamic theory is fascinating and comprehensive and no other equally fascinating, comprehensive theory has appeared to replace it. Third, causal explanations developed to explain data seem to persevere in the face of discrediting information. See Anderson, Lepper & Ross, *Perseverance of Social Theories: The Role of Explanation in the Persistence of Discredited Information*, 39 *J. Personality & Social Psychology* 1037, 1043-48 (1980).

*D. Are Psychodynamic Formulations Relevant to the Establishment of Responsibility in Criminal Proceedings?*

If dynamic formulations aid criminal justice decisionmaking, they are most relevant to assessing a defendant's capacity for behavioral control or criminal responsibility in general;<sup>151</sup> psychodynamic explanations are rarely used to buttress a claim that a defendant lacked a requisite conscious mens rea. Instead, the usual thrust of arguments by proponents of dynamic explanations is to establish that a defendant's unconscious conflicts and motives eroded to a legally significant degree the accused's ability to control the offending behavior.<sup>152</sup> Proponents conclude or imply that, compared to the average criminal defendant, a defendant who is the innocent victim of uncontrollable unconscious forces is less responsible, less blameworthy, and less deserving of punishment.<sup>153</sup> There is no dispute in principle about the relevance of lack of control or capacity for rationality — if a defendant is truly unable to exercise control or to behave rationally, the law should mitigate or excuse his or her responsibility. The question is whether unconscious motivation in fact vitiates control, rationality, and responsibility.<sup>154</sup>

Legal proponents of the mitigating effects of unconscious motivation suppose that a defendant whose behavior meets all the prescribed elements of a crime, including the requisite mens rea, is less guilty because unconscious motives were one cause of that behavior. They rarely analyze the issue, however.<sup>155</sup> They simply as-

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<sup>151</sup> Kadish and Paulsen refer to this as general mens rea. S. Kadish & M. Paulsen, *Criminal Law and Its Processes* 87-88 (3d ed. 1975). The notion is that, because it is unfair to punish someone for behavior that was beyond his or her control, the law requires general mens rea as a precondition for punishment. Cf. H. Hart, *Punishment and Responsibility* 28, 152-56, 227-30 (1968).

<sup>152</sup> See Bonnie & Slobogin, *supra* note 8, at 435-52. A criterion such as "aberrant" may be used to distinguish persons who may pose such claims. See *infra* notes 163-65 and accompanying text.

<sup>153</sup> See Fingarette, *Victimization as Excuse* 16-17 (1981) (unpublished manuscript on file with the Virginia Law Review Association).

<sup>154</sup> Assume, for the purpose of discussing this issue, that the first two steps of my argument are rejected—that psychodynamic psychology is sufficiently valid to be an acceptable basis for expert testimony and that psychodynamic formulations are sufficiently reliable in individual cases to permit their admission into evidence.

<sup>155</sup> As we shall see, Bonnie and Slobogin fall into this category. Philosophers, however, have addressed the issue. See Alexander, *Rational Behavior and Psychoanalytic Explanation*, 71 *Mind* 326 (1962); Audi, *Psychoanalytic Explanation and the Concept of Rational*

sume that the unconscious motives produced by childhood conflicts and trauma are causes, and that because the actor is the innocent victim of those causes and is unaware of them, they compromise self-control or rationality to a legally cognizable degree.<sup>156</sup> It is implied that unconscious causes can be analogized to cases of compulsion or force majeure, where the actor literally is forced to perform the offending action or where the action is the only reasonable thing to do under the circumstances. Or it is implied that unconscious motives can be analogized to some type of brain injury that renders the actor incapable of behaving rationally. These analogies in turn imply that the law should not consider an actor who is under the sway of unconscious causes to be responsible for his or her actions. But unconscious causes (or any other types of causes) are relevant to responsibility only if they produce a legal excuse such as irrationality, lack of mens rea, or compulsion. If a defendant meets at a conscious level the criteria for rationality and mens rea, the actor is not less responsible for his behavior simply because it is possible to identify an unconscious determinant or any other cause for that behavior. To conclude otherwise is to confuse nonresponsibility with causation.

In a recent series of illuminating and complex articles that employ the tools of the modern analytic philosophy of action, Michael Moore has argued persuasively that unconscious motivation almost never vitiates responsibility for actors who otherwise meet the criteria for criminal guilt.<sup>157</sup> A demonstration that behavior has causes does not per se undermine responsibility, because causation

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Action, 56 *The Monist* 444 (1972); Blumenfeld, Free Action and Unconscious Motivation, 56 *The Monist* 426 (1972); Mullane, Psychoanalytic Explanation and Rationality, 68 *J. Phil.* 413 (1971).

<sup>156</sup> In dynamic therapy, the goal is to make the actor aware of the unconscious, adult residues of childhood experience. Conflicts that unconsciously terrified the child and were repressed, leading to deformations of behavior, do not terrify the adult who can counteract the effects of these conflicts once they are made conscious. Freud put it as follows:

In the end, if the situation of the repression can be successfully reproduced in his memory, his compliance [with the rule of free association] will be brilliantly rewarded. The whole difference between his age then and now works in his favour; and the thing from which his childish ego fled in terror will often seem to his adult and strengthened ego no more than child's play.

S. Freud, *The Question of Lay Analysis*, supra note 45, at 205.

<sup>157</sup> Moore, Responsibility and the Unconscious, 53 *S. Cal. L. Rev.* 1563 (1980) [hereinafter cited as *Responsibility and the Unconscious*]; Moore, Responsibility for Unconsciously Motivated Actions, 2 *Int'l J. L. & Psychiatry* 323 (1979).

is not equivalent to, or proof of, a legal excuse. Moore and other philosophers argue that the language and concepts of responsibility analysis and causation analysis are derived from entirely different categories. Responsibility language and concepts refer to persons acting for reasons; causation language and concepts refer to things happening because of antecedent events. The criteria for legal responsibility are simply that the actor is rational in the sense that he or she acts for reasons that fit a practical syllogism, and that the actor meets the particular legal requirements of liability. Thus, a reasonably rational defendant who performs the *actus reus* with the requisite *mens rea* is responsible for the crime. The language of the *prima facie* case in criminal law is one of actors acting for reasons — causation is usually irrelevant. It is possible to redefine all behavior, including the formation of *mens rea*, as events or effects. As such, all have sufficient antecedent causes — physiological, sociological, psychodynamic, and so on. With such a redefinition, however, one is no longer talking the language of persons, reasons, choices, and responsibility. Instead, one is talking about persons and their behavior as objects and events. The two realms of discourse should not be confused, because if causation is equated with excuse, it leads to the reductionist conclusion that no one is responsible—presumably, all behavior has causes.

Unless one is prepared to accept hard determinism and recognize its incompatibility with responsibility, those who wish to use dynamic explanations as excuses must demonstrate more. Simply because there is an unconscious cause of behavior does not mean that it is compelled or that the actor is irrational. Proponents of such excuses must prove that, because of unconscious motivation, the actor's otherwise seemingly rational action was irrational or the actor's choice whether to obey the law was too hard.<sup>158</sup> The task for responsibility skeptics is to show systematically how unconscious motivation produces excusing conditions, and to formulate criteria for determining which unconsciously motivated actors the law should excuse. After all, as Moore has shown, unconscious causes

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<sup>158</sup> Moore suggests, for example, that if an actor holds an unconscious belief that constrains his choices, and *if* it is possible to discover such beliefs, perhaps one would conclude that the actor's choice was too hard. Moore, *Responsibility and the Unconscious*, *supra* note 157, at 1665-66, 1669-70.

do not negate conscious rationality<sup>159</sup> or mens rea,<sup>160</sup> nor does their existence necessarily mean that the person's actions are compelled.

One need not, of course, accept the approach outlined above. Philosophically impure common sense consistently rejects the philosophically pure view by assuming that the behavior of all persons is subject to various causes and that these causes vary in their salience and strength. "Selective determinism"<sup>161</sup> assesses responsibility differentially according to the types and strengths of the pressures that operate on an actor, and it assumes that certain causes vitiate responsibility, even if the causes are not linked coherently to legal excuses. The corollary of selective determinism is selective responsibility skepticism. For example, although the behavior of both the slum kid and the rich kid is caused by something, one assumes that the rich kid is better able to avoid criminal behavior than the slum kid. Some persons then claim that the slum kid is less culpable and perhaps even less legally responsible, because the pressures on him to violate the law are greater than those on his more fortunate counterpart.<sup>162</sup> Bonnie and Slobogin are evidently selective responsibility skeptics and claim that the law should excuse only some persons because their criminal behavior was produced by unconscious determinants. Although this equation of causation with compulsion is muddled, it is the commonsense view. Assuming *arguendo* that selective compulsion is reasonably coherent, the burden is on responsibility skeptics to suggest and justify criteria for the law to employ in excusing some persons because of unconscious motivation.

Aberration is apparently one criterion that might be used to identify those who should be excused,<sup>163</sup> but this criterion is unacceptable. If only aberrant defendants can raise creditable claims for mitigation on the basis of unconscious determinants, a workable definition of aberrance is absolutely necessary. It is all but im-

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<sup>159</sup> *Id.* at 1659-63.

<sup>160</sup> *Id.* at 1649-54.

<sup>161</sup> See Hollander, *Sociology, Selective Determinism, and the Rise of Expectations*, 8 *Am. Sociologist* 147 (1973).

<sup>162</sup> Bazelon, *The Morality of the Criminal Law*, 49 *S. Cal. L. Rev.* 385 (1976). See Haney, *Psychology and Legal Change: On the Limits of a Factual Jurisprudence*, 4 *L. & Human Behav.* 147, 172 (1980) (claiming that there are "mountains of data" to disprove the free will hypothesis). By this, Professor Haney must mean that at present there is greater evidence of environmental causation in some cases than in others. Presumably, all behavior is caused.

<sup>163</sup> See Bonnie & Slobogin, *supra* note 8, at 452, 466.

possible, however, to specify for measurement the criterion of aberrance.<sup>164</sup> Does "aberrance" refer to the defendant's conscious thoughts and feelings and observable actions, or does it refer to the content and dynamics of the unconscious, or both? What is the definition of "aberrant" — statistical aberration? dysfunction? suffering? craziness?<sup>165</sup> What are aberrant unconscious contents or dynamics?

Even if there are identifiable criteria for aberrance, it is not possible to make out a scientific or philosophically sensible claim for selective compulsion. If dynamically unconscious factors cause overt behavior, criminal or otherwise, one must ask whether these causes are "compelling" or overwhelming. Data from studies supportive of psychodynamic propositions and clinical wisdom demonstrate that unconscious dynamic variables are neither necessary nor sufficient causes of any particular behavioral effects.<sup>166</sup> Even if particular unconscious dynamic variables are significantly and causally associated with particular behaviors, the behavior will occur in some cases without the antecedent operations of those variables, and in other cases those variables will operate without producing the effects. At most, particular unconscious dynamics are predisposing causes of varying strength.<sup>167</sup>

For instance, suppose a dynamicist explains a homicide in the following manner. The dynamicist assesses the defendant, post hoc of course, as a dependent person who fears for his manhood and has fragile ego integration. The homicide occurs in response to a stress—a physical beating by a man who is a rival for the woman they both love—a stress that creates enormous anxiety and allegedly activates all the accused's unconscious dynamic difficulties.<sup>168</sup> It is claimed that in order (unconsciously) to assert his manhood, to guarantee his unique access to the loved one, and to ward off ego disintegration, the defendant kills the rival. The defendant reports that consciously he was simply furious at the rival and

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<sup>164</sup> Bonnie and Slobogin, for example, never do so. Moreover, they have not provided a theoretical rationale for why aberrance, however understood and defined, raises a claim for legal excuse based on psychodynamic grounds.

<sup>165</sup> See Morse, *supra* note 2, at 546-54 (discussing the various meanings of aberrance).

<sup>166</sup> See *supra* text at notes 108-22.

<sup>167</sup> For a general discussion of the types of causes identified in the behavioral and medical sciences, see Morse, *supra* note 2, at 564-66.

<sup>168</sup> This hypothetical is based on *State v. Sikora*, 44 N.J. 453, 210 A.2d 193 (1965).

wanted to pay him back for the beating. We know that many people kill who are not under the influence of this defendant's dynamics, including some people who are exposed to similar types of external stress, and that most persons with this defendant's dynamics do not kill, even if they are exposed to the same or similar stresses. Indeed, the vast majority of people with similar dynamics (or, for that matter, with any dynamic constellation) do not kill at all (or offend the law in other nontrivial ways). This defendant's dynamics, even when coupled with the stresses in question, are not a necessary or a sufficient cause of homicide. If the defendant's dynamics are a cause at all, they are only weakly predisposing in the statistical sense.

The implication of the assertion that unconscious dynamic variables are only weakly predisposing causes of behavior is that such causes do not seem entitled to great weight as factors that "compel" criminal behavior. One might counter this implication by arguing that statistical associations are misleading because they are based on the grouping together of cases that are significantly different. For instance, one might allege that the hypothetical defendant considered above should not be included in a class of persons whose dynamic contents are similar, because to do so ignores the comparative strength of our defendant's difficulties.<sup>169</sup> This defendant's conflicts presumably were unusually strong, and he was extraordinarily upset and unbalanced by the stress he underwent. In sum, the defendant's case is unique if it is understood in sufficient detail. This post hoc argument is simple but unacceptable. Explanations of individual differences that adduce the variances in the strengths of unmeasurable and unverifiable underlying dynamic variables are circular: *A* kills and *B* does not because *A*'s (similar) dynamics were greater; we know this because *A* killed and *B* did not, even though their dynamics were similar. Moreover, it is impossible to refute such explanations, because the crucial explanatory variable — strength of conflict — is at present unmeasurable and unverifiable except through the use of unacceptable tautological reasoning.<sup>170</sup>

Uniqueness explanations are generally unverifiable and prove

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<sup>169</sup> This involves the economic point of view in dynamic theory. See *supra* note 106.

<sup>170</sup> See Kubie, *The Fallacious Use of Quantitative Concepts in Dynamic Psychology*, 16 *Psychoanalytic Q.* 507 (1947).

too much. It is possible to argue that cases differ not only in terms of the strength of unconscious determinants, but also in terms of other dynamics that dynamicists have not yet understood or identified. If such other factors are beyond present understanding or measurement, however, it is unavailingly speculative to employ them to explain behavior. In addition, if one digs deeply enough into any case and identifies enough background factors, in principle every case will be unique and contain sufficiently powerful determinants to vitiate responsibility. Thus, arguments about uniqueness lead inexorably to the reductionist conclusion that no one is responsible — a conclusion that creates a more general problem, as we shall see below.<sup>171</sup>

Even if research demonstrates that in some cases unconscious determinants are a strongly predisposing cause of criminal behavior, this does not compel the conclusion that the defendant's responsibility is lessened or that he or she should be punished less or not at all. There are other very strongly predisposing causes of criminal behavior — poverty, for example — that most observers do not think should diminish or negate a defendant's responsibility. The dynamicists must therefore show that unconscious determinants deserve special status as causes that uniquely affect responsibility. Although the legal votaries of dynamicism do not make such a showing, the argument would proceed in the following fashion. Full responsibility requires the capacity for a reasonable degree of rationality and self-control. These capacities in turn require that the actor be reasonably capable of perceiving and weighing the data relevant to the choice to behave, including unconscious cravings and affects. Although a background of poverty may restrict choices enormously, making criminal behavior a more prominent option, it supposedly does not affect the capability for rational assessment and control.<sup>172</sup> By contrast, dynamically unconscious determinants allegedly affect precisely the capacity for

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<sup>171</sup> See *infra* notes 187-89 and accompanying text.

<sup>172</sup> If a particular defendant could demonstrate that his or her subcultural background affected the capacity for rational assessment, however, such a case would raise interesting philosophical and legal problems. Irrationality produced by mental disorder excuses in extreme cases. Should a similar excuse exist where the actor seems incapable of making reasonable choices, according to dominant community standards of rationality, where subcultural socialization was the cause of the lack of rationality? See generally Bazelon, *supra* note 162.



rational assessment and control of behavioral choices. Even if the defendant is able to express a conscious and explicable motive for his behavior, as did our hypothetical homicide defendant discussed above, the defendant is not aware of crucial variables — strongly predisposing, irrational, unconscious determinants — that in large measure motivate the behavior. Moreover, the defendant cannot become aware of this motivational data by paying more careful attention to his or her thoughts, feelings, goals and desires: these dynamically unconscious data are inaccessible because of developmental processes and events that have made them so emotionally painful that they have been unconsciously forced out of awareness. The defendant can only recover such data by entering psychotherapy or by extraordinary feats of self-insight that are beyond the powers of the average person. The defendant who is motivated largely by unconscious determinants therefore is incapable of behaving rationally and exercising self-control, even if he or she appears rational, and thus it would be unfair to hold the defendant fully responsible for criminal conduct.

Assuming that these factual premises are valid, the argument raises squarely the issue of deciding in which cases unconscious determinants are so predisposing to criminal behavior that the law should diminish the defendant's responsibility. Those who argue for a diminution in responsibility have the burden of providing criteria for assessing the strength of predisposition, and for developing normative justifications for the decision that the actor's unconscious determinants, rather than his or her character and conscious choices, were "really responsible" for the actions in question. This they have not done, except, perhaps, by reference to the unacceptable aberrance criterion discussed above.<sup>173</sup>

To help elucidate the difficulties involved, consider the case of a male mugger who always chooses older women to rob, and who often seriously injures his victim during the attack for reasons not apparently connected with his larcenous intent. When asked why he chooses older women as victims, he replies that they are easy,

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<sup>173</sup> See *supra* notes 163-65 and accompanying text. Indeed, a leading psychoanalytic theoretician claims that it cannot be done because psychoanalysis has no clear and coherent theory of action in general and of the role of unconscious determinants in causing action. D. Shapiro, *Autonomy and Rigid Character* 8-11, 21 (1981) (rejecting the model of behavior that sees persons as irresistibly driven by unconscious determinants).

defenseless targets, and, when asked why he senselessly harms them, he replies that he "gets a kick out of it." Here is a perpetrator who obviously knows exactly what he is doing and does so intentionally for explicable if execrable motives. At an ethical level, one could simply assess the defendant as a moral monster, punish him as harshly as the law allows — he is both evil and dangerous — and leave it at that. Many laypersons might say that this person has a "thing" against older women, but they would also say, "So what—he is a bad actor." A dynamicist who interviews the defendant might offer the following woeful tale and formulation. The defendant was abandoned by his parents at an early age and raised by an elderly aunt who was both seductive and emotionally distant from him, and who beat him. This upbringing left the defendant feeling furious and needy. Moreover, at an unconscious level, the defendant wished to take revenge against his "mother," in part by obtaining the "supplies" (money = love = milk) that he so desperately desires. Still, the defendant robbed and then needlessly and brutally harmed innocent and defenseless older women. Moreover, he knew fully that in our society such behavior is not considered nice.

Despite the strong "push" exerted by the defendant's unconscious dynamics, it is impossible to determine if he could have altered his behavior. Even though his choices were constrained to some degree because he was unconsciously attempting to achieve irrational goals, perhaps he could have said to himself, "I want to harm those women, but it is wrong and unnecessary, and I simply will not do it." We can try to assess his general control structures and functioning, but ultimately we will have to guess about the strength of the predisposing, unconscious cause of the desire to harm older women.<sup>174</sup> There is simply no scientifically reliable and

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<sup>174</sup> It is by no means clear that the mugger would not have committed the crime if by chance he had previously been to a psychodynamic therapist and had understood his irrational unconscious motivation for harming older women. If he had committed the muggings despite having apparently obtained insight, three interpretations would be possible: (1) the behavior was freely chosen now and he is fully responsible for it; (2) he had never integrated the insight emotionally so the behavior was still compelled (Intellectual insight is considered a necessary but not sufficient precondition for behavioral change. The patient must emotionally feel and understand the insight in order for it to be efficacious: the person must consciously experience at a noncognitive level the effect of the conflict. This is theory and has not been confirmed, but it is an article of faith among dynamic practitioners.); or, (3) other hypothetical unconscious determinants, not yet discovered, were the real cause. Be-

valid way to make such a determination. The burden should be on the proponents of dynamicism to demonstrate that dynamic explanations can be used to assess responsibility in more than an utterly vague and discretionary fashion.

The strongest conceptual objection to the use of psychodynamic explanations to reduce responsibility is that these explanations lead to the conclusion that no one is responsible. Psychodynamic psychology is deterministic; it posits that behavior, like all other phenomena of nature, is determined by natural laws.<sup>175</sup> A dynamicist may therefore take one of three positions on the relationship between the causation of behavior and responsibility: that determinism and responsibility are irreconcilable (the "incompatibilist" position), that they are reconcilable (the "compatibilist" position), or that the principle of psychic determinism should be abandoned. If the incompatibilist position<sup>176</sup> is adopted, the dynamicist must consider the concept of responsibility to be an "as if" myth that is at present absolutely necessary for social cohesion.<sup>177</sup> Consequently, while recognizing that no person has a "real" choice about how he behaves, such a dynamicist will wish to excuse only those who are obviously insane, subject to duress, or the like. There is no coherent argument that an incompatibilist dynamicist can make for selective diminution of responsibility so long as the *mens rea* for a crime is present and all the extreme excusing conditions are absent.<sup>178</sup>

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cause there is no scientifically valid means of choosing among the three alternatives, any choice must be either conclusory or tautological.

<sup>175</sup> See Basch, *Psychic Determinism and Freedom of Will*, 5 *Int'l Rev. Psycho-Analysis* 257 (1978). Insight might make persons feel better, and, indeed, it might even give a patient the illusion of freedom; nevertheless, all behavior is determined. *Id.*

<sup>176</sup> A useful introductory review of the determinism and freedom or responsibility issue is L. Davis, *Theory of Action* 107-41 (1979).

<sup>177</sup> F. Alexander & H. Staub, *The Criminal, The Judge, and The Public: A Psychological Analysis* 59-65 (rev. ed. 1956); K. Menninger, *The Crime of Punishment* 96-97 (1968); Haney, *supra* note 162, at 172. See also Wolf, *The Importance of Free Will*, 90 *Mind* 386, 393 (1981).

<sup>178</sup> It would be possible in theory to develop a sliding scale model of responsibility based on the hard choices experienced by the actor. As the choice to obey the law becomes harder, the actor is less responsible. See generally Perkins, *Impelled Perpetration Restated*, 33 *Hastings L.J.* 403 (1981) (proposing a general doctrine of excuse for conduct deemed "impelled" because the actor consciously faced a hard choice). Note, however, that such a scheme is not based on causation; presumably all actions are caused. The basis of mitigation or excuse is that the actor consciously experienced a hard choice as in cases of duress. The difficulty in developing such a scheme is discussed *infra* notes 182-86 and accompanying text.

Alternatively, the dynamicist may adopt the compatibilist position, as do many modern philosophers.<sup>179</sup> He or she will argue that determinism and responsibility are reconcilable because each concept belongs in a different conceptual category, and that comparing them commits the familiar Rylean category mistake.<sup>180</sup> The consequences of adopting the compatibilist position, however, are the same as those described above in the discussion of Moore's work:<sup>181</sup> so long as the actor is capable of being reasonably rational at the conscious level, and no standard excusing condition is present, unconscious motivation does not vitiate responsibility.

The third possibility for the dynamicist is to abandon the general underlying postulate of psychic determinism in favor of selective compulsion.<sup>182</sup> Although this violates a fundamental canon of dynamic psychology, it does avoid the pitfalls in responsibility assessment just outlined.<sup>183</sup> But there are worse pitfalls ahead. The "selective compulsivist" chooses cases that he believes demonstrate both the existence of strongly predisposing unconscious determinants of behavior and the relevance of such determinants to responsibility assessment.<sup>184</sup> The insuperable difficulty is that a good dynamicist can create a story of unconscious compulsion to make any case fit whatever criteria are created for diminishing responsibility. The dynamicist can always "show" that for any significant action there was some deep and irrational unconscious motivation that "explains" the action. Even if behavior seems totally rational and explicable, a dynamicist can give an irrational, strongly disposing dynamic explanation for it.

Consider, for example, an unskilled, uneducated, unemployed

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<sup>179</sup> See, e.g., A. Kenny, *Freewill and Responsibility* 22-45 (1978).

<sup>180</sup> G. Ryle, *The Concept of Mind* 15-23 (1949). A category mistake is the allocation of a familiar concept in abstract thinking to logical types to which they do not belong. See *id.* at 17.

<sup>181</sup> See *supra* notes 157-58 and accompanying text.

<sup>182</sup> This appears to be the move made by Bonnie and Slobogin.

<sup>183</sup> Without assuming psychic determinism, the dynamicist who conceives of dynamic psychology as a mechanistic, causal account of behavior cannot make *causal* sense of the flow of free associations—the fundamental data source for dynamic theory and assessment.

<sup>184</sup> This is true although psychodynamic theory does not offer a coherent account of action and will. See *supra* note 173. Although Bonnie and Slobogin have not offered workable criteria for the selection of compelled cases, assume that, in principle, reasonable criteria can be developed. Note, however, that an enormous number of assumptions about the Bonnie and Slobogin position have to be made in order to set forth a reasonable case, and most of these assumptions are almost certainly not viable.

man who embarks on a series of larcenous burglaries. Asked why he engaged in a criminal career rather than accepting welfare, he explains that he can make much more money from burglary. He knows from street information that his chance of getting caught is exceedingly slim and his chance of going to prison if caught is slimmer still. This is a classic case of rational crime, and there should be no obstacle whatever to holding him fully responsible. Dynamic votaries presumably would be loathe in such a clear case to offer a mitigating explanation—there seems to be no indication of aberrance, however defined—but the reason for this hesitancy is elusive. A sophisticated and sympathetic dynamicist could easily “show,” for example, that the passive acceptance of aid from authority figures (i.e., welfare) was unacceptable at a deep unconscious level and that burglary satisfied some equally deep unconscious psychological needs (e.g., breaking and entering a house = oedipal rape of the mother).<sup>185</sup> The selective compulsivist will therefore claim that the defendant was not “really” rational, or that the crimes were the products primarily of unconscious causes over which the defendant had no control, leading to the conclusion of nonresponsibility.<sup>186</sup>

One can find “deep” reasons for almost all behavior if one digs. Although one must admit that there are probably some trivial criminal behaviors about which it would be difficult to fabricate a convincing tale of unconscious causes that overwhelmed the defen-

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<sup>185</sup> If this story did not fit the background or the post hoc data gathered by the dynamicist during the assessment, some other story that would be coherently fitting and satisfying surely could be constructed.

<sup>186</sup> It might be objected that the hypothetical case is unrealistic. Consider therefore the example from California of Lawrence S. Bittaker, who provides a more chilling, real world example. See *L.A. Times*, Feb. 10, 1981, pt. II, at 4, cols. 1-3. Bittaker is a forty-year old man accused of cold-bloodedly murdering and torturing five young women he and an accomplice had kidnapped. Four of the victims were raped and otherwise sexually abused. The crimes were committed in a particularly pitiless way and Bittaker went so far as to tape-record the agonies of his victims and to torture them psychologically as well as physically. Bittaker was entirely rational at the time of the torture-rape-killings, however, and, at his murder trial, the defenses of diminished capacity and insanity were not raised. It is hard to see why counsel should have been so reticent, however, especially in a state like California, which then had an expansive diminished capacity defense to murder. See, e.g., *People v. Conley*, 64 Cal. 2d 310, 411 P.2d 911, 49 Cal. Rptr. 815 (1966). In light of Bittaker's appalling actions, it is inconceivable that even the most neophyte dynamicist would be unable to construct a tale of aberrant psychodynamics for Bittaker. Indeed, unless one believes in pure evil — as surely the responsibility skeptics/method votaries do not — the only explanation for Bittaker's conduct must be his unconscious psyche.

dant, society is not concerned with the assessment of responsibility in such cases. Yet, in serious cases that involve psychologically important behavior and in which one expects an evaluation of responsibility, a dynamicist can always piece together a formulation involving deep, unconscious, compelling factors. There is no principled way for the selective dynamic compulsivist to argue that excusing or mitigating tales can be told only in some cases and not in others. The application of the French proverb, "tout comprendre c'est tout pardonner," is the necessary corollary to this dynamic approach.

Assuming, as we need not, that psychodynamic psychology is generally valid and that it is possible to create reliable formulations in individual cases, dynamic explanations either do not bear on responsibility in most conceivable cases, as Moore has demonstrated, or they prove far too much, as I have shown. Even if dynamic explanations are scientific, they are not relevant to evaluations of responsibility for criminal behavior, and the law should exclude them at all decisional stages of the criminal justice process.

#### *E. The Ghost and the Machine*

Assuming that psychodynamic explanations can help factfinders assess criminal responsibility, reliance on such explanations to determine blameworthiness and to mitigate punishment raises a severe practical problem. The problem is this. A legally sane defendant with the requisite *mens rea* — the machine — has committed an antisocial act. The actor thus evidences dangerousness and, without a dynamic explanation, blameworthiness as well. But suppose a psychodynamicist convinces the court that the defendant is not fully responsible; rather, the actor's unconscious dynamics — the ghosts — are to blame. This acceptance of psychodynamic theory raises two questions related to the criminal justice system's goals of retribution and incapacitation. First, considering only retribution, how should the law calibrate the diminution in responsibility and punishment resulting from an excuse based on a psychodynamic explanation? Second, a defendant who is less responsible for psychodynamic reasons is nonetheless dangerous because of factors beyond his or her control and should be incapacitated: how should the law resolve the tension that will arise between the retributive and incapacitative goals of the system? Psychodynamic psychology, however, can offer little guidance in apportioning pun-

ishment justly or in deciding whether the benefits of incapacitation are worth the costs of punishing less blameworthy defendants.

What are the possible and proper responses of the criminal justice system to the tension between punishment based on the machine and punishment based on the ghost? If the law reduces the punishment — typically imprisonment — meted out to the convicted defendant because he or she is less blameworthy, the dangerous defendant is freed earlier than the law otherwise dictates. Arguably this is an especially undesirable result because dynamically compelled defendants are unaware of the powerful unconscious forces that compel them to commit crimes: the unseen ghost renders the machine a particularly uncontrolled desperado. A reasonable answer to the danger of early release is to treat the prisoner psychodynamically — to exorcise the ghost. This tactic seems of dubious value: although it may make the prisoner feel better emotionally (if he feels bad at all), it is entirely unlikely that the dynamic treatment will substantially change his overt behavior.<sup>187</sup> Moreover, it is risible to imagine significant numbers of dynamicists practicing their arts in the prisons. If the law accepts the psychodynamic story but the convicted defendant is imprisoned longer than his or her culpability warrants in order to preserve social safety, it will be clear that society is willing to punish persons more harshly than they deserve and to employ preventive detention in the criminal justice system. Few other than pure utilitarians will accept this outcome, but it is the logical implication of psychodynamic excuses and should be recognized by those who wish to take such excuses seriously. Consequently, psychodynamic theory, if it is valid and relevant at all, will inevitably create tension between retribution and incapacitation, a tension which it can do nothing to help society resolve, either by ameliorating defendants' dangerousness or by suggesting other resolutions to the problems it identifies.

If reduced sentences that release dynamically compelled dangerous defendants "too early" are acceptable or obligatory because blameworthiness places an upper limit on punishment, the

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<sup>187</sup> See *supra* notes 131-35 and accompanying text. If dynamically compelled defendants can be held and treated until they are no longer dangerous, what will be the future criteria for cure and consequent nondangerousness? At present, of course, such criteria, like dynamic treatment for prisoners, are nonexistent.

dynamicist still has the burden of formulating adequate criteria by which legislatures, judges, and juries should calibrate the responsibility and punishment of these defendants. As mitigating variables, dynamic explanations are not like harsh childhood experiences, family tragedy, illness, and the like. The relationship of these latter types of variables to considerations of culpability and mercy is within the domain of common sense and needs no expert elucidation for sensible decisionmaking.<sup>188</sup> By contrast, the relationship of dynamic explanations to behavioral control is clearly beyond pure lay assessment and requires authoritative clarification. As this article has argued, however, scientifically verified and accepted knowledge of the relationship between dynamic explanations and behavioral control does not exist. If there is no legal or normative theory that links the expert's psychodynamic story to the degree of criminal responsibility and culpability, then the law will be unable without arbitrariness to determine how blameworthy the defendant is and to decide how much he or she should be punished. Because no such theory exists, it is beyond the powers of both experts and laypersons to calibrate the degree of responsibility and severity of punishment which are appropriate in view of a particular defendant's psychodynamic story. Moreover, when the factfinder is faced with conflicting formulations, or conflicting inferences about control based on those formulations, it is difficult to imagine what criteria can be used to assess the comparative merits of the case. There are no objective, external criteria one can use to assess validity: there is only the aesthetic and emotional intuition that the tale being told is satisfying.<sup>189</sup>

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<sup>188</sup> Playwrights and novelists have always known this.

<sup>189</sup> Perhaps, however, dynamicism can be used sensibly in criminal justice decisionmaking. In a previous paper, Morse, *Diminished Capacity: A Moral and Legal Conundrum*, 2 *Int'l J. L. & Psychiatry* 271, 292-96 (1979), I suggested that the determination of whether a defendant was only partially responsible for his or her crime should be accomplished at trial rather than at sentencing. In addition to the usual criminal law verdicts, a verdict of "guilty but partially responsible" would be appropriate in cases where the defendant's conduct fulfilled all the requisite elements of the crime, but the defendant was proven to be substantially less responsible than the average defendant for whatever reason, such as mental disorder or defect, the courts or legislatures chose to accept. If the arguments of the present paper are rejected, psychodynamic factors might be such a reason. Defendants found "guilty but partially responsible" would be sentenced to a punishment equivalent to a fixed percentage of the punishment allowed for the crime.

Bonnie and Slobogin have criticized this proposal as too discretionary, *supra* note 8, at 450, but their criticism misses the mark. My proposal limits potential arbitrariness in deter-



If mitigation based on psychodynamic explanations is taken seriously, sentencing becomes more arbitrary than it is now. Defendants who are especially dangerous because they supposedly have subnormal control over their behavior may be released earlier than other defendants convicted of the same crime, with no assurance that the lack of culpability that justifies the imposition of this cost on society exists. This is an irrational and dangerous result of taking psychodynamicism seriously in the criminal law. A criminal justice system that wishes to allow crucial responsibility decisions to be made on the basis of the vague data provided by psychodynamic analysis can hardly be committed, however imperfectly, to the search for truth. The responsibility skeptics correctly contend that justice requires imprecise data to be admissible in criminal trials, but there is a limit to such imprecision, a limit that dynamic explanations utterly transgress.

### *F. Summary*

Criminal courts should reject expert testimony based on psychodynamic psychology for four reasons. First, scientifically sound evidence about the validity of psychodynamic psychology discloses that much of the psychology is invalid and much has never been

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minations of criminal responsibility and sentencing. Partial responsibility is considered, but there is only one degree of it for all crimes and its relationship to sentencing is fixed. Moreover, because the decision about partial responsibility concerns the fundamental question of blameworthiness, it is a decision that should be made during the high visibility, morally adjudicative trial process. The factfinder, not a sentencing judge, should be allowed to decide in which cases psychodynamic factors affect responsibility. Furthermore, dynamic explanations are more likely to be considered fully if they are assessed at trial rather than at sentencing proceedings (capital punishment aside). The position taken in this article is that psychodynamic explanations do not belong in the criminal justice process at all, but, if they must be admitted, it is more sensible to admit them at trial under a partial responsibility approach of the type delineated just above. It is ironic that Bonnie and Slobogin criticized this proposal for creating too much arbitrariness in the system, when the upshot of their proposals will be just the discretion they decry.

The problems with employing psychodynamic formulations in the criminal justice system would not end, however, even if the law were able to devise a relatively nonarbitrary partial responsibility approach for dealing with dynamically compelled defendants. Assessing craziness is a simple matter compared to the proper construction of a psychodynamic formulation of an individual case. The latter typically requires a substantial number of hours of assessment; indeed, in dynamic therapy the process of assessment is continuous and formulations often change as the therapy produces more data. For both defense and prosecution to obtain a worthy dynamic assessment and formulation, enormous resource expenditures would be required.

tested.<sup>190</sup> Because the testimony lacks scientific validity, its admission would therefore be inefficient, misleading, and prejudicial. At most, courts should only accept expert testimony founded on psychodynamic psychology if the testimony is based on those aspects of the "discipline" that have been confirmed. Second, even if the general validity of psychodynamic psychology can be demonstrated, there is no evidence that clinicians can provide reliable formulations in individual cases. Thus, courts should exclude psychodynamically based testimony because there is no reason to believe that such testimony will be accurate. Third, psychodynamic explanations are not relevant to the ascription of criminal responsibility and the apportionment of punishment. Either they do not bear on criminal responsibility, or they prove too much because such explanations apply equally to all persons and will thereby excuse all persons. Fourth, even if the first three reasons are rejected, psychodynamic psychology is problematic because it is impossible to base either sensible dispositional policies or sound individual decisions on it, and its admission will inevitably enlarge the battle between experts. In sum, the introduction of expert testimony based on psychodynamic psychology will be inefficient, confusing, and prejudicial. As we shall see in the next section, it is also utterly unnecessary.

#### IV. CRIME, CRAZINESS, AND EXPERTISE

In this section, I shall consider briefly the theoretical and scientific merits of some representative examples of the recommendations Bonnie and Slobogin make for the broad use of mental health expertise in criminal cases,<sup>191</sup> and I shall examine whether forensic training will remedy the admitted weakness of current mental health expert testimony. The section will argue that there is little basis for recommending the broad use of expertise, and that training will not solve the present problems. Next, I shall suggest that my *Crazy Behavior* recommendations will make far better, more

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<sup>190</sup> Although some hypotheses have received reasonable confirmation, they are rarely the hypotheses that are most distinctively psychodynamic, and they often can be explained more parsimoniously by other theories. See *supra* notes 101-13 and accompanying text.

<sup>191</sup> Dealing with most of the claims would be fruitless nitpicking, so I shall not do so. Part III of this article already dealt with claims for the usefulness of psychodynamic psychology and these arguments will not be repeated.

rational use of mental health expert testimony than the scheme Bonnie and Slobogin propose. Finally, representative case studies provided by Bonnie and Slobogin will be examined to demonstrate the superiority of the *Crazy Behavior* proposals expanded upon here.

### A. *The Jurorpathology of Everyday Expertise*

The method votaries/responsibility skeptics believe that mental health professionals can contribute much to criminal justice decisionmaking. This belief rests on three assumptions: first, the criminal law must assess and take into account the defendant's actual psychological functioning; second, to do so properly, courts must admit somewhat imprecise data relevant to the defendant's psychology; and third, experts possess the knowledge, skills, and techniques to provide reasonably reliable and valid data relevant to legal questions of mens rea and responsibility. Although I agree that the criminal law must take into account a defendant's psychological states, and that courts must admit somewhat imprecise data to do so, I take issue with the degree to which Bonnie and Slobogin assume that mental health experts can provide legally relevant data that is sufficiently reliable and valid. Bonnie and Slobogin admit that much mental health science is still rather soft,<sup>192</sup> but they believe that most expert mental health testimony is sufficiently precise to qualify for admission into evidence.<sup>193</sup> Moreover, they believe that it is possible to eliminate much of the imprecision and many of the other faults of such testimony by adequately training experts to gather data and testify about it properly.<sup>194</sup> Although their assertions about expertise and forensic training appear facially reasonable, a closer inspection of the data and the arguments that support these assertions renders them unpersuasive.

#### 1. *The Merits of Mental Health Expertise: Some Legal Consequences of the Distinction Between Science and Speculation*

In *Crazy Behavior*, I set out the basic questions that all mental health cases pose, and I suggested how the law should properly

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<sup>192</sup> Bonnie & Slobogin, *supra* note 8, at 461.

<sup>193</sup> *Id.* at 461-62.

<sup>194</sup> *Id.* at 492-522.

frame these questions and how experts should properly answer them.<sup>195</sup> The analysis clarified the question of what data would be useful to legal decisionmakers. I then reviewed the contributions that mental health experts, as experts, could make in providing that data.<sup>196</sup> Indeed, without a clear understanding of the law's concerns in mental health cases, it is often irrelevant to demonstrate that professionals do or do not have particular knowledge or skills. The only knowledge and skills that the law should be concerned with are those that are relevant to proper legal questions.

My review of the scientific knowledge and clinical skills of mental health professionals convinced me that these professionals have much less to contribute to legal decisionmaking than is commonly supposed.<sup>197</sup> As described in Section II of this article, I concluded in *Crazy Behavior* that the proper role of mental health experts is to offer behavioral observations—which they often can gather more efficiently and fully than laypersons—and hard data relevant both to the question of the actor's capacity for self-control and rationality and to predictions about his or her future behavior.

Since the publication of *Crazy Behavior* in 1978, I have continued to review the mental health literature for new data, because many of my views are subject to revision in light of new advances in the behavioral sciences. I still conclude that the law should limit the role of mental health experts, as I suggested previously. Clearly there have been advances in mental science during this period, both in the knowledge about the biological and the psychosocial foundations of crazy behavior<sup>198</sup> — many of which advances are extremely promising — and in the devising of methods for acquir-

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<sup>195</sup> Morse, *supra* note 2, at 542-600.

<sup>196</sup> *Id.* at 604-22.

<sup>197</sup> *Id.* at 602. The most extensive and comprehensive—albeit one-sided—recent review of these issues that takes a similar, but more extreme position, is J. Ziskin, *Coping with Psychiatric and Psychological Testimony* (3d ed. 1981).

<sup>198</sup> See, e.g., G. Brown & T. Harris, *Social Origins of Depression* (1978); Berger, *Biochemistry and the Schizophrenias: Old Concepts and New Hypotheses*, 169 *J. Nervous and Mental Disease* 90 (1981); Carroll, *The Dexamethasone Suppression Test for Melancholia*, 140 *Brit. J. Psychiatry* 292 (1982); Stress, Social Support and Schizophrenia, 7 *Schizophrenia Bull.* 12 (1981); Weitkamp, Stancer, Persad, Flood & Guttormsen, *Depressive Disorders and HLA: A Gene on Chromosome 6 That Can Affect Behavior*, 305 *New Eng. J. Med.* 1301 (1981). On the other hand, some accepted conclusions, such as the link between genetic predisposition and schizophrenia, have come under renewed attack. Lidz, Blatt & Cook, *Critique of the Danish-American Studies of the Adopted-Away Offspring of Schizophrenic Parents*, 138 *Am. J. Psychiatry* 1063 (1981).

ing such knowledge.<sup>199</sup> There have also been contributions to what may loosely be termed the antimedical model of craziness.<sup>200</sup> Nevertheless, most of the advances have not produced data or skills that are legally relevant.<sup>201</sup> For instance, increased comprehension of basic behavioral processes cannot help answer the question of whether a person's craziness is extreme enough to reach the threshold of legal relevance, nor has our comprehension reached the point where it is possible to claim that we fully understand the causes of behavior, or almost do.

Let us now turn to a consideration of some of the claims that Bonnie and Slobogin make for the usefulness of professional knowledge and skills. An assessment of the contributions that mental health experts can offer to criminal law decisionmaking raises two related issues. First, if the knowledge and skills are relevant to legal questions, are they scientifically reliable and valid? Second, if the knowledge and skills are reliable and valid, are they relevant to legal questions? Bonnie and Slobogin clearly recognize two major problems that compromise the reliability and validity of expert testimony: bias in the collection of data and bias in the formation of opinions.<sup>202</sup> In asserting that these difficulties are controllable or remediable within reasonable limits for legal purposes, however, they rarely attempt a systematic evaluation of the scientific validity of the techniques or knowledge they offer, and all too often they support their assertions solely with speculations by a mental health professional. Bonnie and Slobogin assume, for instance, that mental health professionals can perceive and interpret body language cues more effectively than laypersons.<sup>203</sup> Although this assumption may be intuitively appealing, and some mental health professionals may believe it to be true, there is now available extremely good evidence based on rigorously performed research that demonstrates that mental health professionals are no

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<sup>199</sup> Buchsbaum, Ingvar, Kessler, Waters, Cappelletti, van Kammen, King, Johnson, Manning, Flynn, Mann, Bunney & Sokoloff, *Cerebral Glucography With Positron Tomography: Use in Normal Subjects and in Patients with Schizophrenia*, 39 *Archives Gen. Psychiatry* 251 (1982); Carroll, *supra* note 198.

<sup>200</sup> E.g., T. Sarbin & J. Mancuso, *Schizophrenia: Medical Diagnosis or Moral Verdict?* (1980).

<sup>201</sup> A possible exception is the American Psychiatric Association's adoption of DSM-III, *supra* note 131. DSM-III will be discussed in detail, *infra* note 210 and accompanying text.

<sup>202</sup> Bonnie & Slobogin, *supra* note 8, at 512-14.

<sup>203</sup> *Id.* at 506.

better than a norm group of high school students at assessing non-verbal cues.<sup>204</sup> In a similar vein, Bonnie and Slobogin quote an eminent forensic psychiatrist and a law professor to support the conclusion that an "experienced forensic clinician is able to assess the accuracy of the information that he uses to form his conclusion."<sup>205</sup> The quoted experts recognize the difficulties of assessing the accuracy of information received from the patient and other sources, but they conclude that the professional's training allows him or her to discern the truth. These experts may be right, but they cite no evidence in support of their assertion, and, to my knowledge, such evidence does not exist.<sup>206</sup>

The solutions Bonnie and Slobogin offer to remedy the problems in data collection and interpretation are unpersuasive. For instance, they recommend that forensic evaluations should include intensive fact gathering,<sup>207</sup> and that more than one professional should evaluate a subject to avoid the data collection biases that are inherent in solo evaluations.<sup>208</sup> I endorse these recommendations completely, but I am less impressed by the process they suggest for forming opinions based on evaluations. Bonnie and Slobogin recommend ongoing discussion by the evaluating staff

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<sup>204</sup> R. Rosenthal, J. Hall, M. DiMatteo, P. Rogers & D. Archer, *Sensitivity to Nonverbal Communication* 294-319 (1979).

<sup>205</sup> Bonnie & Slobogin, *supra* note 8, at 510 (citing Diamond & Louisell, *The Psychiatrist as an Expert Witness: Some Ruminations and Speculations*, 63 Mich. L. Rev. 1335, 1353 (1965)).

<sup>206</sup> Recent research calls into question the ability of mental health experts to be reasonably objective in legal proceedings. Simon & Zusman, *The Effect of Context on Psychiatrists' Perspective on Illness: A Case Study* (1981) (unpublished manuscript on file with the Virginia Law Review Association).

For a final example, Bonnie and Slobogin also appear to endorse the use of the Draw-A-Person Test (DAP) in forensic assessment. Bonnie & Slobogin, *supra* note 8, at 503 n.221. This test asks subjects to draw pictures of a man and a woman. Proponents of the test allege that professionals can draw accurate diagnostic inferences about the subject from the characteristics of the pictures. Despite its popularity, the DAP lacks reliability and validity, however, and its continued use by clinicians should be an embarrassment rather than a justification for using its results in the criminal justice system. See Harris, *Book Review*, *in* 1 *The Seventh Mental Measurements Yearbook* 401 (O. Buros ed. 1972); Kitay, *Book Review*, *in id.* at 404; Roback, *Human Figure Drawings: Their Utility in the Clinical Psychologist's Armamentarium for Personality Assessment*, 70 *Psychological Bull.* 1 (1968). Unfortunately, clinical psychologists are evidently unaffected by negative research evidence concerning the tests they use. Wade & Baker, *Opinions and Use of Psychological Tests: A Survey of Clinical Psychologists*, 32 *Am. Psychologist* 874, 879 (1977).

<sup>207</sup> Bonnie & Slobogin, *supra* note 8, at 508-09.

<sup>208</sup> *Id.* at 505, 513.

leading to a consensus formulation.<sup>209</sup> This is a sensible way to construct a hypothesis, but not to validate one. Unless other investigators can *independently* reach the same conclusions, there is no reason to believe that the formulation is reliable or valid. Multiple evaluations are a fine idea, but consensus opinions do not correct for bias. Rather, they tend to reflect the varying persuasiveness of the various formulators. More powerful, persuasive professionals will tend to convince the others, producing no gain in reliability and validity.

Speculations, assumptions, and assertions are not substitutes for hard evidence, and the method votaries simply do not show that their "expert" techniques and knowledge are reliable and valid. Unreliable and invalid "scientific" evidence cannot assist the factfinder, and it may be misleading and prejudicial. Therefore courts should not accept it.

The second difficulty with suggestions for the broad use of expertise is that even reliable and valid techniques may not generate data that respond to the questions properly asked by the criminal law. Techniques may yield data acceptable for some mental health purposes but inadmissible for use in the courtroom. The burden is on the dynamicists to demonstrate that the data in question is admissible.

Let us take some examples. The major change occurring in the last three years that bears on the use of mental health experts in the courtroom is the promulgation by the American Psychiatric Association of the long-awaited third edition of the *Diagnostic and Statistical Manual of Mental Disorders (DSM-III)*.<sup>210</sup> Use of this

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<sup>209</sup> Id. at 515-17.

<sup>210</sup> DSM-III, *supra* note 131. Bonnie and Slobogin do not deal with DSM-III, but it is crucially important to current psychiatric and psychological practice and research in the United States.

DSM-III differs from its maligned predecessor, American Psychiatric Association, *Diagnostic and Statistical Manual of Mental Disorders* (2d ed. 1968) (DSM-II), in at least four ways. First, the criteria of DSM-III rely far more on research evidence than on the sheer consensus opinions of its developers. Second, the diagnostic criteria for the various disorders are considerably more precise, and include relatively clear inclusion and exclusion criteria. Third, DSM-III adopts a multiaxial approach that considers factors such as physical health and social functioning in the total diagnostic picture. Fourth, there is theoretical and empirical reason to believe that DSM-III is considerably more reliable than its predecessor. These four factors render DSM-III possibly relevant to legal decisionmaking because reliable and relatively precise diagnoses conceivably might be efficient and adequate proxies for the determination of whether a person is crazy. But even if DSM-III is truly as reliable as it

manual promises to make expert diagnoses more reliable and to allow experts to articulate their diagnostic criteria and reasoning more precisely. Nevertheless, although the diagnostic categories of *DSM-III* are far more precise than those of the second edition of the manual (*DSM-II*), they are still not adequate proxies for legal craziness because they are not sufficiently precise to provide the factfinder with the necessary, full, textured account of how the defendant behaved.<sup>211</sup> Persons whose behavior places them within the same diagnostic category may differ greatly in the degree and quality of their behavioral abnormalities.<sup>212</sup> In determining whether a defendant is crazy, there is simply no substitute for the fullest possible account from all sources of the defendant's behavior at the time of the alleged offense. If experts provide the factfinder with such rich behavioral data, a diagnosis will add little of value because the criteria for *DSM-III* diagnoses, like those of *DSM-II*, are almost entirely behavioral.<sup>213</sup> Thus, permitting the ex-

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appears to be—and there is some reason to doubt its reliability, Morse, *supra* note 29, at 70 n.75—it is still not an adequate proxy for craziness. See *infra* notes 211-13 and accompanying text.

The authors of *DSM-III* also claim that there is research evidence for the validity of the various categories, but *DSM-III* admits that most categories "have not yet been fully validated," *DSM-III*, *supra* note 131, at 8, and I and others believe that in fact such evidence is distinctly lacking and that only future research will establish the validity of the diagnostic categories of *DSM-III*. Morse, *supra* note 29, at 70 n.75 (citing sources). See also Kendell & Brockington, *The Identification of Disease Entities and the Relationship Between Schizophrenic and Affective Psychoses*, 137 *Brit. J. Psychiatry* 324, 329 (1980) (meaning of validity of a disease entity in psychiatry rarely defined; no natural boundary found between schizophrenia and affective psychoses). The chief author of *DSM-III*, Robert Spitzer, has referred optimistically to this problem in a recent interview. *A.P.A. Monitor*, Oct. 1981, at 33.

Finally, *DSM-III* cautions that its use in nonclinical contexts, "such as determination of legal responsibility . . . must be critically examined in each instance within the appropriate institutional context." *DSM-III*, *supra* note 131, at 126.

<sup>211</sup> This statement and similar statements that follow in the text can only be confirmed by simple inspection of *DSM-III* criteria. Consider the general and specific criteria for schizophrenia and its subtypes. *DSM-III*, *supra* note 131, at 181-93. Knowing a person meets these criteria hardly substitutes for a full behavioral portrait of the actor.

<sup>212</sup> *Id.* at 6 ("Another misconception is that all individuals described as having the same mental disorder are alike in all important ways. Although all the individuals described as having the same mental disorder show at least the defining features of the disorder, they may well differ in other important ways that may affect clinical management and outcome."). Moreover, Axis V of *DSM-III*, which rates the highest level of adaptive functioning within the past year, is too imprecise to serve as a proxy for an assessment of social functioning.

<sup>213</sup> Behavioral abnormality is a necessary criterion for all diagnoses and it is sufficient for most, including the majority of disorders whose presence in a defendant may raise questions



pert to provide a diagnosis yields no additional data; and in labeling the defendant as suffering from a disorder, the expert interjects a host of unproven assumptions about disease processes and consequent lack of control. The criminal law must avoid this question-begging result if it is to resolve the question of responsibility properly.<sup>214</sup>

For another example, Bonnie and Slobogin suggest that a behavioral rating method called Ego Functions Assessment (EFA) can provide a reliable and valid method of assessing ego functioning.<sup>215</sup> Indeed, examination of the EFA reveals that it includes measures

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about criminal responsibility. Cf. Rappeport, *Differences Between Forensic and General Psychiatry*, 139 *Am. J. Psychiatry* 331, 333 (1982) ("It is not because the man had a psychosis that he is not responsible; it is how his illness affected his behavior and his ability to form the necessary criminal intent or to have the *mens rea*, or guilty mind, that is important.").

<sup>214</sup> It may be objected that the danger of ascribing legal relevance to an expert diagnosis is overstated. Professor John Monahan of the University of Virginia School of Law has suggested in a personal communication that a diagnosis may be, first, a convenient summary of behavior, and, second, an efficient means of communicating that persons who behave in certain ways (those whose behavior fits a particular diagnosis) are likely to behave in other ways. Interview with John Monahan (June, 1981) (notes on file with the Virginia Law Review Association). These suggestions are unpersuasive, however. A diagnosis simpliciter conveys little additional, legally relevant information to a legal factfinder beyond the description of the criterial behaviors themselves. Moreover, the diagnoses of DSM-III are too vague to convey much information about a person's degree of craziness or the quality of the criterial behaviors. DSM-III recognizes this point explicitly when it notes that persons with the same diagnosis may be different in important ways. DSM-III, *supra* note 131, at 6. Any convenience gained by using the shorthand — and convenience would be the only virtue — is more than offset by the problems I have identified.

The second suggestion merits more attention, but only in those cases where a diagnosis is significantly statistically related to other legally relevant behavior (beyond the criterial behaviors to which the diagnosis refers). At present, hard data linking diagnoses to other legally relevant behavior are rare, but when such data exist, experts can present them without using the diagnostic shorthand. For instance, the expert can say: "People who behave in A, B, C ways, are also X% likely to behave in Y (legally relevant) way." This is undoubtedly "less efficient" than saying, for example: "Schizophrenics are X% likely to behave Y way." But, again, the only loss is minor time efficiency, a price that seems worth paying in light of the possibility of the greater efficiency loss of predisposing the factfinder to an incorrect decision.

Professor Monahan has also suggested that, in any event, criminal juries pay little attention to diagnosis. If so, there is little practical loss in allowing or prohibiting diagnostic conclusions or diagnostic shorthand. On a more theoretical level, however, I still wish to avoid using question-begging diagnoses (and other forms of question-begging) as much as possible in mental health related cases in order to increase the honesty, clarity, and integrity of criminal justice decisionmaking. These issues are dealt with in further detail in the discussion of the case of Mr. G at *infra* notes 241-74 and accompanying text.

<sup>215</sup> Bonnie & Slobogin, *supra* note 8, at 521.

for "reality testing," "judgment," and "sense of reality," all of which do seem relevant to craziness and the capacity for control. Even assuming that the EFA is reasonably reliable and valid for mental health purposes,<sup>216</sup> however, there are several reasons to think it will not aid legal decisionmakers in resolving the relevant questions. First, defendants will always take the test after the time of the offense — usually long after — and its retrospective and external validity is unknown.<sup>217</sup> It therefore tells the factfinder nothing definite about the defendant's behavior at the time of the offense. Second, because all the scales rely on behavioral criteria, there is no advantage to giving a scale score rather than simply describing the behavior. A scale score is a conclusion: the raw data are far more useful and easily interpretable by laypersons. A description of the defendant's behavior itself is a better account of the defendant's functioning than a test score.<sup>218</sup>

In sum, those who propose to introduce into evidence the results of mental health tests and techniques should first demonstrate the legal relevance and usefulness of those results. Otherwise, factfinders may be so impressed with the "scientific" findings that they fail to form their own independent conclusions on legal issues, conclusions that they could easily have formed had they been presented with the raw, behavioral data.<sup>219</sup> Most of the evidence that Bonnie and Slobogin offer to support the broad use of expert testimony appears either scientifically unreliable or invalid, or not

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<sup>216</sup> A check of the Social Science Citation Index for every year since the EFA was first published until the present revealed that it is virtually never cited and never used in its full form in research or clinical reports except by its authors. The EFA may be both useful and employed regularly by mental health professionals, but there is simply no evidence of this in the literature. Thus, there is little substantial evidence of its reliability and validity beyond its initial publication, and, in its initial publication, the authors only suggested that it might be useful for legal purpose. This suggestion was never tested, however.

<sup>217</sup> A test may be said to be retrospectively valid if responses to it at a later date provide accurate information about a subject's behavior at an earlier date.

<sup>218</sup> Even if the EFA is a useful statistical technique for reporting research findings on, say, the outcome of psychotherapy, its results are not a useful means of presenting data to a legal factfinder.

<sup>219</sup> And, of course, few tests will be more helpful to factfinders than the raw behavioral data on which they are based. A recent review of the validity of psychological tests for providing legally relevant information produced "dismal" results. Poythress, *Is There a Baby in the Bathwater? Psychological Tests and Expert Testimony* 9 (1981) (unpublished manuscript on file with the Virginia Law Review Association). See Comment, *The Psychologist as Expert Witness: Science in the Courtroom*, 38 Md. L. Rev. 539 (1979).

demonstrably relevant to legal questions. Observation and hard data are the only sound and legally relevant evidence that the law can reasonably expect from the testimony of experts. The techniques and methods that Bonnie and Slobogin propose will not cure these problems and therefore will not produce evidence that will assist criminal law decisionmakers.

## 2. *Forensic Training: The Future of an Illusion*

Forensic training, despite the best intentions of proponents like Bonnie and Slobogin, cannot "cure" the defects in mental health science and testimony. No amount of training can render unreliable, invalid, and irrelevant methods reliable, valid, and relevant. Training programs to ensure that future expert witnesses understand the limits of their expertise would be terribly useful.<sup>220</sup> But even if mental health science has reasonably relevant information to offer the criminal law, there are insuperable practical problems with ensuring the proper training of adequate numbers of future mental health expert witnesses.

The proposal for improved forensic training as a means to justify the broad use of mental health expertise is the pursuit of a "fugitive utopia." The broad use of clinical mental health expertise should require that expert witnesses be fine clinicians,<sup>221</sup> but a

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<sup>220</sup> From personal communications with Richard Bonnie and Christopher Slobogin, I gather that they agree that good training programs should stress the limits of expertise. The difficulty, of course, is that we disagree wholeheartedly about what those limits are.

<sup>221</sup> One of the immediate difficulties is creating reliable and valid criteria for assessing who is a skillful clinician, but accepted, validated criteria do not exist at present. I do not wish to overstate this point; there may be personal qualities, for instance, that seem desirable. See S. Garfield, *supra* note 101, at 69-82. These qualities tend to be idealized and non-specific, however, and which qualities are chosen seems to depend on the professional theoretical orientation of the respondent. *Id.* For an example, concerning specific skills, a recent study from a prestigious psychiatric department tested the reliability of judgments by senior supervisors of the psychotherapy performance of psychiatric residents using a simple rating device. Although the reliability was statistically significant, it was "uniformly low." Liston, Yager & Strauss, *Assessment of Psychotherapy Skills: The Problem of Interrater Agreement*, 138 *Am. J. Psychiatry*, 1069, 1071-72 (1981). For another example, a Canadian study examined the reliability of ratings by senior psychiatrists of the performance by residents on the oral, clinical portion of a practice examination for board certification. The study concluded that, "there is a significant degree of interrater disagreement about the quality shown in a videotape of a simulated certification examination procedure." McCormick, *A Practice Oral Examination Rating Scale-Inter-Observer Reliability*, 26 *Can. J. Psychiatry* 236, 237-38 (1981). Simply put, it will be very hard in many cases for training programs to judge the clinical skills and knowledge of practicing mental health professionals. The train-

training program of the type that Bonnie and Slobogin propose can teach neither clinical skills nor, a fortiori, the complexities of psychodynamic psychology and assessment.<sup>222</sup> Forensic training programs will have to assume that enrolling clinicians have excellent clinical skills and are genuinely knowledgeable about mental health theory and data in general, and about psychodynamic principles in particular. But there is no guarantee that enrollees will have this knowledge or skill. Most psychiatric residency training is empirical and insufficiently theoretical. Even board-certified psychiatrists are rarely sophisticated dynamicists (or sophisticated theorists of any stripe) unless they privately engage in the intensive training provided by psychoanalytic institutes or similar institutions. Training in clinical psychology is usually more theoretical and research oriented, but, again, there is rarely sophisticated training in psychodynamic psychology.<sup>223</sup> At the completion of their formal training, few mental health professionals have the panoply of skills and knowledge that should be a prerequisite for training in the type of forensic program that Bonnie and Slobogin propose.

I suspect that even highly reliable and valid admissions criteria would not guarantee the enrollment of excellent clinicians, because

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ing program might develop its own evaluation instruments, including observation of a sample clinical assessment, but such evaluations would be very expensive and of unclear reliability and validity. For the purpose of this discussion, however, I shall assume that adequate criteria for assessing clinical skills can be developed.

<sup>222</sup> It should be remembered that formal psychoanalytic training rarely takes less than five years of intensive work, including a full analysis of the student, and often it takes longer. A recent impressionistic study of candidates for Canadian certification in psychiatry found that the examinees had less sophistication in psychodynamics than would be expected among psychiatric specialists. Warne, *The Current Level of Psychodynamic Knowledge of Candidates for the Canadian Certification Examination in Psychiatry*, 26 *Can. J. Psychiatry* 296, 299 (1981) (also noting insufficient emphasis on psychodynamics in same training centers).

A recent survey of third-year psychiatric residents found that residents rated their medical education and experience with drug therapy as the highest aspects of their psychiatric training, whereas personal analysis or psychotherapy and training in psychoanalysis and research were rated lowest. See Coryell & Wetzel, *Attitudes Toward Issues in Psychiatry Among Third-Year Residents: A Brief Survey*, 135 *Am. J. Psychiatry* 732, 733 (1978). See also Coryell, *The Organic-Dynamic Continuum in Psychiatry: Trends in Attitudes Among Third-Year Residents*, 139 *Am. J. Psychiatry* 89, 90-91 (1982) (rankings of training experiences were the same as in Coryell & Wetzel, *supra*, but survey demonstrated some revival of enthusiasm for psychoanalysis; response rate low, however).

<sup>223</sup> Fewer and fewer clinical psychologists define themselves as psychodynamically oriented. See Garfield, *supra* note 131, at 178.

better clinicians generally will not be interested in forensic work.<sup>224</sup> The primary business of mental health clinicians is, and *ought to be*, evaluating and helping crazy people who want help. Forensic work rarely helps any patient in a mental health sense. Where the prosecution retains a professional to evaluate a defendant, the clinician is not the agent of the allegedly disordered subject. Indeed, the professional is acting contrary to the defendant's perceived best interest. No stance, no conduct, could be further from the primary caring role of a competent mental health professional. If a substantial percentage of better-than-average clinicians decline to engage in forensic work because it is not the type of work that maximizes the fundamental goals of their profession, forensic training programs will have to teach those who do enroll the basic clinical skills as well as the theoretical and clinical psychodynamic techniques and knowledge required for testimony of the type that Bonnie and Slobogin propose.

A final problem is that if broad use of expertise continues, there will be a much greater need for trained forensic specialists nationwide.<sup>225</sup> Even if the programs were excellent and enrolled fine clinicians, it would be impossible to develop enough programs to train enough forensic specialists to possess a degree of skill and knowledge that would ameliorate the present problems. Most testifying professionals would therefore have the same disabilities that are so obvious today. Training programs cannot, under the Bonnie and Slobogin view of appropriate testimony, supply adequate amounts of excellent expert testimony.

The forensic training program remedy will be an ineffective placebo. No amount of training can ameliorate the scientific weaknesses or irrelevance for legal purposes of much mental health expertise. Even if the science makes dramatic gains, as I expect it will, training programs will make only minuscule improvements in the deficiencies of expert testimony: either the skills and knowledge that might be imparted will be unnecessary, or, less optimistically, there will be no means by which enough proper training can

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<sup>224</sup> This is only a hunch. Admittedly, there are no reliable data about any of these topics. I did consult, however, a totally nonrandom group of mental health professionals who are knowledgeable about forensic work and they universally agreed with the proposition in the text.

<sup>225</sup> This will be especially true if the law widely and broadly accepts the validity of excusing and mitigating psychodynamic explanations.

be provided, even assuming proper training is "the answer."

B. *Rationalizing Expert Testimony:  
Beyond the Battle Principle*

The *Crazy Behavior* proposals for the proper use of expert testimony would not require expensive and ineffective training programs and would alleviate almost all of the present problems of unreliability, invalidity, and irrelevance. According to those proposals, experts would not offer conclusions, psychiatric or legal, nor would they offer unproven theoretical explanations.<sup>226</sup> Experts would only offer clinical observations and hard data.<sup>227</sup> Consequently, they would not need to know the law, or how to separate fact from value questions. Moreover, experts would not need the sophisticated psychodynamic knowledge that the Bonnie and Slobogin scheme demands but cannot provide. Theoretical sophistication or lack of it would not be an issue, because the factfinder would not need to hear theoretical speculation from experts. Perhaps most important, the "battle of the experts" would be reduced to minor skirmishes in most cases.

Three basic factors probably are primarily responsible for the battle of the experts: the softness of mental health theory, data, and collection methods; the nonscientific character of legal issues; and the inevitable bias of mental health experts as they enter the criminal justice system as advocates. Let us consider these factors in light of the *Crazy Behavior* proposals.

First, experts often disagree about both observational data and inferential conclusions such as diagnoses or theoretical explanations for behavior. Some degree of disagreement among experts on factual matters and interpretations of these facts occurs in all fields, but it is especially problematic in mental health: the primary assessment technique is human interaction, the criteria for drawing conclusions are often unusually vague, there are myriad competing explanatory theories, and the conclusions reached are commonly unverifiable.<sup>228</sup> Under such conditions, it is unsurprising that experts will disagree about the subject's behavior or explana-

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<sup>226</sup> Morse, *supra* note 2, at 618-19.

<sup>227</sup> *Id.* at 601, 625.

<sup>228</sup> Rarely are laboratory tests or other forms of relatively objective and verifiable data employed.

tions. The measuring tool changes from examiner to examiner, no theoretical scheme is self-evidently correct, and reliable and valid benchmarks for drawing inferences are often lacking.

Limiting expert testimony to observations of behavior and hard data would alleviate many of the difficulties produced by the softness of mental health science. Observers are far less likely to disagree about observations than about inferential conclusions, and disagreement about observable behavior, precisely because it is about *observable* data, is easier to resolve than disagreements about inferences. Moreover, factfinders need not be exposed to the unseemly spectacle of experts jousting over inferences, because factfinders are perfectly capable of drawing those inferences themselves if they understand the relevant legal questions. Within the realm of hard data produced by acceptable scientific methods,<sup>229</sup> there can of course be disagreements about what the data show, but here, too, there are criteria for assessing studies — primarily methodological soundness — that provide a touchstone for assessing disagreements. The *Crazy Behavior* scheme therefore will confine the courtroom-battle behavior of mental health experts to those matters where factfinders themselves can assess and resolve the disputes.<sup>230</sup>

Second, demanding or allowing conclusions from mental health experts on ultimate legal issues such as criminal responsibility or the capacity to form *mens rea*<sup>231</sup> is certain to cause unseemly dis-

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<sup>229</sup> See *supra* note 78.

<sup>230</sup> I suggest that all forensic assessment interviews be videotaped and that the factfinder view the tapes if either party so requests. When mental health experts testify on the basis of clinical examination, they will rely on parts of the examination that support their conclusions and the factfinder may not hear about portions of the examination that would lead to contrary inferences. Cross-examination can alleviate this difficulty to some degree, but it is not an adequate substitute for allowing the factfinder to view the raw data to decide whether the defendant's behavior during that interview supports the expert's inference. Even if the factfinder is not routinely allowed to view the tape on grounds of cost, inefficiency, threat of confusion, or the like, cross-examination will be far more effective if counsel can see the opposing expert's examination.

The American Psychological Association (APA) has recently recommended taping of clinical interviews as a means of ensuring the fair administration of court-ordered, government-requested clinical interviews. Amicus Brief of APA at 22-23, 26-27, *United States v. Byers*, No. 78-1415 (D.C. Cir. 1981). The reasoning of this brief applies to all clinical interviews performed to provide evidence in criminal cases.

<sup>231</sup> See, e.g., Fed. R. Evid. 704. A notable exception is a California statute that prohibits mental health expert testimony on the issue of whether a criminal defendant had the requisite *mens rea* in fact at the time of the alleged offense. Cal. Penal Code § 29 (West Supp.

agreements among experts because such issues are not capable of scientific resolution. Consider a case where the experts agree on the behavioral data, and perhaps even on the diagnosis and inferred psychodynamic mechanisms that would explain the behavior. Nonetheless, one expert testifies that the defendant could conform to the requirements of the law or that the defendant had the mental capacity to form a *mens rea*, and the other expert testifies to the opposite. Moreover, both deliver their opinions as a matter of "reasonable medical (or psychiatric or psychological) certainty." Because there is no scientific standard for the "ability to conform" or for the capacity to form a *mens rea*, it is again unsurprising that the experts will disagree. The legal standards are primarily moral and social — only the application of moral, social, community standards and values can determine whether a specific defendant's behavior conforms to the legal standards. Facts alone do not provide the answer. Disagreement among experts on these issues is as inevitable as disagreements among jurors, because there is no scientific resolution of the questions the law poses. Experts offering legal conclusions are operating as extra, and unnecessary, jurors. If these fertile grounds for disagreement are removed, factfinders can discharge their duties unencumbered by the unnecessary confusion and obfuscation resulting from expert disagreement.

Finally, the pre-existing bias of mental health experts and the bias resulting from participation in the criminal justice system as advocate-witnesses also promote the battle of the experts. Mental health professionals, like all other citizens, have social and political biases that extend to their views of criminal justice.<sup>232</sup> Moreover, even a relatively neutral professional inevitably becomes an advocate when entering a case on one side or the other. In addition, the softness of mental health science and the legal system's willingness to accept the conclusions of experts on legal issues provide little

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<sup>232</sup> The most notable example in recent years is Dr. John Grigson of Texas, known familiarly as the "hanging psychiatrist," because he has testified for the prosecution in favor of the death penalty in many cases. Dr. Grigson was the psychiatrist involved in *Estelle v. Smith*, 101 S. Ct. 1866 (1981) (holding that the fifth and sixth amendments apply to a psychiatric examination of a criminal defendant where the results of the examination may be used to support the imposition of the death penalty). Dr. Bernard Diamond is a notable example of a psychiatrist who is sympathetic to the defense. See Diamond, *Criminal Responsibility of the Mentally Ill*, 14 *Stan. L. Rev.* 59, 60-61 (1961).



scientific constraint on the expert's testimony and great opportunity for them to express these biases. In a particular case, experts may agree on the behavioral data, but their individual views on criminal justice and their advocacy bias will lead them to disagree on the issues of criminal responsibility. By asking an expert's opinion on a nonscientific issue and by allowing experts to testify about impermissibly soft data or theory, the legal system permits experts to air their own biases before the credulous factfinder as if those biases were matters of reasonable scientific certainty.

Few people subscribe today to the myth of the "impartial" mental health expert.<sup>233</sup> Accepting the inevitable bias of mental health experts, however, should not lead necessarily to the conclusion that the law cannot ameliorate the effects of this bias. The *Crazy Behavior* proposals will substantially reduce the effects of partiality by eliminating much testimony that allows the expression of such bias and by permitting testimony on those topics — observable behavior and hard data — where the effect of bias on opinions is diminished and far easier for the factfinder to detect and address. Although Bonnie and Slobogin are commendably sensitive in general to the problem of bias in expert witnesses,<sup>234</sup> they do not confront directly the problems of criminal justice and advocacy bias and do not attend to their remedies. The *Crazy Behavior* proposals will do far more to eliminate these difficulties than advances in mental health science or the forensic training of experts.<sup>235</sup>

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<sup>233</sup> Morse, *supra* note 2, at 608 & n.172. See Gardner, *The Myth of the Impartial Psychiatric Expert—Some Comments Concerning Criminal Responsibility and the Decline of the Age of Therapy*, 2 *L. & Psychology Rev.* 99 (1976).

<sup>234</sup> See Bonnie & Slobogin, *supra* note 8, at 512-14.

<sup>235</sup> The *Crazy Behavior* proposal would have other beneficial effects as well. Limiting expert testimony would decrease costs without depriving the legal system of valuable and sufficient expertise. The expertise most needed by the legal system is clinical assessment to provide behavioral observations, and clinical assessment skill is the most plentifully available of all mental health clinical skills (assuming, of course, as courts do now and will continue to do, that clinical skill is a valid entity in mental health). Moreover, because hard data could come from professionals of any discipline competent to provide it — not just psychiatry and psychology — the *Crazy Behavior* scheme would enlarge the pool of experts. See Morse, *supra* note 2, at 622-24.

Hearsay problems would also decrease. Expert witnesses are allowed to base their testimony on hearsay, see Fed. R. Evid. 703, but, again, use of hearsay testimony is a particularly unfortunate mistake in the mental health area where the data are behaviors and the measuring tools are so inaccurate and subject to distortion. See Note, *Hearsay Bases of*

There is not today substantially greater mental health expertise relevant to criminal law questions than I claimed in *Crazy Behavior*. Nor is forensic training a workable solution. Given the present state of the mental health arts, the *Crazy Behavior* proposal is far more likely to make expert testimony more accurate and less expensive. Most importantly, under the *Crazy Behavior* scheme there will be no unfairness to either the prosecution or the defense. To buttress these contentions, this article will now turn to an assessment of representative cases used by Bonnie and Slobogin to enhance the validity of their analyses and proposals.

### C. *The Cases*

In this section, I shall examine the exemplary cases that Bonnie and Slobogin provide from the comparative perspectives of their proposals and mine. In the course of this analysis, I shall consider the theoretical and scientific validity of the various expert tools that Bonnie and Slobogin use. I hope to demonstrate that the *Crazy Behavior* system is fairer and more valid.

#### 1. *Mr. G. and the Usefulness of Diagnosis*

Only a full and textured description of the defendant's behavior provides the factfinder with the information necessary to answer proper criminal law questions. A reliable diagnosis does not provide such a description.<sup>236</sup> If one is given a diagnosis, one presumably has a disorder. The criminal justice system really wants to know, however, how the defendant behaved at the time of the alleged offense — what the actor was thinking and feeling, and how much control he or she had over the behavior. Although the behavioral data that meet the criteria for a diagnosis may help answer these questions, the diagnosis itself conveys little additional infor-

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Psychiatric Opinion Testimony: A Critique of Federal Rule of Evidence 703, 51 S. Cal. L. Rev. 129 (1977). There is no indication, other than bald assertion, that mental health professionals can evaluate the accuracy of their informants' observations. The distortions that inevitably occur when an expert bases his or her conclusions on the unverified behavioral observations of others may be acceptable in clinical practice, but they are not acceptable for the vitally important function of determining guilt and punishment.

<sup>236</sup> See *supra* notes 210-14 and accompanying text. Bonnie and Slobogin recognize that the question of mental disease or disorder is neither crucial nor necessary to criminal justice decisionmaking, Bonnie & Slobogin, *supra* note 8, at 466-73, but I believe that they do not confront fully the implications of this recognition.

mation about cognitive, emotional, or control processes. It does not give the factfinder a "feel" for how abnormal the defendant was or how much he or she was suffering. Nor does it provide the judge or jury with a three-dimensional portrait of the defendant that will permit the judge or jury to make their difficult and important legal and social decisions.

Bonnie and Slobogin claim that "[e]xpert opinions about the comparative severity of behavioral dysfunction often rely upon diagnostic concepts."<sup>237</sup> With all due respect, they have it backwards. Modern diagnosis begins with signs and symptoms (i.e. behaviors) and attempts to determine into which category the behaviors best fit. Some diagnoses are considered more severe than others precisely because their behavioral criteria are more severe and crazy. Even if the expert presents the definitional, behavioral criteria that led to a reliable *DSM-III* diagnosis, a full portrait of the severity of the defendant's abnormality will not be conveyed unless the expert also reports the fullest description of all the defendant's behavior, including the normal aspects.<sup>238</sup> In determining whether the actor is sufficiently crazy to meet the test of legal abnormality, what Bonnie and Slobogin dismissingly refer to as "a bare description of symptoms"<sup>239</sup> represents most of the data that there are. Nor, in the absence of hard data linking a diagnosis to other legally relevant information, can a diagnosis provide any further information on a person's other psychological functioning or behavior.<sup>240</sup> If

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<sup>237</sup> Bonnie & Slobogin, *supra* note 8, at 468.

<sup>238</sup> Although the multiaxial approach of *DSM-III*, *supra* note 131, also includes assessment Axis V, which is used to assess the patient's highest level of adaptive functioning during the last year, this Axis is not necessarily related to the patient's diagnosis; instead it relies on behavioral assessment.

<sup>239</sup> Bonnie & Slobogin, *supra* note 8, at 469.

<sup>240</sup> As discussed *supra* note 214, if hard data link (or do not link) a reliable diagnosis to other law-related behavioral consequences, say, some types of antisocial behavior, that diagnosis may appear useful. See, e.g., Monahan & Steadman, *Crime and Mental Disorder: An Epidemiological Approach* (1982) (unpublished manuscript on file with the Virginia Law Review Association) (no relationship between crime and particular disorders); Special Report, *The Nature of Aggression During Epileptic Seizures*, 305 *New Eng. J. Med.* 711, 715 (1981) (directed aggression during seizures extremely rare and it is nearly impossible to commit murder or manslaughter during random and unsustained psychomotor attacks). Even then, however, the diagnostic term is not necessary. The expert can convey the same data in the following form: X% of persons who, like this defendant, behave in ways A, B, C (where A, B, and C are the criteria for schizophrenia), also engage in aggravated assaultive behavior, whereas only the lesser percentage, Q, of normal persons also engage in such behavior.

the court is concerned with the defendant's state of mind, a rich, factual description of the defendant's delusions is relevant and useful; the bare conclusion that the defendant was schizophrenic is not.

To prove that diagnosis itself is legally relevant, Bonnie and Slobogin relate the case of Mr. G, a twenty-five-year-old charged with capital murder for the rape and murder of a seventeen-year-old woman and the subsequent murder of the young woman's mother.<sup>241</sup> Mr. G's history, as narrated by Bonnie and Slobogin,<sup>242</sup> is a sad one: his mother abused him; his father died when Mr. G was two; his stepfather, to whom he was particularly close, died when Mr. G was ten; Mr. G had alcohol, drug, and school problems during adolescence; he was isolated and lacked close friends; and, during his late adolescence, Mr. G observed the promiscuous behavior and alcohol and drug abuse of his mother and sister (Mr. G's mother denied her promiscuity and the abuse of intoxicants by her and the sister).<sup>243</sup> Although the tale is told in terms of conclusions, and almost nothing of the positive aspects of Mr. G's life is related—for example, the relationship between Mr. G and his stepfather—it is admittedly an awful life.

To determine the degree to which Mr. G was abnormal, Bonnie and Slobogin provide information about Mr. G's present psychological functioning. During his clinic interview, Mr. G "appeared extremely depressed and demonstrated considerable anxiety."<sup>244</sup> Based on this information, no mental health professional would have an idea, except in the vaguest way, of how Mr. G appeared. A description in raw behavioral detail of how he behaved would be much more illuminating and helpful than these conclusions. Anxiety and depression are common affects that laypersons with the relevant behavioral information can assess.<sup>245</sup> The case study tells

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<sup>241</sup> Bonnie & Slobogin, *supra* note 8, at 469.

<sup>242</sup> How one tells the tale is of course heavily influenced, wittingly or unwittingly, by one's preconceptions, biases and purposes in telling it. This is true for therapists, see Rosenhan, *On Being Sane in Insane Places*, 179 *Sci.* 250, 253 (1973); sources cited *supra* note 146-47, and we can be certain it is true for criminal justice observers. Surely the case history could be presented to make Mr. G appear far more "normal."

<sup>243</sup> Bonnie & Slobogin, *supra* note 8, at 469.

<sup>244</sup> *Id.*

<sup>245</sup> See, e.g., Rippere, *How Depressing: Another Cognitive Dimension of Commonsense Knowledge*, 19 *Behav. Research & Therapy* 169 (1981); Rippere, *Predicting Frequency, Intensity and Duration of Other People's Self-Reported Depression*, 18 *Behav. Research &*

us that Mr. G demonstrated an "unsophisticated and almost child-like" thought process,<sup>246</sup> but it does not report what the criteria for this conclusion were. We do not know whether Mr. G was compared to twenty-five-year-old males of his socioeconomic status, intelligence, and education, nor do we know what *scientific, expert* criteria were used to reach these completely unhelpful conclusions. Bonnie and Slobogin report that Mr. G had pervasive *repressed* hostility toward his mother and sister, but that he also consciously considered them "junkies" and "sluts."<sup>247</sup> Was the hostility so repressed? The case study reports that Mr. G had "autistic thought processes,"<sup>248</sup> but we do not know what this means. Again, instead of using a conclusory "scare" label — autistic — the report should describe Mr. G's thought processes in detail and let the factfinder decide how normal Mr. G's thinking was. If there are studies relevant to persons like Mr. G that provide statistically normative data about cognitive processes, the factfinder should by all means have this data as well. Bonnie and Slobogin also relate that Mr. G was able to communicate in a "normal" fashion and that he had supported himself for several years with no apparent problems.<sup>249</sup> One would like to hear much more about Mr. G: how did he get along with co-workers, did he date, did he have any friends, what was the nature of his friendships, and so forth. Any factfinder who possesses all the data upon which Bonnie and Slobogin based their conclusory opinions, plus all the additional data that I would require, would be fully able to decide whether the defendant was sufficiently crazy to meet the legal threshold for an insanity defense.

Not only do Bonnie and Slobogin provide unhelpful conclusions

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Therapy 259 (1980); Rippere, Commonsense Beliefs About Depression and Antidepressive Behaviour: A Study of Social Consensus, 15 *Behav. Research & Therapy* 465 (1977). Cf. Sarbin, Anxiety: Reification of a Metaphor, 10 *Archives Gen. Psychiatry* 630 (1964) (denying the validity of the construct).

One might object that this assertion does not account for cases of so-called "smiling" or "masked" depression, where the person is allegedly depressed but shows none of the usual outward signs of this affective state. If this is a valid diagnostic category, then experts can present the behavioral detail and linking data that lead them to conclude that a person is depressed, even though the person does not appear to be. If the category is speculative, however, the expert should not infer for the factfinder that a person feels blue (or whatever) contrary to his or her appearance. The category does not exist in DSM-III, *supra* note 131.

<sup>246</sup> Bonnie & Slobogin, *supra* note 8, at 469-70.

<sup>247</sup> *Id.*

<sup>248</sup> *Id.* at 470.

<sup>249</sup> *Id.*

about Mr. G's psychological functioning, they also insist on diagnosing Mr. G in an attempt to "aid" the factfinder. The diagnosis they offer, however, like their behavioral conclusions, is of dubious reliability and assistance. Bonnie and Slobogin claim that Mr. G's "early emotional isolation and unresponsiveness to others, his pervasive but repressed hostility toward his mother and sister, and his autistic thought processes were indicative of a severe 'schizoid personality disorder.'"<sup>250</sup> They allege that Mr. G's "behavioral manifestations met the accepted criteria for the diagnosis [of schizoid personality disorder]."<sup>251</sup> This diagnosis is questionable for several reasons. First, Mr. G's childhood functioning is *not* a criterion for the present diagnosis of "schizoid personality disorder." Although a childhood schizoid personality may predispose to the adult trait, there are data indicating otherwise.<sup>252</sup> The issue is whether Mr. G is still isolated and unresponsive. We know he was able to support himself financially, and that he saw women socially. Second, as we have seen, his hostility was hardly repressed, and there are simply no data to support the conclusion that at present he demonstrates *abnormally* autistic thought processes. More importantly, Bonnie and Slobogin have labeled Mr. G with a now-abandoned diagnosis from *DSM-II* which is extraordinarily vague, highly unreliable, and of unknown validity.<sup>253</sup> Arguably, too, Mr. G's diagnosticians have

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<sup>250</sup> Id. (emphasis added).

<sup>251</sup> Id.

<sup>252</sup> LaVietes, *Schizoid Disorder*, in 3 CTP-III, *supra* note 1, at 2613, 2614-15.

<sup>253</sup> Bonnie & Slobogin, *supra* note 8, at 470 n.35. The vagueness of the diagnosis can be confirmed simply by examining it:

This behavior pattern manifests shyness, oversensitivity, seclusiveness, avoidance of close or competitive relationships, and often eccentricity. Autistic thinking without loss of capacity to recognize reality is common, as are daydreaming and the inability to express hostility and ordinary aggressive feelings. These patients react to disturbing experiences and conflicts with apparent detachment.

*DSM-II*, *supra* note 210, at 42. It takes no sophistication in mental health science to recognize that these completely unquantified criteria are so broad and vague that persons who display extraordinarily disparate kinds of behavior might easily be so classified. Of course, the unacceptably weak reliability and unknown validity of *DSM-II* diagnoses—which had been developed by professional consensus and without regard to hard scientific data—were the major reasons that *DSM-II* was abandoned in favor of *DSM-III*. For example, the average reliability coefficient for personality disorders was found to be only .32 in a re-analysis of major reliability studies. Spitzer & Fleiss, *A Re-analysis of the Reliability of Psychiatric Diagnosis*, 125 *Brit. J. Psychiatry* 341, 344 (1974). Robert Spitzer, the primary author of *DSM-III*, recently noted that "it is gilding the lily to refer to the general six sentence *DSM-II* description of schizophrenia as criteria. To do so is to obscure the most important ad-

missed the clearly correct diagnosis. As early as 1917, similar cases were noted and classified as "cyclonic brainstorms."<sup>254</sup> Alternatively, according to *DSM-III*, it may not be clear that Mr. G has any disorder at all.<sup>255</sup>

Even if Mr. G is given a reliable *DSM-III* diagnosis, it is of little assistance to the factfinder. Although Bonnie and Slobogin properly note that it is necessary for experts to present the underlying data and reasoning upon which they base their diagnosis, they present us here with almost pure conclusions. They tell us that Mr. G was not psychotic,<sup>256</sup> but that he may have been "bordering on that condition."<sup>257</sup> Why? We do not know the criterion for the conclusion, or whether all schizoids border on psychosis, or how many ultimately cross the line, or whether elements of psychosis appear before the line is crossed. In any case, the use of a diagnosis to present such data, even if they exist, is quite unnecessary and perhaps misleading and prejudicial.<sup>258</sup> Finally, Bonnie and Slobogin note that Mr. G was not an "average" schizoid personality and that his condition was severe.<sup>259</sup> But what are the criteria for these assertions about severity, and are they reliable and valid?<sup>260</sup>

Bonnie and Slobogin claim that conclusions about severity, possible proximity to psychosis, and nonaverageness "could place into

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vance in nosologic research that occurred in the 1970's . . . : the specified inclusion and exclusion criteria for making a psychiatric diagnosis." Spitzer, In Reply, 38 *Archives Gen. Psychiatry* 1229 (1981). This point surely applies even more strongly to the description of "schizoid personality" in *DSM-II*. Thus, the claim that a person's behavior met the "accepted" criteria for the *DSM-II* category of "schizoid personality disorder" is virtually meaningless.

<sup>254</sup> Remondino, *Cyclonic Brain Storms: Their Entity and Character and their Relations to Legal Medicine*, 34 *Medico-Legal J.* 1 (1917) (attributing the brainstorms to an oversupply of blood in the brain and recommending bleeding as a possible remedy).

<sup>255</sup> The major contenders would be "schizoid personality disorder," *DSM-III*, *supra* note 131, at 309-11, and "avoidant personality disorder," *id.* at 323-24. The critical category for differential diagnosis would be "schizotypal personality disorder," *id.* at 312-13. Another possibility is "isolated explosive disorder," *id.* at 297-98. I shall leave it to my more intrepid colleagues to conclude where in the diagnostic shell game Mr. G's pea may be found. Comparative inspection of the four categories listed above and Mr. G's case history should convince the reader, however, that guessing the correct location of the pea, if it is under any of the shells at all, will not be easy.

<sup>256</sup> Bonnie & Slobogin, *supra* note 8, at 470.

<sup>257</sup> *Id.*

<sup>258</sup> See *supra* notes 214, 240.

<sup>259</sup> Bonnie & Slobogin, *supra* note 8, at 470-71.

<sup>260</sup> As far as I know, such validated criteria do not exist.

better perspective the type of disorder from which Mr. G suffered. This diagnostic description helps the factfinder to assess the relative severity of Mr. G's mental condition, facilitating a more informed decision on the 'mental disease or defect' question."<sup>261</sup> I disagree. These conclusions offer little in addition to the factfinder's knowledge of the defendant's raw behavior. Conclusory labels do not help the factfinder to judge the nature or severity of a "condition" when the labels and conclusions are diagnosed on the basis of behavioral criteria. A factfinder in full possession of the raw behavioral data will be capable of drawing conclusions for legal purposes about the nature and severity of the defendant's abnormality. Moreover, why should the "type of disorder" a defendant suffers from be an important issue? The difficulty in diagnosing Mr. G strongly confirms my thesis. Whether Mr. G's behavior fits a diagnostic category or falls between the diagnostic cracks is not the legal point. The relevant legal questions are whether the defendant is so crazy, and whether the craziness is so related to the criminal behavior, that the usual rules of criminal responsibility perhaps should not apply. The factfinder can judge this without conclusory help from the experts.<sup>262</sup>

Assume, for example, that there are personalities marked by the traits of shyness, over-sensitivity, and seclusiveness, and that these traits can be assessed reliably. For some persons, these characteristics may be mild or simply considered their "personality." For others, these traits may be so intense that one feels that there is something wrong. But one does not need an expert to decide when personality traits stop being normal, or even quirky, and begin to appear crazy. Even within the abnormal range, as either experts or laypersons define it, there will a continuum of abnormality or severity that laypersons can assess with sufficient data. If there are distributions describing the frequency of schizoid behaviors, experts can present them without a diagnosis. If the threshold question is simply one of craziness or abnormality *vel non*, for legal purposes laypersons and experts alike can assess the defendant on the basis of the behavior.

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<sup>261</sup> Bonnie & Slobogin, *supra* note 8, at 471.

<sup>262</sup> Bonnie and Slobogin recognize that mental disorder is not the issue. See *id.* at 510. But even if one is playing the game by irrational rules, the information as presented is not helpful.



Not content with using a diagnostic label to assist the determination of normality, Bonnie and Slobogin also essay a psychodynamic formulation to explain Mr. G's behavior.<sup>263</sup> They "explain" that severe schizoids can confuse fantasy and reality. Mr. G had known his victims for some time and considered them sluts. He therefore identified them with his hated mother and sister and projected his hatred and disgust for his mother and sister onto the victims. At the time of the killing, the daughter-victim had just rejected intercourse with Mr. G after previously encouraging his sexual advances. Presumably enraged by the rejection, this severe schizoid acted as some schizoids (and many nonschizoids, I might add) do — by acting out his repressed hostility impulsively and antisocially in a highly symbolic and regressive manner. In short, to take revenge against his mother and sister, Mr. G unconsciously perceived his victims as surrogates and killed them after they (once again, symbolically as well as realistically) mistreated and disappointed him.<sup>264</sup>

Let us examine this diagnostic formulation and its relationship to Mr. G's criminal responsibility. Bonnie and Slobogin do not claim that Mr. G was unaware of what he was doing or was unable to form the intent to kill. Indeed, the killing of the mother appears to be a classic, premeditated, intentional homicide.<sup>265</sup> The claim must be that Mr. G was substantially less able than the average person — to a legally and morally relevant degree — to control his behavior. But no scientific or normative theory linking alleged psychological causes to responsibility is provided. Attempts to do so using psychodynamic formulations are unsuccessful.<sup>266</sup> Moreover, the scientific validity and usefulness of the diagnostic information is questionable, and the diagnosis was not necessary to construct the dynamic explanation of the case.<sup>267</sup> Assuming *arguendo* the va-

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<sup>263</sup> It is not clear why an explanation was necessary, but I assume it was for purposes of sentencing.

<sup>264</sup> Bonnie & Slobogin, *supra* note 8, at 472.

<sup>265</sup> Even in a jurisdiction such as California that requires considerable forethought, see, e.g., *People v. Anderson*, 70 Cal. 2d 15, 447 P.2d 942, 73 Cal. Rptr. 550 (1968), the killing of the mother can be no less than a second degree murder.

<sup>266</sup> See *supra* notes 151-86 and accompanying text.

<sup>267</sup> Dynamicists construct a formulation on the basis of inferences from general clinical-behavioral assessment, and rarely, if ever, do they use classical disease-entity diagnoses to aid their dynamic work. Cf. Goldsmith & Mandell, *The Dynamic Formulation—A Critique of a Psychiatric Ritual*, 125 *Am. J. Psychiatry* 1738, 1740-42 (1969) (criticizing usefulness

lidity of a dynamic explanation, a separate diagnosis will not be of further use to the factfinder.

To bolster their contention that a diagnosis is useful in determining criminal responsibility, Bonnie and Slobogin offer the "finding" of a study that concluded that persons with schizoid traits may be overrepresented among sudden murderers.<sup>268</sup> Use of this study furnishes a good example, however, of how lack of scientific rigor in expert testimony can waste time and lead factfinders astray. The study is so flawed that little if anything can be reliably concluded from it,<sup>269</sup> especially because its findings, as the investigators admit, are not consistent with those of other investigations.<sup>270</sup> Moreover, even if by chance the study's findings happened to be valid — and it would be by chance — *its findings do not seem to apply to Mr. G.* The study did not find that schizoids were overrepresented among sudden murderers; instead, it concluded only that sudden murderers had certain schizoid personality traits. The study noted that sudden murderers also had other non-schizoid personality traits, and that personal isolation was common among the controls as well. Furthermore, the background characteristics of the sudden murderers were unlike those of Mr. G. Finally, it is not at all clear that Mr. G's adult personality traits

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and scientific validity of psychodynamic formulations and demonstrating that they are not good predictors of correct diagnosis). Moreover, dynamic explanations were the basis for many DSM-II categories, so there is a large risk of tautology, as Bonnie & Slobogin rightly recognize, in moving from dynamics to diagnosis and back again. Bonnie & Slobogin, *supra* note 8, at 473.

<sup>268</sup> Bonnie & Slobogin, *supra* note 8, at 472 n.139 (citing Weiss, Lamberti & Blackman, *The Sudden Murderer: A Comparative Analysis*, 2 *Archives Gen. Psychiatry* 669 (1960)).

<sup>269</sup> The study is methodologically well-meaning, but nonetheless poor. The sample size is tiny and there are insufficient controls. Extensive data were collected, but in an unstructured and unreliable fashion. Weiss, Lamberti & Blackman, *supra* note 268, at 669. Further, the investigators were not, and perhaps could not be, blind to the subject's status as an experimental or control subject. The investigators created assessment categories *after* the data were collected, and they do not report either the categories or the criteria for them. *Id.* at 670. Although the categories were quantified, placement of the data on the quantitative scales was done by consensus of the investigators rather than independently (thus, it is impossible to obtain and report reliability coefficients). *Id.* Finally, the study reports that there are significant differences between the experimental and control groups, but there is no statistical treatment of the data whatsoever. *Id.* at 672-75. The investigators recognize some of the flaws in their study, but blithely conclude that "spot checks" [of the data] "indicate" that most of the flaws probably do not undermine their findings. *Id.* at 675. This apologia is "nonsense," however.

<sup>270</sup> *Id.* at 676.

matched those of the study's sample of sudden murderers. This study is unfortunately representative of the type of weak and unhelpful scientific data that experts often rely on, but it does not advance the understanding of Mr. G, or the understanding of "sudden murderers." It would be of no assistance to criminal justice decisionmaking.<sup>271</sup>

Now let us consider the proper and efficient use of expertise in the case of Mr. G. On the questions of normality and responsibility, one simply wants as much information as possible about the defendant's behavior — his thoughts, feelings, and actions. Laypersons, such as family, friends, and co-workers, can provide this information, and skilled mental health clinicians can also efficiently gather and present the necessary behavioral data.<sup>272</sup> Armed with this data, the factfinder or sentencing judge can decide the legal/moral question of whether the defendant's abnormality and lack of rationality or self-control are sufficient to warrant the consideration of an insanity defense or sentencing mitigation. Diagnostic and other conclusory labels for behavior — for example "autistic," "childlike," and "severe" — add nothing to the fundamental assessment of behavior that the factfinder or sentencing judge must make. Moreover, using diagnostic labels and other conclusions prejudices decisionmaking about responsibility by in-

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<sup>271</sup> Another example of the use of weak and unhelpful science that Bonnie and Slobogin provide is the discussion of the Ibn-Tamas case, in which a beaten wife killed her husband and was charged with murder. Bonnie & Slobogin, *supra* note 8, at 482-84. "Scientific evidence" about battered women was adduced to cast doubt on the defendant's culpability. But the evidence used was based on "findings," which the investigator herself admits were obtained in an unscientific and nonrigorous fashion. L. Walker, *The Battered Woman* xiii (1979). Unless subjects are randomly selected and there are controls in such a study, it is by no means certain that one's findings are representative of or unique to battered women. Moreover, it would be useful to know what percentage of battered women kill. In any case, I submit, and one court has agreed, that the "findings" are matters of common sense and lay assessment. *State v. Thomas*, 66 Ohio St. 2d 518, 423 N.E.2d 137 (1981). If a defendant who has been battered kills her lover because she actually, psychologically perceives the situation as presenting her with no alternative but to kill, perhaps a defense should be allowed on that basis. But the defense should not be based on pseudo-science that obscures more than it answers. If self-defense is raised, for example, inaccurate data may obscure a necessary assessment of what the reasonable woman would have done under the circumstances. If society wishes to abandon an objective test for self-defense in such situations, it should do so openly. For an attempt to balance the issue, see generally Note, *Partially Determined Imperfect Self-Defense: The Battered Wife Kills and Tells Why*, 34 *Stan. L. Rev.* 615 (1982).

<sup>272</sup> Morse, *supra* note 2, at 611-12.

jecting all the unproven speculations about lack of control or disease processes that are inherent in psychiatric language. The question is whether crazy behavior is the cause of, or sufficiently related to, other behavior, and this question can be answered by any person with sufficient behavioral data about the defendant. For legal purposes, a label conveys nothing more than the behavior itself conveys, and it may convey far less by misleading the factfinder.<sup>273</sup>

Rather than demonstrating the need for expert diagnosis, conclusory labels, and psychodynamic formulations, the case of Mr. G establishes that laypersons, armed with sufficient information, could assess Mr. G's responsibility for the dual homicide he committed. First, Mr. G was clearly not crazy and was perfectly aware of what he was doing. No one knows whether Mr. G could have avoided killing the victims. All that is really known about the crimes is that Mr. G allowed the irrational anger that was so violently aroused by the first victim's provocative but rejecting behavior to get the best of him. Further explanations are sheer speculation.<sup>274</sup> The most one can do in assessing responsibility is to form a

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<sup>273</sup> Sometimes, however, hard data relevant to an actor's behavioral choice exist. In such cases, the factfinder should certainly have such data to help understand the actor's choice. Suppose, for instance, that the study of "sudden murderers" criticized above, see *supra* note 269, was methodologically sound and applicable to Mr. G. If the study had been properly done, it *might* have shown that there are significantly more schizoids among sudden murderers than among the population in general or than among other classes of criminals. The study might be useful because it would tend to show that being schizoid is somewhat predisposing to sudden murder. Of course, to have a full picture of how predisposing being schizoid is, one would also want to know how many schizoids are sudden murderers. The answer, probably, is only the tiniest fraction. Wolff & Chick, *Schizoid Personality in Childhood: A Controlled Follow-Up Study*, 10 *Psychological Med.* 85, 93-94 (1980). Thus, even if almost *all* sudden murderers were schizoids, being schizoid would be only slightly predisposing to sudden murder. Nevertheless, there is something about being schizoid—presumably a condition beyond the actor's control—that increases the probability slightly that one will suddenly murder, and a jury should know this. With this information — which can be provided without using the label "schizoid," and all the behavioral observations that can be mustered, the factfinder or sentencing judge can decide if Mr. G's desire to murder was too hard to be resisted.

<sup>274</sup> Mr. G had no "good" motive for killing either the daughter or the mother, but it takes little psychological sophistication to understand that, as a result of his childhood experiences, he may have developed a high degree of irrational anger and resentment toward women, especially women who reminded him in some way of his sister or mother. Furthermore, this explanation — if an explanation is needed at all in the case of a person who appears not to meet even the threshold level of abnormality — is easily within the domain of laypersons who are armed simply with the behavioral data of Mr. G's life and the events

commonsense judgment on the basis of Mr. G's past behavior. Did he have a history of attacking women, even nonmurderously, in response to rejecting behavior? Perhaps Mr. G can generate a history of control problems in response to certain stimuli that might lead to the legal/moral conclusion that he is less responsible than most murderers because it is too hard for him to control himself in response to rejecting, "slutty" women. If so, the judge or jury may have some basis for reaching the nonscientific conclusion that perhaps Mr. G is not fully responsible and should be punished less. But, if the dual murder was an isolated incident, as it appears to have been in this case, then the problem of responsibility assessment is even harder. It is of course possible that previously controllable impulses finally overwhelmed Mr. G at the time of the murders, but it is more probable that he simply did not exert the self-control of which he was capable. This, too, the judge can decide in light of Mr. G's history and his description of his behavior at the time of the killings.

As any sensible layperson would have concluded, Mr. G was found insufficiently insane to raise the insanity defense. Despite his horrible childhood and adult resentments, and the sympathy one may therefore have for him, Mr. G clearly operated according to the usual behavioral assumptions and rules. He was not crazy or out of touch with reality, and he was capable, within reasonable limits, of unexceptional, normal behavior. By trying to give Mr. G a diagnosis in addition to describing his behavior, one only runs the risk of obfuscating the crucial normality and control issues by confounding them with disease-talk. Decisionmaking according to the *Crazy Behavior* proposals would use more scientifically honest information and would be more fair than decisionmaking according to the Bonnie and Slobogin scheme.

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in question. A sophisticated critic might object, however, that this "lay" explanation is nothing more than description. Such a criticism would be apt but unavailing for the proponent of dynamic "explanations." After all, dynamic formulations are also mere descriptions buttressed by the alleged presence of unobservable quasi-mechanical forces or unvalidated unconscious motivations. See generally Rubinstein, *Explanation and Mere Description: A Metascientific Examination of Certain Aspects of the Psychoanalytic Theory of Motivation*, in *Motives and Thought: Psychoanalytic Essays in Honor of David Rapaport* 20 (R. Holt ed. 1967).

## 2. *Ms. B and Mens Rea*

The substantive criteria for almost all crimes include a conscious mental element, the mens rea of the offense, such as intent, purpose, knowledge, or advertence to risk (recklessness). Basic considerations of fairness, as well as the federal constitution, require that the law permit criminal defendants to present any evidence, including truly "expert" mental health testimony, that bears on whether the defendant had the requisite mental state. Bonnie and Slobogin correctly note that the prosecution is able to rely on commonsense inferences about behavior to convince a factfinder that this mental state was present at the time of the offense.<sup>276</sup> After all, a claim of no mens rea is often counterintuitive in light of the defendant's observable behavior. In effect, then, the burden of proving the absence of a mens rea shifts to the defendant. Consequently, the law should give a defendant every reasonable opportunity to cast doubt on whether the necessary mens rea was present. Once again, the flaw in the Bonnie and Slobogin analysis lies in its failure to note the scientific limits of the expert testimony used to disprove the presence of mens rea.

Even severe mental disorders rarely interfere with a defendant's ability to form a mens rea, as Bonnie and Slobogin properly recognize.<sup>276</sup> In the few cases where a disorder would so interfere, the disorder generally will be sufficiently extreme to justify an insanity defense.<sup>277</sup> In brief, a mental disorder will only prevent the forma-

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<sup>276</sup> Bonnie & Slobogin, *supra* note 8, at 446 n.52, 477.

<sup>276</sup> *Id.* at 448.

<sup>277</sup> A rare example is *People v. Wetmore*, 22 Cal. 3d 318, 583 P.2d 1308, 149 Cal. Rptr. 265 (1978). Wetmore was charged with burglary but claimed that he delusionally believed he was in his own apartment with his own possessions and therefore had no intent to steal. If Wetmore is believed, he lacked the mens rea for burglary or even for simple breaking and entering, and he would clearly be not guilty under any insanity defense test. Generally, however, even the most insane defendants will have the requisite mens rea, but they will typically be motivated by crazy reasons. Thus a person who plans and executes a killing because he believes the devil has told him to do so meets the technical requirements for first degree murder, but is also legally insane.

Until recently in California, however, many murder defendants who were not legally insane could show that they lacked "malice aforethought," see, e.g., *People v. Conley*, 64 Cal. 2d 310, 411 P.2d 911, 49 Cal. Rptr. 815 (1966), or premeditation, see, e.g., *People v. Wolff*, 61 Cal. 2d 795, 394 P.2d 959, 40 Cal. Rptr. 271 (1964). These seeming exceptions to the statement in the text were the result of highly creative and dubious interpretations of the elements of murder by the California Supreme Court. In effect, that court "legislated" a mini-insanity defense as part of the elements of premeditation and murder. Consequently, a

tion of a mens rea if it renders the defendant incapable of knowing what he or she is doing, and such defendants are legally insane according to any insanity test. One may rightfully wonder why there is any dispute about the role of mental health experts in mens rea determinations if it appears that in almost no instance will mental disorder negate mens rea. But if one abandons one's reliance on disease or disorder concepts, and instead simply considers behavioral observations unencumbered by theoretical baggage, the problem — if not the resolution — becomes clear.

Determining the existence of mental states from behavioral observations can sometimes be exceedingly difficult. Most persons can remember times when they probably appeared to others to be acting intentionally, or with knowledge of the circumstances, but they were, to state it commonsensically, in "another world." For some reason they did not notice the circumstances, or made a silly or even unbelievable error in perception. For instance, people commonly will drive their cars quite competently without noticing for some length of time where they are going. They are on "automatic pilot," so to speak: at some level they are able to take account of the circumstances, but they are not consciously aware of where they are or how they are driving. To others, however, they may appear quite attentive to their driving. To take another example, a person may walk out of a bar or restaurant, whether or not drunk, and forget to pay the bill. It is perfectly conceivable, especially to the many people who have done this, that the patron simply forgot to pay rather than intentionally failed to pay. Despite the common inference others might draw from observing such behavior, the mens rea of intent or even of recklessness may be lacking. This type of situation may arise in the criminal law context, so there is reason to consider how such behavior occurs and how it may properly be proven.

There is a wide variety of explanations for slips, lapses of attention, and other behaviors that may be inconsistent with a mens rea, but the two leading contenders are psychodynamic psychology and cognitive/information processing models. The former claims, following Freud's classic exposition in *The Psychopathology of*

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defendant whose behavior met the traditional elements of murder might be found guilty only of manslaughter. See Morse, *supra* note 189, at 279-82. These cases have now been "legislatively overruled" in California. See Cal. Penal Code §§ 188-189 (West Supp. 1982).

*Everyday Life*,<sup>278</sup> that such behaviors covertly express unacceptable wishes and the defenses against them. In contrast, cognitive/information processing models liken the mind to an imperfect information gathering and processing system: it takes in and sorts correctly a great deal of data, but it also misses, misprocesses or incorrectly "outputs" a substantial amount of data.<sup>279</sup> It is bootless to argue which model is correct, because at present there is no definitive resolution: dynamicists can construct very compelling stories in individual cases, but the more controlled empirical evidence favors the cognitivists. There is no disagreement, however, about the existence of the phenomenon. The issue is how to resolve legal cases that involve such behavior.

Bonnie and Slobogin provide the case of Ms. B to examine the issue of the proper assessment of a counterintuitive claim that a particular defendant lacks *mens rea*, and they attempt to demonstrate that a psychodynamic explanation is a fine tool to aid the factfinder's decision. Ms. B was charged with *knowingly* possessing nine stolen welfare checks.<sup>280</sup> The conditions under which she possessed them would have raised the suspicions of almost any conscious, sentient person, and thus a prosecutor might successfully rely on the intuitively obvious inference that Ms. B knew the checks were stolen. Ms. B claimed, however, that she did not know the checks were stolen because she received them from a close friend of her boyfriend, who told her they were rightfully his.<sup>281</sup> If Ms. B truly believed her boyfriend's friend, she is innocent even if she is unusually credulous.

Bonnie and Slobogin try to explain and support Ms. B's credulity with a psychodynamic tale. Ms. B is a highly passive and dependent person who will go to great lengths to please and avoid conflicts with people to whom she is emotionally attached. In the situation at hand, she strongly desired to please her boyfriend and thus wanted to please his good friend. This desire to please, however, conflicted with the obvious suspiciousness of the welfare checks. To resolve this conflict, she unconsciously used certain defense mechanisms characteristic of passive, dependent persons —

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<sup>278</sup> 6 Standard Edition (1901).

<sup>279</sup> See references cited *supra* note 122.

<sup>280</sup> Bonnie & Slobogin, *supra* note 8, at 477.

<sup>281</sup> *Id.* at 478.



namely, denial and repression — to keep from her conscious awareness the illegal character of what she was doing.<sup>282</sup> Bonnie and Slobogin recognize that Ms. B does not suffer from a serious mental disorder, but they argue that such a disorder should not be required. They argue that because mental health experts have special scientific knowledge of general personality functioning, the law should permit them to offer formulations like the one offered in the case of Ms. B. They claim that such evidence is scientifically respectable and bears directly and probatively on the credibility of Ms. B's claim of *no mens rea*.<sup>283</sup>

It is possible that Ms. B is telling the truth and therefore lacked the requisite *mens rea*, but there are far more convincing and less scientifically dubious ways of supporting her claim. Bonnie and Slobogin rely on an empirical assumption when they claim that Ms. B may have been "denying" and "repressing" the suspicious character of the checks: such defenses are assumed to be "characteristic" of people with her personality traits. We do not know the data source for this assumption, nor do we know whether there are studies relevant to Ms. B's case that reliably link particular defenses to particular personality types. Perhaps the assumption is based largely on clinical wisdom. But if studies do exist, the factfinder should know the probability that a particular character type exhibits particular defenses, and under what conditions and to what degree (although such studies show only the probability of certain behavior within a class of persons that arguably includes Ms. B). In the absence of such studies, the law should not permit experts to offer conclusions based on unproven speculation or "clinical wisdom," especially where, as here, the factfinder can draw its own inferences from the observational data.<sup>284</sup>

If one is permitted to speculate, as Bonnie and Slobogin will surely allow, a more parsimonious and damning explanation may be available for Ms. B's personality and behavior in the case at hand: Ms. B detests conflict with her boyfriend, and to avoid it in this situation she simply decided to overcome her reluctance to act illegally. Although acting illegally may have provoked anxiety, there is no evidence that the anxiety was so intense that she had to

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<sup>282</sup> *Id.* at 478-79.

<sup>283</sup> *Id.* at 479-80.

<sup>284</sup> See *infra* notes 287-90 and accompanying text.

deal with it entirely unconsciously. She may have simply employed a number of other defenses, such as various forms of rationalization,<sup>285</sup> to justify her wrongful acts. For instance, she may have told herself that the friend needed the money and that the government would replace the lost checks anyway, so no one would be hurt. Indeed, she may have employed no defenses: rather, the desire to please her boyfriend may have simply overwhelmed any anxiety she felt about wrongful possession, and she may have knowingly decided to proceed with the illegal behavior.<sup>286</sup> Suppose the prosecution offered this parsimonious theory and then buttressed it with the testimony of a mental health expert. There would be no sensible way for the factfinder to resolve this conflict of theories without additional information. Clearly it would be more desirable to provide additional data to the factfinder without theoretical explanations, and allow him or her to reach his or her own conclusions about the defendant's *mens rea*.

The additional data necessary for the factfinder is simply behavioral observation. Although the debate about the comparative validity of situational and personality-trait explanations of human behavior continues,<sup>287</sup> it seems probable that characteristic personality styles do exist.<sup>288</sup> Neither the mental health expert nor lay witness needs any "scientific" explanation to assert plausibly that observations indicate that Ms. B is characteristically a "denier and

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<sup>285</sup> See J. La Planche & J. Pontalis, *supra* note 46, at 375.

<sup>286</sup> A similar and more common example is the person who buys a wristwatch from a street vendor, knowing full well that it is probably stolen.

<sup>287</sup> See, e.g., Epstein, *The Stability of Behavior: I. On Predicting Most of the People Much of the Time*, 37 *J. Personality & Soc. Psychology* 1097 (1979); Epstein, *The Stability of Behavior II. Implications for Psychological Research*, 35 *Am. Psychologist* 790 (1980); Eysenck & Eysenck, *Mischel and the Concept of Personality*, 71 *Brit. J. Psychology* 191 (1980); Mischel, *On the Interface of Cognition and Personality: Beyond the Person-Situation Debate*, 34 *Am. Psychologist* 740 (1979).

<sup>288</sup> The Grant Study of Harvard men carried out over thirty years, for instance, has demonstrated that characteristic styles can be identified reliably and are significantly linked to other variables such as physical health and occupational and social functioning. The findings of this fascinating and important study are summarized in G. Vaillant, *Adaptation to Life* (1977). Although this study used male subjects only, there is no reason to expect that women lack characteristic personality styles, albeit, perhaps styles different from those exhibited by the Grant Study males. See Vaillant, *Natural History of Male Psychological Health V. The Relation of Choice of Ego Mechanisms of Defense to Adult Adjustment*, 33 *Archives Gen. Psychiatry* 535 (1976); Vaillant, *Theoretical Hierarchy of Adaptive Ego Mechanisms: A 30-Year Follow-Up of 30 Men Selected for Psychological Health*, 24 *Archives Gen. Psychiatry* 107 (1971).

represser" under conditions of conflict with those persons she loves or is dependent upon.<sup>289</sup> The expert should be able to demonstrate the validity of the assertion with countless instances from Ms. B's life and from a probing interview that "tests" her defenses.<sup>290</sup> From the examination, the expert ought to be able to describe to the factfinder any "out of it" behavior observed when Ms. B confronted conflict about dependency. Finally, Ms. B's family members, close friends, and boyfriend ought to be able to testify about all the occasions, presumably of conflict, when Ms. B seemed to misperceive or failed to perceive the events around her. Without such a background of similar behavior, Ms. B's claim is almost completely implausible; with such a background, the claim is entirely plausible (although not necessarily true), and the factfinder would be entitled to believe it.

Without behavioral substantiation, the Bonnie and Slobogin theory is not "informed speculation." Rather, it is a scientific fable that unfairly prejudices the prosecution's case because it provides the defendant with an "out" to which she is not entitled on scientific grounds. The law must set some limit to the degree of imprecision and scientific softness that it will allow in the evidence either side offers to support its claims. Moreover, if Ms. B has a history of similar behavior, the theory is superfluous. The theoretical rationale does not change a whit the probability estimate — based on Ms. B's prior history of misperception in conflictual circumstances — that she did not *knowingly* possess stolen checks. The factfinder's final assessment ought to be based entirely on Ms. B's behavioral history and on any existing hard data relevant to persons like her.

My system may appear more costly in this case because it asks for the collection and presentation of a great deal of behavioral

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<sup>289</sup> This simply means that the person appears systematically to fail to perceive salient aspects of the environment and his or her own behavior. According to dynamic theory, these aspects of outer and personal reality create so much anxiety that the person unconsciously prevents himself or herself from perceiving them.

<sup>290</sup> "Testing the defenses" is an interviewing technique whereby the interviewer brings up or points out to the subject just those stimuli or topics that seem to create anxiety and to motivate the use of defenses. The purpose is to see how strong or rigid the defenses are, how they operate, and the degree of anxiety or dysfunction the subject evidences if the interviewer's interventions penetrate the defenses. See, e.g., Kernberg, Goldstein, Carr, Hunt, Bauer & Blumenthal, *Diagnosing Borderline Personality—A Pilot Study Using Multiple Diagnostic Methods*, 169 *J. Nervous & Mental Disease* 225, 226-27 (1981).

data. On the other hand, it is probably no more costly than a system that engages experts to work out fully and then justify a coherent formulation. Offering a theoretical explanation without rich behavioral substantiation is admittedly quicker and cheaper, but it fails to provide evidence that would sensibly aid a factfinder. Presentation of behavioral evidence plus theory is more expensive and less helpful.

### 3. *Mr. Z: Dynamic Explanations and Criminal Responsibility*

Bonnie and Slobogin present the case of Mr. Z to illustrate how mental health experts can help determine criminal responsibility. From a clinical and legal standpoint, the case of Mr. Z is the most interesting that they present because his criminal behavior is the least comprehensible.<sup>291</sup> Mr. Z was a twenty-three year old artist who had attempted six rapes in six years.<sup>292</sup> According to Mr. Z, before each attempt he felt a "powerful" though vague "impulse" which he described as follows: "there was a feeling of real power and hate and anger . . . I was really excited sexually. You know, I just felt . . . there is a word, I can't think of it."<sup>293</sup> Mr. Z selected his victims at random, and, after entering their houses through unlocked doors, he felt scared but "could not make himself leave." During his assaults, he was "horrified" at what he was doing. He never actually raped his victims because, he claimed, he was too "scared and angry with myself." On two occasions, he apologized to his victim after the assault.<sup>294</sup>

Mr. Z was an articulate, intelligent person who gave no outward signs of craziness,<sup>295</sup> but he admitted that for as long as he could

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<sup>291</sup> Professor Bonnie, the Director of the Institute of Law, Psychiatry and Public Policy, graciously allowed me to read the full "Psychiatric Evaluation" of Mr. Z (approximately 12 single spaced pages) and to view selected portions of the videotapes of the evaluation interviews. In the notes that accompany my discussion of Mr. Z, I shall cite material from these sources simply as Institute Evaluation.

<sup>292</sup> Bonnie & Slobogin, *supra* note 8, at 488.

<sup>293</sup> *Id.* at 489 & n.193.

<sup>294</sup> *Id.* at 489. During one of these two instances, Mr. Z bloodied the victim's nose when she wouldn't stop kicking and during the other he accidentally cut the victim's finger. Mr. Z would generally try to quiet his victims by threatening to kill them and by wielding a knife (usually a "butter knife" according to Mr. Z). Institute Evaluation, *supra* note 291.

<sup>295</sup> Indeed, Mr. Z's behavior during the Institute's psychiatric evaluation, as seen in the videotapes, was undeniably rational and normal. Moreover, he consistently showed and expressed appropriate affect, including evidently genuine remorse for his deeds.

remember, he had had suicidal thoughts, hated himself, and felt that he had little to offer to others.<sup>296</sup> He had friends, however, and kept a journal, and from these sources the Virginia Clinic concluded that he isolated himself socially, neglected his work, and had suffered long periods of depression.<sup>297</sup> On the basis of further history, the Clinic concluded that Mr. Z "suffered from chronic depression throughout his adolescent years and had developed abnormally low self-esteem."<sup>298</sup> This was due, the Clinic believed, to very harsh treatment from his perfectionist, demanding father, who would choke Mr. Z for trivial disciplinary infractions. Mr. Z was terrified by such incidents, and told of nightmares of his father's face "exploding" due to some unexplained transgression on Mr. Z's part.<sup>299</sup> Finally, the Clinic discovered that sexual relations between Mr. Z's parents were exceedingly strained. Mr. Z's mother apparently abhorred sex, would consent to intercourse with his father only rarely and unwillingly, and, when she did so, called the father derogatory names. As a small boy, Mr. Z saw many of these incidents and was horrified by them.<sup>300</sup>

Mr. Z clearly was not legally insane: the prosecution could easily have proven the elements of attempted rape, and Mr. Z pled guilty to several of the charges. The question in this case is whether, under a sentencing scheme that allows for a reduction in sentence based on mitigating factors, the law should consider Mr. Z less responsible and less culpable.

Bonnie and Slobogin claim, incorrectly, that under my system only the selective, conclusory, and summary material presented would be "available to the factfinder in reaching a conclusion about Mr. Z's 'normality' and the psychological explanation for his offense."<sup>301</sup> But Bonnie and Slobogin have not given the factfinder the crucial, behavioral data my system demands. They have given us no information about Mr. Z's present functioning except vague

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<sup>296</sup> The Institute Evaluation revealed that these feelings began when Mr. Z was in college, rather than earlier.

<sup>297</sup> Bonnie & Slobogin, *supra* note 8, at 489.

<sup>298</sup> *Id.* at 490.

<sup>299</sup> *Id.* at 489.

<sup>300</sup> *Id.* at 489-90. The Institute Evaluation reports that he remembered these incidents vividly and that they sounded "as if his father were raping his mother," but the report does not mention that he was horrified by the events. Institute Evaluation, *supra* note 291.

<sup>301</sup> Bonnie & Slobogin, *supra* note 8, at 490.

*conclusions* about depression and social isolation. Yet Mr. Z had friends and kept a diary. I would want to know much more about his interpersonal and occupational functioning. How did he function in school? What were the positive aspects of his relationships with his parents?<sup>302</sup> Also, we are not provided with a complete sexual history, although such information is crucial to assessing this case.<sup>303</sup> In sum, we are given no substantial developmental history and no full present functioning analysis.

Now let us turn to an examination of Mr. Z's criminal responsibility. I agree with Bonnie and Slobogin that a factfinder could not assess Mr. Z's normality solely on the basis of the behavioral data that they present. But if the full behavioral data required by my scheme were available, the factfinder would possess ample information to determine how "normal" Mr. Z was for the purpose of judging his responsibility. Still, Bonnie and Slobogin claim that experts are necessary to help the factfinder understand whether Mr. Z could have controlled himself. Without the experts, they allege, a layperson could develop only one intuitive explanatory theory — that Mr. Z "was seeking sexual gratification . . . even though the circumstances of the offenses may be inconsistent with this explanation."<sup>304</sup> A better explanatory theory is necessary because, without additional hypotheses which could be provided only by an expert, "the factfinder is likely to see no basis for questioning Mr. Z's capacity to 'control' his behavior as a normal person would."<sup>305</sup>

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<sup>302</sup> For example, his parents were terribly concerned about Mr. Z and his plight and offered their cooperation to the Institute. Institute Evaluation, *supra* note 291. Mr. Z's parents may have been difficult and not overtly affectionate parents, but they were not uncaring or unconcerned.

<sup>303</sup> The Institute Evaluation provides more helpful, although not sufficiently complete, information. Mr. Z reported that he had been scared of having an intimate relationship with a woman because he feared that women would not like him. He masturbated compulsively until he formed his first stable relationship with a girlfriend. These behaviors are not unusual, however. Mr. Z reported that he fantasized about rape while masturbating — not an uncommon fantasy — because he believed this was the only way he could have a woman, and he admitted that he masturbated so frequently in order to curb frequent powerful impulses to forcefully have intercourse with a woman. These feelings seemed to coincide with periods of extreme pressure from problems in his life. Finally, at the time of the evaluation, he had formed a stable relationship with a woman he planned to marry before his arrest. No information is provided about their sexual relationship although such information would be very useful.

<sup>304</sup> Bonnie & Slobogin, *supra* note 8, at 491 n.196.

<sup>305</sup> *Id.* Contrary to the assertion of Bonnie and Slobogin, laypersons could develop a reasonably plausible theory. Let me suggest the following explanation which is entirely consis-

No theory is necessary, however, because consideration of Mr. Z's own description of his mental state at the time of the crimes provides the factfinder with the information necessary to evaluate Mr. Z's difficulties in controlling his impulses and his criminal responsibility.<sup>306</sup> When Mr. Z was "overcome" with the "strong urges" to rape, he claims that he became "panicky." He had to have a woman or "go crazy." Sometimes he would stifle the urge by shutting himself up in his room, thinking about his girlfriend or masturbating. At other times, he allegedly could not control himself: he would "fight it back and forth" before eventually succumbing. On each occasion, he had been drinking or using drugs either before or while his urges came upon him. If Mr. Z was unable to conquer his panic, he would usually begin trying doors at neighboring apartment complexes or homes, attempting to find a woman alone. The victims were chosen entirely at random. Remember too Mr. Z's statement about his feelings just before committing his crime: "there was a feeling of real power and hate and anger . . . I was really excited sexually. You know, I just felt . . . there is a word, I can't think of it."<sup>307</sup>

If Mr. Z credibly presented this story on the witness stand or to a sentencing judge, with or without expert or lay explanation of his crimes, the jury might reasonably believe that Mr. Z had unusually difficult problems with self-control and might conclude that mitigation is the just response. Whether or not they would be correct, the decision would not be a matter for experts. No expert explanation could provide "scientific" evidence of control problems that would add anything to Mr. Z's own account. Expert opinions might provide the sentencing judge with an "authoritative" rationalization for mitigating punishment, but it would be no more than that.

Bonnie and Slobogin do not believe that laypersons are capable

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tent with the data Bonnie and Slobogin present and well within the powers of laypersons. The reason Mr. Z attempted but never completed rape was that he harbors great hatred towards women and perhaps he fears them as well. Moreover, he is very mixed up about women and sexuality and about hurting people. Furthermore, these feelings are the result of his unfortunate childhood. The very fact that he could engage in such perplexing behavior is good evidence of the degree to which he is mixed up about women, sex and hatred.

<sup>306</sup> The following material is taken from the Institute Evaluation, *supra* note 291.

<sup>307</sup> Bonnie & Slobogin, *supra* note 8, at 489 n.193. One wonders, unavailingly, what thoughts of Mr. Z were elliptically omitted. This statement is presented in the same way in the Institute Evaluation, *supra* note 291. Perhaps the ellipses were meant simply to indicate pauses.

of forming an opinion about Mr. Z's control problems and his responsibility without causal explanations for his behavior provided by experts. "We have no doubt that the motivations for his [Mr. Z's] behavior would remain obscure to the untrained layman, even if the factual information were presented."<sup>308</sup> It is unfortunately the case, however, that the motivations for Mr. Z's behavior are obscure to experts and laypersons alike. At this stage in the scientific understanding of human behavior, no one can fathom much of the human heart except in a commonsense and post hoc fashion. Probably, life in general and the assessment and control of human conduct would be more manageable if we understood behaviors to the extent Bonnie and Slobogin assume, but we do not. In a case like Mr. Z's, an evaluation of the rich available data, using commonsense and compassion, is all that one can reasonably expect.

Bonnie and Slobogin offer the dynamic formulation of Mr. Z as "an organizational and interpretational tool that neither the factfinder nor counsel could have supplied."<sup>309</sup> But laypersons can construct coherent interpretations or explanations that organize and make sense of the data just as well as the expert. Lay interpretation may not sound as mysterious, scientific, or complex, but, as I demonstrated in Section III of this article, there is little reason to credit dynamic accounts of causation.<sup>310</sup> The dynamic explanation offered by Bonnie and Slobogin does not organize and provide a more scientifically confirmed causal account of Mr. Z's life than common sense could — it only seems to do so in scientific fashion.

What is more, even if we accept psychodynamic explanations, they will not be "probative and helpful on the issue of the defendant's ability to control his conduct,"<sup>311</sup> as Bonnie and Slobogin believe, because there is no theory to link intrapsychic causal accounts of behavior to control problems.<sup>312</sup> Presumably all behavior

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<sup>308</sup> Bonnie & Slobogin, *supra* note 8, at 491.

<sup>309</sup> *Id.* at 488.

Given Mr. Z's psychosexual development, the result of Z's drive toward self-degradation was his preoccupation with sexual aggression. Seeing his parents virtually do battle each time they had intercourse led Z, on an unconscious level, to equate sex with violence and degradation. The attempted rapes can be seen as an impulsive acting out of an unconscious desire to prove himself a "bad person."

*Id.* at 490.

<sup>310</sup> See *supra* notes 84-135 and accompanying text.

<sup>311</sup> Bonnie & Slobogin, *supra* note 8, at 491.

<sup>312</sup> See *supra* notes 151-86 and accompanying text.



is intrapsychically caused. To claim that Mr. Z is intrapsychically abnormal simply begs the question.<sup>313</sup> Pseudoscientific explanations only tend to prejudice the determination of whether a defendant was able to control himself. By conceptualizing the question as one of disorder or disease, these explanations incorporate stereotypical notions of control, thereby begging the crucial legal question. Dynamic formulations, without more, are not explanations of behavior, they are simply post hoc interpretations of that behavior. They cannot tell us about the degree of impulse control problems that Mr. Z experienced; they can only help rationalize a belief that he is telling the truth and deserves mitigation.

Skilled clinicians may help the factfinder by providing useful observations about a defendant's thoughts, feelings, and actions, but their science does not afford "deep" explanation and understanding. Mr. Z's unadorned life story and description are the best foundation for his plea for mercy. If the factfinder hears the rich data that I believe is crucial, without the spurious contributions of experts, it will be able fully and fairly to determine whether a defendant is criminally responsible.

#### 4. Summary

As the case analyses demonstrate, when mental health issues are implicated in criminal justice decisionmaking, the *Crazy Behavior* proposals provide the most sound and complete data to ensure sensible and fair decisions. Defendants should have every opportunity to present reasonable and relevant evidence on the questions of mens rea and responsibility, but legal decisionmakers do not need spurious science and speculative fables in order to be fair to defendants. The *Crazy Behavior* proposals, too, will admit imprecise information into evidence, but they will help keep the imprecision within reasonable limits.<sup>314</sup> By contrast, I believe that on

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<sup>313</sup> Indeed, I had a patient whose history and dynamics matched those of Mr. Z quite closely, yet the overt behavior was very different. Although my patient was mixed up about women, anger, and sex, his "antisocial" fantasies and impulses were never acted out. Instead, he married, made a tenuous but lasting adjustment in the marriage, and pursued a reasonably successful career and social life. My patient, too, was mixed up and would very much have liked to ravish women, *but he didn't*. What is the difference between Mr. Z and my patient? Frankly, I have no idea and neither do others unless they wish to engage in conclusory, tautological reasoning.

<sup>314</sup> Information about the influence of alcohol or drugs on behavior is an example. See

grounds of scientific invalidity and legal irrelevancy, the Bonnie and Slobogin approach is uncomfortably akin to "anything goes." Even though the *Crazy Behavior* proposal is opposed to modern trends in evidence law, careful analysis of mental health issues in criminal justice decisionmaking leads to the conclusion that, in that context, the modern trend is an unwise policy and ought to be reversed. Although this will cause no loss in fairness, it will effect great gains in honesty and efficiency. To vary Freud's phrase, if the law adopts the *Crazy Behavior* proposals, where fantasies were, there fact shall be.<sup>315</sup>

#### V. CONCLUSION: RESPONSIBILITY SKEPTICISM AND METHOD VOTARISM, TERMINABLE AND INTERMINABLE

Although the climate of opinion in criminal justice has become decidedly more conservative in the last decade, the emphasis on subjective assessment in determinations of liability has shown little sign of abating. I believe the emphasis on the defendant's subjective states is entirely right. Mental states and the capacity for control are important in ascribing liability: the criminal law should be clear about the limits of responsibility and about what mental states are required for various crimes, and it should ensure highly accurate factfinding about these issues at trial and dispositional proceedings. Even under presumptive sentencing schemes, there is often some room for discretion, and it should be exercised intelligently and fairly.

Unfortunately, two harmful beliefs that are common among influential participants and observers of the criminal justice system accompany the criminal law's emphasis on subjective functioning: first, the belief that large numbers of persons are not responsible

supra note 149, however, for an explanation of why this information is not nearly as problematic as dynamic explanations.

<sup>315</sup> This, of course, is a variation of Freud's famous dictum that after therapy succeeds, "Where id was, there ego shall be." S. Freud, *New Introductory Lectures on Psycho-Analysis*, in 22 Standard Edition 57, 80 (1964).

An eminent psychiatrist has recently written:

[W]e should make every effort to be highly critical of data in the field of psychiatry and to convey this critical attitude to our residents. This means that we must be knowledgeable about the published evidence and must not accept illiteracy. . . . Why not opt for a hard and data-bound viewpoint?

Winokur, *What to Do?, or What Do We Owe Our Residents*, 15 *Biological Psychiatry* 599, 611 (1980).

for their conduct, and second, the belief that there are experts who somehow can simplify the extraordinarily complex and difficult question of criminal responsibility. Based more on compassion than on empirical data or hard analysis, the first proposition erodes the moral fabric of society and the deterrent and educative effects of the criminal law. The second proposition, which is based on credulity and an understandable need for authoritative explanations in the face of troubling perplexities, leads to misguided decisionmaking processes that surrender the responsibility for deciding hard questions to experts and compromise the integrity of the criminal justice system. We must accept the reality that often there are no scientific explanations for the behavior of individual defendants, and that common sense and compassion are our best tools for ascribing responsibility and meting out punishment. There are no panaceas for the ongoing problem of crime in our nation or for the ineffectiveness of our criminal justice process. Nevertheless, I believe a renewed emphasis on both personal responsibility for one's actions and the limitations of irrelevant expertise will have a salutary effect on crime and criminal justice.