

RATIONALITY, RESEARCH AND LEVIATHAN: LAW ENFORCEMENT-SPONSORED RESEARCH AND THE CRIMINAL PROCESS

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INTRODUCTION

The theme of this symposium is rationality and the law of evidence. It is perhaps fitting, therefore, to start by examining the concept of rationality in general, and then to ask what role (if any) rationality plays in the process of adversary litigation. After that, we will turn to the special problems presented by scientific research conducted on behalf of law enforcement for prosecution use in criminal cases.

I. RATIONALITY IN THE ADVERSARY TRIAL

The root of the term “rationality” (we are informed by the Oxford English Dictionary) is the Latin word *ratio*,¹ meaning in its most fundamental sense, a reckoning or calculation, from the Latin verb *reor*, meaning “to reckon” or “to think.”² So rationality has to do with thinking, in the sense of

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1. See 8 THE OXFORD ENGLISH DICTIONARY 168 (4th ed. 1978).

2. See D.P. SIMPSON, CASSELL'S NEW LATIN DICTIONARY 514 (Funk & Wagnalls Co. 1960).

reckoning, consciously proceeding from one thought to another, that is, reasoning. In addition, in most modern views, rationality is not taken to encompass thinking in any purely free associative sense,³ but reasoning instrumentally in regard to a goal or the solution to a problem.⁴

The main locus of formal cogitation concerning the notion of rationality is, not surprisingly, philosophy, specifically, that branch of philosophy concerned with problems of knowledge (epistemology). Rationality is only one factor in modern approaches to the problem of knowledge, a problem represented by such questions as, “when (if ever) is it proper to say that a person ‘knows’ something?” The dominant approach to these questions involves the interplay of three primary variables: belief, truth, and justification.⁵ Belief is a person’s subjective holding concerning the truth of a proposition.⁶ Truth involves the reality of the content expressed by the proposition independent of belief.⁷ Justification involves the quality of the reasons for belief.⁸ It is in regard to justification that rationality plays its role.⁹

Of course, in general epistemological theorizing, there are many deep waters and dangerous shoals. Much, if not most, modern epistemology appears to be directed toward, and driven by, the problem of radical skepticism, which asserts that it is impossible in any absolute way to establish the truth of any proposition (or even the existence of an objective reality) and hence, that real knowledge is impossible.¹⁰ While it is generally conceded that no functioning human operationalizes or consistently believes the tenets of radical skepticism,¹¹ the gnawing of this rodent on the foundation of all claims to knowledge has set the global agenda for epistemology: kill or cabin

3. Such free association might constitute a part of a rational process for the solution of some problems.

4. See, e.g., JONATHAN ST. B.T. EVANS & DAVID E. OVER, RATIONALITY AND REASONING 1 (1996) (“process by which we can apply our vast stores of knowledge to the problem at hand”); see also SUSAN HAACK, EVIDENCE AND INQUIRY 177 (1993) (reasoning well “in conformity with the agent’s goals and beliefs”); see also ROBERT NOZICK, THE NATURE OF RATIONALITY xii (1993) (rationality is a “tool”, though also “a crucial component of the self-image of the human species”). Nozick spends a section of chapter five, *Is Instrumental Rationality Enough*, developing an argument that some rationality is not instrumental, though it seems to us that the symbolic uses of rationality he identifies can be regarded as other ends.

5. See MICHAEL WILLIAMS, PROBLEMS OF KNOWLEDGE: A CRITICAL INTRODUCTION TO EPISTEMOLOGY 17 (2001).

6. See *id.* at 18.

7. See *id.*

8. See *id.* at 19-20.

9. See *id.* at 34-35. Williams associates the concept of rationality with his notion of “epistemic responsibility.”

10. See *id.* at 3-4.

11. See WILLIAMS, *supra* note 5, at 9-10, 79.

the rat in the narrowest confines. In attempting this, most epistemologists eschew the difficulties of knowledge claims in regard to metaphysical or value statements, finding the game difficult enough in regard to knowledge claims about the physical world. In this attempt, many competing approaches to justification have been put forth, including foundationalism,¹² coherentism,¹³ probabilism,¹⁴ reliabilism,¹⁵ “naturalized epistemology,”¹⁶ “social

12. Foundationalism seeks to describe the first sources of knowledge (“basic beliefs”) globally. *See id.* at 38-39; HAACK, *supra* note 4, at 14-17. Foundationalism generally requires certainty for basic beliefs if it is to resist radical skepticism, and the dominant classical tradition in epistemology required such certainty for anything to be called knowledge. *See also* WILLIAMS, *supra* note 5, at 40. Nothing so foundationally certain has been discovered that would account for most knowledge of the external world. *See* NOZICK, *supra* note 4, at 124.

13. Coherentism seeks to justify a belief based on its being embedded in a system of coherent belief. *See* HAACK, *supra* note 4, at 17-19; *see also* WILLIAMS, *supra* note 5, at 117-18. Its most prominent exponent is Laurence Bonjour. *See* LAURENCE BONJOUR, *THE STRUCTURE OF EMPIRICAL KNOWLEDGE* (1985). No one denies that lack of coherence detracts from justification, and that at least a high degree of “local coherence” for obviously inter-related ideas is to be expected of anything properly called knowledge. However, by itself, coherentism threatens to grant the honorific title of “knowledge” to highly coherent and detailed paranoid fantasies or to pseudo-sciences such as astrology.

14. “Probabilism” seeks to find justification in evidence that renders beliefs more or less probable without requiring absolute starting points. Most probabilism is thus fallibilist—it is willing to grant the title of knowledge to beliefs that might be wrong. Probabilism always carries with it a remote foundational problem regarding initial information with which to set probabilities, and Hume’s problem of induction to boot. *See* WILLIAMS, *supra* note 5, at 201-14. Nevertheless, in many contexts probabilism and its associated commitment to standards of inductive logic and Bayesian reasoning may capture the highest standards for knowledge we can hope to have. Whether they are always possible for humans or practically necessary in every context are other questions.

15. Reliabilism defines knowledge as the result of a reliable process (“truth-reliable methods”). *See id.* at 33. Reliabilism is generally taken to be externalist. People are taken to have knowledge when they reach it through the results of a reliable process whether they understand the process or not. Reliabilism becomes less externalist if it requires that the person basing beliefs on a reliable process at least have some acceptable ground for believing the process is reliable, but at this point the problems of other approaches resurface. *See id.* at 32-37.

16. “Naturalized epistemology” is a bit difficult to nail down in a short note. The notion sprang from a 1969 paper by Willard Van Orman Quine. Quine appears to suggest the incorporation in (or substitution for) epistemology of what today would be called neuroscience and cognitive psychology. *See* W.V. Quine, *Epistemology Naturalized*, in *ONTOLOGICAL RELATIVITY AND OTHER ESSAYS* 69 (1969), *reprinted in* *NATURALIZING EPISTEMOLOGY* 15 (Hilary Kornblith ed., 1985). Quine’s exact intentions were not entirely clear. *See* HAACK, *supra* note 4, at 118-35. Nevertheless, much interesting work has been done by such philosophers as Alvin Goldman under the general banner of naturalizing epistemology. *See, e.g.,* ALVIN I. GOLDMAN, *EPISTEMOLOGY AND COGNITION* (1986). Though new empirical insights on human capacity may provide relevant information that philosophy should take into account, the extent to which epistemology can be “naturalized”, given the inevitably normative nature

epistemology,”¹⁷ and various sub-schools, combinations and syntheses thereof.¹⁸ Some of these global approaches may contain fruitfully suggestive insights concerning how the policies of the law of proof might be best reconceived, restructured, or carried out. Mike Redmayne’s paper in this symposium,¹⁹ and the work of Ronald Allen and Brian Leiter elsewhere,²⁰ consider the potentials of such analysis. Fortunately for us, we do not have to swim in such deep waters. The law is a particular enterprise the very nature of which resolves some basic philosophical issues for the purposes of discourse about the law.²¹ For example, the free will-determinism debate is a profound philosophical issue, but the law as an enterprise assumes the existence of free will, choice, and responsibility. Similarly, and even more fundamentally in a common-sense way, the legal enterprise assumes the reality of the exterior physical world, evidence of which is available to the senses. As a result, it further assumes that the products of particular factfinding exercises can, at least at times, be properly said to either correspond with those exterior facts (be accurate), or not (be inaccurate). Whatever the

of criteria of justification, is open to question. See WILLIAMS, *supra* note 5, at 32-34. See generally Mike Redmayne, *Rationality, Naturalism and Evidence Law*, 2003 MICH. ST. DCL L. REV. 849. We have no position on the extent to which general epistemology can or should be naturalized. However, we do believe that the law of proof should be “naturalized”, in the sense of being responsive to the best available empirical evidence on the human capacities that are used in generating the factfinding outputs of the system. See generally Ronald J. Allen & Brian Leiter, *Naturalized Epistemology and the Law of Evidence*, 87 VA. L. REV. 1491 (2001).

17. Social epistemology sees the roots of knowledge in groups rather than individuals. Again, a leading expositor is the prolific Alvin Goldman. See ALVIN I. GOLDMAN, *KNOWLEDGE IN A SOCIAL WORLD* (1999). See also NOZICK, *supra* note 4, at 125-31.

18. Two leading such approaches are “Contextualism,” perhaps best represented by Michael Williams, and the “Foundherentism” of Susan Haack. Foundherentism seeks to combine sense experience and coherence into a single general system of justification. See HAACK, *supra* note 4, at 19-21, 73-94. Contextualism holds that the whole program of seeking a universal criterion of grounding for all knowledge is a misplaced quest, and instead seeks to identify proper criteria of justification in regard to the context of a particular enterprise of inquiry. Its focus is thus local rather than global, and social rather than atomistically individual. See NOZICK, *supra* note 4, at 98-100; see also WILLIAMS, *supra* note 5, at 159-72, 225-26. We find Williams’s version of contextualism, with its strong element of probabilism, its emphasis on bounding of knowledge by observational constraint and its default-and-challenge analysis of justification in context, to be most helpful and fruitful for examining the defensibility of knowledge claims in both the law and in science.

19. See generally Redmayne, *supra* note 16.

20. See Allen & Leiter, *supra* note 16; see also Brian Leiter, *The Epistemology of Admissibility: Why Even Good Philosophy of Science Would Not Make for Good Philosophy of Evidence*, 1997 BYU L. REV. 803.

21. We take this approach to be the proper contextualist approach to the default positions of the legal context.

problems of global correspondence theories of *knowledge* in general epistemology, it is proper to attempt to evaluate the correspondence of the outputs of the legal factfinding process to external ground truth properly assumed to exist in the context within which the law operates. The law goes further. It embraces the general proposition that an important goal of the legal process is such correspondence: “truthseeking,” “that the truth may be ascertained,” in the words of Federal Rule of Evidence 102.²²

If we accept, as previously set out, that rationality involves reasoning toward a goal or problem solution,²³ and if we accept that the legal system contextually defines the goal by which the role played by rationality in the litigation system is to be judged (truthseeking),²⁴ we still have not set out any criteria by which to recognize such “veritistic”²⁵ rationality when we see it.²⁶

It is uncontroversial to say that reasoning can be flawed in various ways. Rationality must not involve fallacious reasoning. So how is one to recognize

22. FED. R. EVID. 102.

23. The results of various psychology experiments, notably those of Daniel Kahneman and Amos Tversky, discussed *infra* note 38, indicate that there are at least some situations in which humans in general do not process information well according to the standards of logic and probability theory. How commonly this may generate important error in everyday life is currently the subject of hot debate. See *infra* note 38. The obvious fact that *homo sapiens* is an intelligent and successful species has led Jonathan Evans and David Over to posit two kinds of rationality, which they call rationality₁ and rationality₂. See EVANS & OVER, *supra* note 4. The first, which they also style “personal rationality,” describes success at achieving personal goals independent of conscious accounts, and the second, “impersonal rationality” applies to the ability to reason consciously according to the prescriptive criteria of formal reasoning rules such as logic. See *id.* They assert that in the main humans have a large capacity for rationality₁ and a limited capacity for rationality₂. See *id.* The extent to which it makes sense to associate successful processing of information resulting in the achievement of a goal independent of conscious accounts with “rationality” of any kind is problematic. It would appear to make cockroaches the most rational of animals. Suffice it to say that what the law looks for in its factfinding processes, and what we are dealing with in this article, is some version of rationality₂.

24. The process of the law also utilizes factfinders as particularized value judgment makers in regard to such issues as negligence, insanity, etc. We confine ourselves in this paper to an examination of rational norms in regard to the finding of truly empirical brute facts, such as whether a charged criminal defendant was or was not physically present during the commission of a crime.

25. Or, in Williams’s terms, “truth conducive.” See WILLIAMS, *supra* note 5, at 65.

26. While we said in *supra* note 24 that we would not deal with issues of value committed to the determination of the legal process, we are of course putting forth normative criteria when we talk about standards of rationality. See WILLIAMS, *supra* note 5, at 11, 14, 19-26. These define what (we argue) the process must do in order to qualify for the honorific label “rational,” which is an important part of both a justified belief in the accuracy of the process, and an evaluation of the process as telling the truth about itself in the promises it has made to be a truthfinding system.

fallacious reasoning, as contrasted with proper (rational) reasoning, in regard to the factfinding tasks set by the legal context?

The nature of fallacy has been a subject of inquiry at least since Aristotle,²⁷ and full accounts of the nature and sources of fallacy are not completely worked out, especially in regard to processes whose object is factual inquiry.²⁸ Two sources of fallacious reasoning appear to predominate: premises mistakes (often resulting from poor evaluation of premises and the reasons to treat them as true or possibly true²⁹) and operational mistakes

27. See generally, ARISTOTLE, LOGIC (ORGANON), reprinted in 8 GREAT BOOKS OF THE WESTERN WORLD 5 (Robert Maynard Hutchins ed., 1952).

28. See NOZICK, *supra* note 4, at 135.

29. This would be an appropriate place to distinguish “rationality” from “rationalism.” “Rationalism” as a term of art refers to a general philosophical school which posits that real knowledge proceeds from thought independent of sense data. It is usually contrasted with empiricism, which posits that all knowledge is based on sense data. In the words of Francis Bacon, “*Empiricos enim formicæ more congerere tantum et uti; Rationales autem araneorum more telas ex se conficere*” (“Empiricists are indeed like ants, they collect things and use them, but rationalists, like spiders, spin threads out of themselves.”). FRANCIS BACON, COGITATA ET VISA (1607) (English translation by Risinger). Modern philosophy tends to view this collision of views a bit like the nature-nurture debate in general, with a dominant empiricism leaving some room, *à la* Noam Chomsky, for an innate neural structure providing innate organizing principles. In this regard, a proper warrant to accept a premise, at least in regard to a question concerning the world of the senses (the “is,” a question of fact), would have to be in some sense empirically based. See generally GEORGE LAKOFF & MARK JOHNSON, PHILOSOPHY IN THE FLESH: THE EMBODIED MIND AND ITS CHALLENGE TO WESTERN THOUGHT 3-15 (1999). It should be noted that some epistemologists, while accepting the centrality of “observational constraint,” reject the possibility and the importance of a coherent general answer to the grounding problem. See WILLIAMS, *supra* note 5, at 153-55, 173-76, 178, 186; NOZICK, *supra* note 4, at 123-25.

(including logical error, fact-value confusion,³⁰ and failure to properly account for indeterminacy in the form of probability³¹).

An ideal rational process arguably would involve more than the avoidance of specific error, however. Proper framing, explicitness and skepticism must join with proper premise warrants, fact-value separation, consistency and appropriate recognition of unavoidable indeterminacy in an ideal process of rational reasoning directed toward the kind of facts with which the law deals.

Arguably, such a perfectly rational process would be infinitely detailed on all levels of abstraction. For instance, it would describe and account for the act of John punching a wall with his fist, by descriptions of the time-space co-ordinates of every atom in John's body and the door, by the biochemical

30. Aristotle put forth ideas concerning proper means of argumentation about values in the *Organon* without being very precise about the distinction between fact and value. See ARISTOTLE, *supra* note 27, at 162. The necessity to clearly distinguish and separate propositions of fact and value in rational discourse is a theme of much modern philosophy, perhaps most dramatically represented by ALFRED JULES AYER, *LANGUAGE, TRUTH AND LOGIC* (1962), a book in which Ayer took the position that once such assertions were carefully separated, assertions of value were revealed as pseudopositions which were only expressions of personal emotion about which no rational discourse could be undertaken, a position called "emotivism". (On the role of emotions in rationality, see *infra* note 39.) Needless to say, not everyone agrees, and one of the main problems of subsequent writing has been to build a convincing, if not necessary, method for linking fact to value. See, e.g., LAWRENCE C. BECKER, *ON JUSTIFYING MORAL JUDGMENTS* 136 (1973). Probably the most famous and most powerful is John Rawls's "veil of ignorance" methodology. See JOHN RAWLS, *A THEORY OF JUSTICE* (1971). Suffice it to say that works such as those of Becker and Rawls show that much in the method of rationality can be brought fruitfully to bear on issues of value. In general, this article is limited to the legal process and rationality in regard to factual questions. Rationality and its role in regard to normative issues committed to the jury, and the interplay between fact and value in the trial process, raises other, even more complex issues.

31. Formal evaluations of the concept of reason have for millennia been dominated by the notion of necessity and certain knowledge. See generally G.J. Warnock, *Reason*, in 7 *THE ENCYCLOPEDIA OF PHILOSOPHY* 83 (Paul Edwards ed., 1967). It is probably not too much to say that humans seem to lust for certainty, a lust for which there can be no (rational) satisfaction. See WILLIAMS, *supra* note 5, at 56-58. It was not until the early eighteenth century, with the publication of Jakob Bernoulli's *Ars Conjectandi* in 1713, that the indeterminacy of available knowledge in the human condition began to be given significant formal attention, though gamblers, farmers and lawyers (along with the rest of humanity) had been aware of it forever. Probability theory, as a subject, is full of surprising areas of dispute, including such questions as the relation between formal probability theory and common sense notions of probability, and the propriety of speaking of the probability of a unique past event. See generally Max Black, *Probability*, in 6 *THE ENCYCLOPEDIA OF PHILOSOPHY* 464 (Paul Edwards ed. 1967). Nevertheless, it does not seem too controversial to assert that any acceptable modern notion of rationality would have to involve careful assessment of the generally probabilistic nature of knowledge about the factual world.

processes taking place, by the physics of the mechanics involved, by the social facts that preceded, accompanied and seem to account for the punch, by the possibilities of error inherent in the apprehension of the punch, etc. Of course, humans cannot hope to obtain that level of rationality. However, given the shortness of life, part of “best available” rationality lies in arriving at defensible judgments concerning the appropriate framework and level of abstraction to be utilized as a ground for reasoning in regard to a given problem in a given context.³²

Besides being infinitely detailed, a perfectly rational process would arguably be infinitely explicit within the appropriate frame of reference. All things that could bear on a line of reasoning or a problem under investigation would be explicitly stated. Since life is too short for this, humans are apparently wired to cut through the “blooming buzzing confusion” (to use William James’s cogent phrase³³) by organizing things in broader, perhaps too often dichotomous,³⁴ categories, and by using metaphorical structures³⁵ and heuristics³⁶ to reason. This compromise with “perfect rationality” to accomplish attainable functionality is not irrational. It is not irrational to use a cookbook without understanding the principles behind it, or to decide that margarine will likely work like butter, if the meal must be made and the store

32. See STEVEN PINKER, *HOW THE MIND WORKS* 15, 335 (1997). “How stringent the standards should be for assessing reasons will depend upon what is at stake.” NOZICK, *supra* note 4, at 97-98. See also WILLIAMS, *supra* note 5, at 56. Sometimes what is irrational within one frame may become rational when viewed from a more general context. This is best illustrated by the diplomacy saw that the most rational way to behave in maximizing the likelihood of getting one’s way in negotiations is to present a risk of irrational action. See PINKER, *supra* at 407-16. One can perhaps assert that the retention of irrationality in the trial process serves rational ends of social organization by supplying catharsis which might otherwise end in extra-legal conflict. See generally Charles Nesson, *The Evidence or the Event? On Judicial Proof and the Acceptability of Verdicts*, 98 HARV. L. REV. 1357 (1985). Such considerations are generally beyond the scope of this article.

33. See WILLIAM JAMES, *THE PRINCIPLES OF PSYCHOLOGY* (1890), reprinted in 53 GREAT BOOKS OF THE WESTERN WORLD 318 (Robert Maynard Hutchins ed., 1952).

34. These dichotomies can come in two varieties (there we go again), crisp and fuzzy, and each has its own weaknesses. See PINKER, *supra* note 32, 126-29.

35. See LAKOFF & JOHNSON, *supra* note 29, at 45-73.

36. See Gerd Gigerenzer & Peter M. Todd, *Fast and Frugal Heuristics: The Adaptive Toolbox*, in SIMPLE HEURISTICS THAT MAKE US SMART 3 (Gerd Gigerenzer et al. eds., 1999). Cass Sunstein has supplied a useful definition of “heuristic”: “[h]euristics are rules of thumb, substituting a simple question for a more difficult one.” Cass R. Sunstein, *The Laws of Fear*, 115 HARV. L. REV. 1119, 1125 n.24 (2002) (reviewing PAUL SLOVIC, *THE PERCEPTION OF RISK* (2000)) (citing Daniel Kahneman & Shane Frederick, *Representativeness Revisited: Attribute Substitution in Intuitive Judgment*, in HEURISTICS OF INTUITIVE JUDGMENT: EXTENSIONS AND APPLICATIONS (T. Gilovich et al. eds., 2002)).

is closed. Given constraints of time and energy, it is the rational thing to do.³⁷ In that sense, rationality is bounded not merely by its frame of reference, but by the terms of the problem to be addressed. However, our apparent hard-wired tendencies to categorically dichotomize, to operate by heuristics, and to reason by metaphor, carry with them error possibilities that a rational process should be aware of and take into account.³⁸

37. In this sense, rationality in practice is always pragmatic. It is also appropriate to note that given both the time constraints of a dispute resolution system and the kind of human issues which are the stuff of most litigation (at least as to factual issues), the generally appropriate frame of rationality for the enterprise might best be characterized as not-so-naïve naïve realism, and common sense empiricism leavened with uncommon good sense about its limits. See Ronald J. Allen, *Common Sense, Rationality, and the Legal Process*, 22 *CARDOZO L. REV.* 1417, 1426-27 (2001).

38. One current extremely important debate concerns the extent to which such error possibilities manifest themselves under various conditions encountered in the modern world. Tversky and Kahneman have shown that humans manifest a substantial “probability blindness” under various test conditions. See Amos Tversky & Daniel Kahneman, *Judgment Under Uncertainty: Heuristics and Biases*, in *JUDGMENT UNDER UNCERTAINTY: HEURISTICS AND BIASES* (Daniel Kahneman et al. eds., 1982). (Perhaps it is not totally surprising that formal consideration of probability emerged so late in human intellectual history). However, Gerd Gigerenzer asserts that the poor performance of people under test conditions shown by Tversky and Kahneman is more an artifact of the artificiality of the way the information is presented in the tests than a function of inaccurate judgement in normal conditions. See Gigerenzer & Todd, *supra* note 36; see also Gerd Gigerenzer, *How to Make Cognitive Illusions Disappear: Beyond “Heuristics and Biases”*, in 2 *EUR. REV. SOC. PSYCHOL.* 83 (Wolfgang Stroebe & Miles Hewstone eds., 1991). However, as one of us has recently written:

The debate appears to be a debate over whether our cognitive cup is half or more empty, or half or more full, since both sides concede that there are some problems we solve well, and some problems we deal with poorly. . . . Our mental *armamentum* of default heuristics has obviously served us well in the environment in which we evolved and though it continues to serve us well in most situations, societal and technological evolution have placed us in a context in which more of our store of historically helpful heuristics may be counterproductive when applied to modern tasks.

D. Michael Risinger & Jeffrey L. Loop, *Three Card Monte, Monty Hall, Modus Operandi and “Offender Profiling”*: Some Lessons of Modern Cognitive Science for the Law of Evidence, 24 *CARDOZO L. REV.* 193, 196 n.10, 198 n.13 (2002). The kind of information processing called for in the rather artificial context of the courtroom may present problems analogous to Tversky and Kahneman’s test conditions. See the discussions in PINKER, *supra* note 32, at 343-51 and LAKOFF & JOHNSON, *supra* note 29, at 527-28.

Also, since rationality seeks to neither overvalue nor undervalue claims about premises,³⁹ a probing and therefore somewhat skeptical⁴⁰ initial response to new information and new claims would appear to be a desirable component of rational reasoning.⁴¹ As for generalizations and extensions, rational reasoning is conservative. Speculation and the generation of hypotheses need not be constrained as long as the difference between hypothesis and fact is maintained.

Finally, although there is nothing in the nature of rationality that demands dialogue over monologue, it seems reasonable to believe that rationality can, in the aggregate, be improved by group interactions,⁴² but only as long as the interactions are of a group dominantly and explicitly committed to pursuing rationality in regard to whatever is under consideration.

So we take veritistic rationality to be a process of reasoning that attempts to be careful, to be mildly skeptical, to be conservative in generalization, to identify premises, to take care to examine the reasons for accepting premises, to consciously separate notions of fact and value (is and ought), to be consistent, to avoid logical fallacy, and to properly account for probability and

39. A phrase about what rationality “seeks” raises the issue of the role of emotion in the pursuit of rationality. Emotions are value-charged responses, positive or negative, and as such are not necessarily in conflict with rationality. The real question is, what do you value? An emotional commitment to rationality plays a role in rational tasks, virtually of necessity, since without some emotional response to the task there would be no reason to muster the effort to do it at all. See DANIEL GOLEMAN, *EMOTIONAL INTELLIGENCE* 52-54 (1995); see also MELVIN J. LERNER, *THE BELIEF IN A JUST WORLD: A FUNDAMENTAL DELUSION* 10 (1980); PINKER, *supra* note 32, at 363-424. While emotion driving towards ends other than or regardless of rationality can be the enemy of the rational process, emotion can also be its friend. Rationality can be pleasurable, satisfying, even exciting.

40. The kind of skepticism Williams refers to as “ordinary” skepticism, as opposed to “radical” or “philosophical” skepticism. “Ordinary scepticism is *demanding* and *selective*. Philosophical scepticism [is] radical and general It is not simply different from but *precludes* scepticism of the ordinary kind.” WILLIAMS, *supra* note 5, at 60.

41. In the words of Francis Bacon, “it is the peculiar and perpetual error of the human understanding to be more moved and excited by affirmatives than negatives, whereas it ought duly and regularly to be impartial; nay, in establishing any true axiom the negative instance is the most powerful.” FRANCIS BACON, *NOVUM ORGANUM*, BOOK I, 109, point 46 (1620), *reprinted in* 30 *GREAT BOOKS OF THE WESTERN WORLD* 110 (Robert Maynard Hutchins ed., 1952).

42. This is one aspect of various approaches to “social epistemology.” See *supra* note 17. One need not go as far as Nozick claims Habermas went in holding rationality to be impossible outside of groups. See NOZICK, *supra* note 4, at 125, star footnote, to believe that groups can often have truth conducive effects on reasoning by virtue of pooled knowledge and separation of viewpoint, though it depends heavily on the characteristics of the individuals involved. Checking your possibility of error with another is a generally good thing, though by no means a guarantee.

indeterminacy. In recognition of the demands of complexity and the shortness of life, rationality may make use of organizing categories, metaphors and heuristics, but it attempts to do so consciously and carefully, aware of their limitations.

Viewed in this way, it seems reasonable to believe that, while humans are capable of such rational thought, beyond a certain point it requires effort. Like knowledge itself, rationality as an ideal is like a coordinate being approached by an asymptotic function. We are always somewhere along the line described by the function. We cannot attain rationality, but we can often know that we have improved our position relative to the unreachable abscissa.⁴³ While it seems reasonable to believe that the conclusions of rationality have a special claim to reliability (at least in regard to the circumstances of the physical world) improvements in rationality don't come easy past a certain point.

In setting out the preceding ruminations on desiderata, we do not presume to define rationality ineluctably for good and all. We claim neither great sophistication nor sufficient detail of exposition to identify the many problems remaining, nor are we wedded to the completeness of our list of factors bearing on rationality, or to the particular role of any one of them. However, we do believe that something like what we have set out captures the central notion of veritistic rationality in context close enough for current purposes. The point of all this is that some human enterprises, such as science, are committed to pursuing such rationality as a central part of their methodology. At least at the trial level, law is not one of these.⁴⁴

This circumstance is a virtually inevitable consequence of our partisan adversary system of trial. An advocate in litigation is not merely allowed to, but morally obliged to, proffer the most effective evidence and marshal the most effective arguments possible, to the end of obtaining victory for her client. There is no professional obligation to honor rationality in pursuit of truth, but rather, to employ rationality, if at all, in pursuit of winning. And litigators are the ultimate pragmatists in this regard. If phrenologists were admissible and effective with juries, any responsible litigator would use one even if she personally (and rationally) thought they were bunk. Indeed, she probably would be violating her professional obligation to her client to *forego* a phrenologist. The same goes for methods of argument. If she can convince the jury in front of her by fallacious argument, she is allowed to, encouraged

43. See NOZICK, *supra* note 4, at 112-13.

44. Another way of saying this is that it would be irrational for an observer to conclude from looking at the arrangements for litigation that the law was set up in such a way as to promote optimal rationality from the factfinders employed by the process.

to, perhaps even obliged to do so.⁴⁵ As regards winning, it would be the (non-veritistically⁴⁶) rational thing to do.

This is not to say that rationality in pursuit of truth has nothing to do with trials. Rational discourse can be effective advocacy. When a litigator judges this to be the case she will use it. However, as masters of rhetoric have known since classical times, appeals to reason are only one available implement in the toolkit of persuasion,⁴⁷ and not every apparent appeal to reason would meet the criteria of rational discourse upon close examination. And irrationality is not an available objection to an argument made by an advocate to a factfinder in court.⁴⁸

But doesn't the litigation system hold itself out as committed to rationality in pursuit of truth, at least in some major, important and official way? After all, as we have already pointed out, Federal Rule of Evidence 102 does say that the evidence rules are there "to the end that the truth may be ascertained."⁴⁹ The reference to truth must be taken to imply some commitment to rationality, or at least to a claim that the results of the system

45. See Carrie Menkel-Meadow, *The Trouble with the Adversary System in a Post-Modern, Multi-Cultural World*, 1 J. INST. FOR STUDY LEGAL ETHICS 49, 74 (1996). This is most forcefully put in the words of the MODEL CODE OF PROF'L RESPONSIBILITY Canon 7 (1970) ("A lawyer should represent a client zealously within the bounds of the law"). The MODEL RULES OF PROF'L RESPONSIBILITY R. 1.3 (1983) (Diligence) waters this language down considerably, appearing to allow individual attorney judgment. ("[A] lawyer is not bound . . . to press for every advantage that might be realized for a client . . . The lawyer's duty to act with reasonable diligence does not require the use of offensive tactics." *Id.*) Nevertheless, when it comes to proffering evidence or making arguments which the system regards as competent, we are confident that most litigators will do it when it aids their client. It should also be noted that nothing in the special responsibilities of prosecutors under CPR DR 7-103 or Model Rule 3.8 imposes any greater restriction on prosecutors in this regard, as long as there is probable cause for the prosecution as a whole.

46. Such rational action would be veritistic only in the sense of accurately predicting the likely result of the game.

47. See generally, EDWARD P.J. CORBETT, *CLASSICAL RHETORIC FOR THE MODERN STUDENT* (3d ed. 1990).

48. As John Henry Wigmore stated:

Consequently, nothing that the counsel may say as to the desired inference can be the giving of evidence by him; for the evidence is by hypothesis already there. His suggestions are logical, not evidential. Now in this domain of logic, it is conceded, the counsel is free from restraint during argument. His desired inferences may be forced, unnatural, and untenable; but as to this the jury are to judge; that is precisely their function. To declare the desired inference irrational is to beg the question by prejudging what the jury may believe.

6 JOHN HENRY WIGMORE, *EVIDENCE IN TRIALS AT COMMON LAW* § 1807 at 366 (James H. Chadbourn rev., 1976).

49. FED. R. EVID. 102.

should be consistent with rationality as far as determinations of actual historical or predictive fact are concerned.⁵⁰ Here lies the crux of the problem. The underlying assumption of the legal system (at least on the surface) appears to be that the collision between two interested adversaries with no loyalty to either veritistic rationality or accuracy beyond their tactical uses, will result in outcomes which both maximize accuracy and are consistent with such rationality. This is analogous to the usual account given in justification of capitalism by the more extreme of classical theorists, which boils down to the claim that competition and the marketplace can take the vice of individual competitors (greed) and turn it into a social virtue (maximized utility). In both cases, the premise seems dubious in many circumstances, and perhaps even more so in the partisan adversary context of litigation, where the self-interest of the economic market's individual consumers is replaced by the less straightforward motivations of the jury. Viewed in this way, it is easy to accept the view that the rules of evidence developed mostly as a reaction to the rise of the purer forms of adversariness in the trial system,⁵¹ apparently in an effort to control adversary abuses. They are in a sense the anti-trust laws of the proof process.⁵²

The point of this essay is not to evaluate in detail whether the evidence rules have been more effective than the Sherman Act in their respective spheres. Nor is it to bash the litigation system overmuch. Given the multiplicity of masters served by that system, its unfettered adversary nature

50. "Justice David W. Peck identified 'truth and . . . the right result' as not merely 'basic' but 'the sole objective of the judge.'" Marvin E. Frankel, *The Search for Truth: An Umpireal View*, 123 U. PA. L. REV. 1031, 1033 (1975) (quoting DAVID W. PECK, *THE COMPLEMENT OF COURT AND COUNSEL* 9 (1954)). However, Judge Frankel went on, "In principle, the paramount objective is the truth. Nevertheless, for the advocate turned judge this objective marks a sharp break with settled habits of partisanship." *Id.*; see also JEROME FRANK, *COURTS ON TRIAL: MYTH AND REALITY IN AMERICAN JUSTICE* (1949).

51. See Stephan Landsman, *The Rise of the Contentious Spirit: Adversary Procedure in Eighteenth Century England*, 75 CORNELL L. REV. 497 (1990); John H. Langbein, *Historical Foundations of the Law of Evidence: A View from the Ryder Sources*, 96 COLUM. L. REV. 1168, 1197-1202 (1996); T. P. Gallanis, *The Rise of Modern Evidence Law*, 84 IOWA L. REV. 499 (1999). Professor Damaska has a slightly different view, assigning the function of some rules to an adversary control function, some to assumptions about the nature of jury decision, and some to other characteristics of Anglo-American trial arrangements. See MIRJAN R. DAMAŠKA, *EVIDENCE LAW ADRIFT* 4 (1997).

52. The sense is that the rules are there to drive participants (in markets or in litigation) away from behavior destructive to the ends of the system while continuing to allow the motivation toward such behavior which otherwise drives the system.

may give us the greatest net veritistic rationality we can expect.⁵³ Rather, we wish to point out the distorting effects of partisanship on veritistic rationality when they are brought to bear across the cultural boundary that separates law from science.

II. NORMAL SCIENCE VERSUS LITIGATION-DRIVEN SCIENCE

Unlike the law, the culture of science as a general proposition is specifically and fully committed to rationality in the process of inquiry and conclusion. Of course, science does not completely achieve this unattainable goal, and it sometimes falls shorter than we would like to believe,⁵⁴ but nevertheless its paramount goal is unambiguous.⁵⁵

53. When our friend and sometimes co-author Mark Denbeaux read a draft of this article, he thought we were being too hard on advocates and perhaps on the whole litigation system. This footnote (and some modification of the main text) is our attempt to satisfy his concerns. First, though the adversary function as it exists is the easiest way to illustrate the weak commitment of the litigation system to rationality, it is not the only part of the system without much primary allegiance to rationality. On both the civil and criminal side, the system attempts to satisfy many goals, which may be likened to a pack of beasts competing to be fed. "Search for truth" rationality is only one of them, and not necessarily the strongest. Among the others are vindication and vengeance, compensation, satisfactory drama, protection of established order and distribution of wealth, and so on. For a fuller catalogue with sources, see D. Michael Risinger, *John Henry Wigmore, Johnny Lynn Old Chief, and "Legitimate Moral Force": Keeping the Courtroom Safe for Heartstrings and Gore*, 49 HASTINGS L.J. 403, generally and at 437 n.89 (1997-98). An attempt at a rational system without adversariness would likely result in the official (and not necessarily rational) interests of repeat players, most especially the government, dominating outcomes without regard to rationality. The adversary function is to correct for this. The result is more close to rational than any other system could be that did not purge itself of its multiplicity of functions, a purge not politically likely to happen.

It may in fact be the case that unrestrained adversarialism maximizes rationality of result, given the impossibility of designing a non-adversary system which would maintain its commitment to veritistic rationality in the face of these other pressures. Nevertheless, when specific and correctable distortions of the process away from a rational course can be pointed out, we would hope that the system as it exists would have the decency to honor its apparent formal obeisance to rationality and act to correct them. We are often disappointed.

54. Perhaps the late Stephen Jay Gould was the paramount collector in recent times of instances of such normative distortion. See, e.g., the story of Samuel George Morton's Craniometry in STEPHEN JAY GOULD, *THE MISMEASURE OF MAN* 50-69 (1981), and the story of Blumenbach's taxonomy of mankind in STEPHEN JAY GOULD, *I HAVE LANDED* 356-66 (2002), and many in between.

55. Susan Haack has recently published a brilliant book explaining the process of science as "continuous with everyday empirical inquiry," that is, common sense rationality, and thus not "privileged," but "distinguished." See SUSAN HAACK, *DEFENDING SCIENCE—WITHIN REASON*, generally and at 94-95 (2003).

In addition, science, as an enterprise, has some advantages over law in undertaking a rational program. One advantage that science has as a rational enterprise is the subject matter it has staked out as its domain of inquiry, the world at least potentially available to sense data and its analogues.⁵⁶ As to that world, messy problems of value are to be, and can in theory be, excluded. But science is an enterprise conducted by humans, with all that that entails. Value notions do creep into the way categories are conceived, questions framed, and answers given.⁵⁷ Nevertheless, the ideal is to eliminate the “ought” and leave the “is” stark, and the culture of science encourages, promotes and rewards effort toward this end.⁵⁸

In so doing, science has a second advantage over litigation. Litigation is time-bound in a way that science is not. Despite what appear to the participants to be interminable delays, law in general must provide decisions within a time frame appropriate to the resolution of human disputes upon which the course of actual lives depend. This time constraint conditions the rationality that can be achieved. Science is under no such constraints, and can live with many questions for which the most agreed-upon answer is “more research is needed.”⁵⁹

Also, science is a human enterprise with its own competitive arena and marketplace of ideas which is structured to promote rationality in the long run. Like other activity, progress in science is in part dependent on “entrepreneurship,” in the case of science, irrational commitment to the factual status of hypotheses beyond what is justified by the available evidence.⁶⁰ Nevertheless, the emphasis on testing, and the professional reward for skepticism, in general insure a process where claims do not stay ahead of evidence indefinitely.

However, while the process of science is internally strong, it is potentially seriously weakened when removed from its normal context of cultural support and exposed to outside adversary pressures. When the

56. Such analogues include data collected with the aid of sense enhancing instruments and non-human perceptors. *See id.* at 101-02.

57. *See supra* note 54. *See id.*

58. “[S]cience works because, as an insitution, it has managed to strike a delicate balance between freedom and constraint, and because its procedures, however theoretically mediated, involve interactions with nature that we do not fully control.” WILLIAMS, *supra* note 5, at 233 (citing and interpreting the model of Thomas Kuhn).

59. *See generally* Neil B. Cohen, *The Gatekeeping Role in Civil Litigation and the Abdication of Legal Values in Favor of Scientific Values*, 33 SETON HALL L. REV. 943 (2003).

60. *See* WILLIAMS, *supra* note 5, at 236. For a discussion of this phenomenon, and a summary of our views on science as a social enterprise, see D. Michael Risinger, Mark P. Denbeaux, & Michael J. Saks, *Brave New “Post-Daubert World”—A Reply to Professor Moenssens*, 29 SETON HALL L. REV. 405, 435-39 (1998).

cultures of adversary advocacy and science interact, it is often the science that suffers. Nowhere is this more obvious than in regard to litigation-driven research in general and prosecution-driven research in particular.

There has long been a suspicion that litigation-driven research suffers from threats to validity more pervasive than those of ordinary research. For instance, John Monahan and Laurens Walker have written of relatively greater dependability of “off the shelf” as opposed to “tailor made” research.⁶¹ Such a finding would hardly be surprising, even assuming perfect researcher honesty, given the pressures to satisfy the piper involved in such research, and the potentially insidious role of various observer effects in any setting where they are not carefully controlled. And some serious thought must be given to the meaning of “perfect researcher honesty” in the context of any research.

Here the experience of the law in dealing with witness credibility can constructively inform the evaluation. The personal dimension of credibility is not based merely on the rather artificial dichotomy between cold-blooded liars and truth-tellers. Rather, it is based on some evaluation of a range of accuracy-affecting subjective phenomena all of which can be more or less conscious, including such things as failure to be forthcoming and exaggeration. These can be thought of as covered by the parts of the standard witness oath referring to “the whole truth” and “nothing but the truth,” and while violations of these exhortations rarely result in prosecutable perjury, they are the rational grist for evaluating how much to reject or discount the face value of the information asserted by a witness.

These same evaluations must be made to some degree in regard to study design, data reports and evaluations of data in every research report. However, in the normal practice of science it is hoped, at any rate, that professional acculturation reduces these worries to a functional minimum. Science to this degree is based on trust, albeit a trust defensible as reasonably warranted in most contexts within the normal process of science.⁶²

Nothing undermines the conditions warranting this normal trust like partisanship. This is not to say that partisanship does not exist in constrained forms in all practice of science by humans. It is now a generally accepted commonplace that science is a group social activity whose individual outputs are in part the product of human hopes and aspirations interacting with the

61. See John Monahan & Laurens Walker, *Social Authority: Obtaining, Evaluating, and Establishing Social Science in Law*, 134 U. PA. L. REV. 477 (1986).

62. See HAACK, *supra* note 55, at 70-71.

assumed substrate of objective external empirical fact,⁶³ and this understandably gives rise to various forms of partisanship, even if it is limited to nothing more than over-valuing the importance of one's own research agenda in the grand scheme of things. However, the mental discipline and methodological requirements which constitute an important part of the culture of science may bring these human threats to validity within acceptable bounds in individuals, and the broad group nature of science (and its professional reward of the skeptical criticism of the work of others) insures that something like progress can emerge through the bias cancellation that results from multiple evaluation. However, in any context where partisanship is elevated and biases are uncanceled either by group practice or by methodological alternatives, then the foundations of the trust upon which science operates are undermined. Nowhere is this more likely to be a serious problem than in a litigation-driven research setting, because virtually no human activity short of armed conflict or dogmatic religious controversy is more partisan than litigation. In litigation-driven situations, it is the unusual expert who can resist the urge to help "his side" win.⁶⁴

Such problems inhere not only in research undertaken for use in a particular case, but also in research undertaken for use in unspecified cases to come, as long as the litigation interest of the sponsoring party is sufficiently clear. This is what differentiates litigation-driven research from much other interest-driven research. For instance, in research directed toward the U.S. Food and Drug Administration approval of drugs, the drug companies are interested not only in positive findings, but also in the discovery of dangers which might precipitate costly litigation in the future. In addition, the research will have to be conducted according to protocols set by the U.S. Food and Drug Administration, and it will be reviewed by a community of regulators who are technically competent and, in principle at any rate, appropriately skeptical. In much litigation-driven research, on the other hand, there is a single unambiguous desired result, and the results will be presented to a reviewing community (judges and juries) which is not generally science

63. For an excellent case study of the human realities of normal science practice at a high level, see JEFF GOLDBERG, *ANATOMY OF A SCIENTIFIC DISCOVERY* (1988). On the risks of error from hopes and aspirations not properly controlled for resulting in "observer effects" generally, and "need-determined perception" in particular, see D. Michael Risinger, Michael J. Saks, William C. Thompson, & Robert Rosenthal, *The Daubert/Kumho Implications of Observer Effects in Forensic Science: Hidden Problems of Expectation and Suggestion*, 90 CAL. L. REV. 1, 12-26 (2002).

64. For some egregious examples (from the testimonial rather than the research side of the street) involving offender personality profilers who were members or alumni of the FBI Behavioral Science Unit, see Risinger & Loop, *supra* note 38, at 253-77.

literate. These conditions are more like the ground conditions relating to industry sponsored research in regard to food supplements and tobacco, two areas of notoriously problematic claims.

In light of these considerations, the National Academy of Sciences is currently seeking funding for a study to evaluate the effects of adversary sponsorship on litigation-driven research.⁶⁵ However, the proposed project is currently directed toward these effects only in regard to civil litigation.⁶⁶ In this article, we wish to focus attention on an area of concern which does not appear to figure prominently in the intended NAS examination of the problems of litigation-driven research: law enforcement-sponsored research in regard to the reliability of prosecution-proffered expert evidence in criminal cases.

Not all law enforcement-sponsored research presents special litigation-driven problems. For instance, research into sub-population base-rates of various DNA markers would seem to have no litigation bias problem. The sponsoring agency has little interest in any non-accuracy-based payoff that might result from such research. At the opposite extreme is research directly into the error rates of various currently accepted forensic identification techniques which have never been subjected to any formal validity testing.

Many forces combine to raise special concerns in such areas. From the perspective of prosecution and law enforcement, any such research can only result in a net loss. This is because in these areas there is generally a carefully fostered public perception of near infallibility. Study data revealing any significant error rate under common real-world conditions undermines that carefully cultivated public perception. In addition, study data which can show deficiencies in individual practitioners threaten these individuals' continued usefulness as effective witnesses. Valid or not, however, such testimony is extremely useful to a prosecutor personally convinced of the guilt of the defendant (which, given the partisan nature of the process, is essentially every prosecutor) and willing to use whatever the law allows to convince the jury of the same thing. The loss of such evidence would be especially impactful in cases where other admissible evidence against the defendant is weak. So research results calling into question the validity of such expertise, or defining its error rates, are profoundly threatening because they undermine a powerful tool in obtaining convictions, whatever the validity of the technique, and also because they threaten the status and livelihoods of the law enforcement team members who practice the putative expertise.

65. Risinger's telephone conversation with Anne-Marie Mazza, Director of the Law, Science and Technology Program, National Academy of Sciences (Mar. 18, 2003).

66. *See id.*

It is not entirely surprising, therefore, to discover that until recently such research was uncommon, especially in regard to those forensic science claims which predated the so-called “Frye test”⁶⁷ for the admission of “novel” “scientific” evidence, and which therefore (“scientific” or not) were never “novel.” As a result, such claims had never been faced with even indirect validity inquiry in court.⁶⁸ And even in regard to those processes which had been reviewed as novel, the review often consisted of little more than making sure that there was some community that would vouch for the accuracy of the claimed process and could loosely be called “scientific.”⁶⁹

The winds of change began to blow forcefully with the decision in *Daubert v. Merrell Dow Pharmaceuticals, Inc.*,⁷⁰ of course, although it was a couple of years before the first significant *Daubert* challenge to prosecution-proffered expertise was heard,⁷¹ and there is still reason to believe that substantial resistance exists among the judiciary to applying *Daubert* and its broadened offspring, *Kumho Tire Co. v. Carmichael*,⁷² to prosecution-proffered expertise as rigorously as they have been applied to the expert proffers of civil plaintiffs.⁷³ Nevertheless, there have been some successful

67. So named for the U.S. Court of Appeals case which originally set out the approach, *Frye v. United States*, 293 F. 1013 (D.C. Cir. 1923).

68. For a discussion of the standards of admissibility used for expert evidence historically, see Mark P. Denbeaux & D. Michael Risinger, *Kumho Tire and Expert Reliability: How the Question You Ask Gives the Answer You Get*, 34 SETON HALL L. REV. 15 (2003). The initial (and Risinger says groundbreaking, since Saks is too modest to say it) examination of this problem is to be found in David L. Faigman, Elise Porter, & Michael J. Saks, *Check Your Crystal Ball at the Courthouse Door, Please: Exploring the Past, Understanding the Present, and Worrying About the Future of Scientific Evidence*, 15 CARDOZO L. REV. 1799, 1803-09 (1994).

69. See Denbeaux & Risinger, *supra* note 68, at 25.

70. 509 U.S. 579 (1993). Actually, the winds had begun to pick up in some lower courts in the years running up to *Daubert*, especially in regard to causation in toxic tort cases, though the center of gravity was still business as usual. See Denbeaux & Risinger, *supra* note 68, at 18.

71. See *United States v. Starzecpyzel*, 880 F. Supp. 1027 (S.D.N.Y. 1995) (handwriting identification).

72. 526 U.S. 137 (1999). *Kumho Tire* clarified and expanded *Daubert* in two ways. It made clear that judges have a significant gatekeeping responsibility to evaluate the reliability of all proffered expertise under Federal Rule of Evidence 702, not just proffers of “scientific” evidence, and it elucidated the requirement that such evaluation be made specifically in regard to the reliability of the expertise as it was applied in the particular case. See generally D. Michael Risinger, *Defining the “Task at Hand”: Non-Science Forensic Science After Kumho Tire Co. v. Carmichael*, 57 WASH. & LEE L. REV. 767, 767-78 (2000).

73. See generally D. Michael Risinger, *Navigating Expert Reliability: Are Criminal Standards of Certainty Being Left on the Dock?*, 64 ALB. L. REV. 99 (2000).

challenges, most notably in regard to handwriting identification expertise,⁷⁴ and the potential for challenges in other areas have made law enforcement, particularly the FBI, seek research that could be used to resist such challenges.⁷⁵ However, certain aspects of that research give reason to believe that it must be approached with great caution. Various strategies appear to have been used to insure that any positive results will be exaggerated and any negative results will be glossed over. These include: (1) placing some propositions beyond the reach of empirical research; (2) using research designs which cannot generate clear data on individual practitioner competence; (3) manipulating test procedures in such a way as to change bad results into good results; (4) refusal to share data with researchers wishing to re-analyze the data; (5) encouraging overstated interpretations of data in published research reports; (6) conditioning access to case data in FBI files on accepting a member of the FBI as co-author (at least if the researcher is not viewed as a friend); and (7) burying results which might be viewed as negative in the middle of a report, coupled with an unexplained disclaimer that the data cannot be used to infer the false positive error rate they seem to indicate.

The clearest example of the first strategy is the claim of fingerprint examiners that their technique has a “methodological error rate of zero”⁷⁶ and that any errors which occur are merely lapses on the part of individual

74. Most notably, full exclusions in *United States v. Saelee*, 162 F. Supp. 2d 1097 (D. Alaska 2001) and *United States v. Fujii*, 152 F. Supp. 2d 939 (N.D. Ill. 2000), and admission limited in various ways in *United States v. Hernandez*, 42 Fed. Appx. 173 (10th Cir. 2002), *United States v. Hidalgo*, 229 F. Supp. 2d 961 (D. Ariz., 2002), *United State v. Lewis*, 220 F. Supp. 2d 548 (S.D. W. Va. 2002), *United States v. Rutherford*, 104 F. Supp. 2d 1190 (D. Neb. 2000), *United States v. Brown*, No. CR 99-184 ABC (C.D. Cal. Dec. 1, 1999) (unpublished order), *United States v. Santillan*, No. CR-96-40169 DLJ, 1999 WL 1201765 (N.D. Cal. 1999), *United States v. Hines*, 55 F. Supp. 2d 62 (D. Mass. 1999), *United States v. McVeigh*, No. 96-CR-68, 1997 WL 47724 (D. Colo. Feb. 5, 1997), *Starzecpyzel*, 880 F. Supp. 1027. For a complete discussion of the handwriting cases, see D. Michael Risinger, *Handwriting Identification*, in DAVID L. FAIGMAN, DAVID H. KAYE, MICHAEL J. SAKS, & JOSEPH SANDERS, 3 MODERN SCIENTIFIC EVIDENCE 400 (2d ed. 2002).

75. See, e.g., National Institute of Justice: The Law and Science of Expert Testimony, U. S. Department of Justice Solicitation: Forensic Document Examination Validation Studies (June, 1998), available at <http://www.ncjrs.org/pdffiles/sl297.pdf>; National Institute of Justice, U.S. Department of Justice Solicitation: Forensic Friction Ridge (Fingerprint) Validation Studies (March, 2000), available at <http://www.ncjrs.org/pdffiles1/nij/sl000386.pdf>.

76. See the testimony of Stephen Meagher, Unit Chief of Latent Print Unit 3 of the Forensic Analysis Section of the FBI Laboratory given in *United States v. Mitchell*, 199 F. Supp. 2d 262 (E.D. Pa. 2002), and quoted in *United States v. Llera Plaza*, Nos. 98-362-10, 98-362-11, 98-362-12, 2002 WL 389163 at *14-15 (E.D. Pa. 2002). (This opinion was originally reported in the advance sheets as 188 F. Supp. 2d 549, but it was suppressed in the bound volume after Judge Pollak reversed himself in regard to his initial limitation on fingerprint evidence, so the Westlaw citation is the only one generally available).

examiners. Since the technique can never be performed except through the subjective judgment of human fingerprint examiners, it is impossible to test the asserted division of responsibility for error empirically. The claim is thus rendered unfalsifiable, but nevertheless continually invoked as part of the “scientific” basis for the process of identification by comparison of fingerprints.

As to the second strategy, one need only examine the first three FBI sponsored studies of Dr. Moshe Kam of Drexel University and his collaborators concerning the performance of handwriting identification experts to see the process at work.⁷⁷ The studies were supposed to compare the performance of ordinary persons and document examiners in the identification of handwriting as to authorship by comparison with known samples of a person’s handwriting. Instead of designing a test that would do this directly, Kam et al. adopted a roundabout design which randomly generated sorting tasks out of a large stockpile of known handwriting.⁷⁸ The results were that each individual test taken by each individual test subject, expert or non-expert, was slightly different from every other.⁷⁹ Some may have represented hard tasks and some trivial tasks.⁸⁰ This meant that, given a large enough number of such tests administered to both the expert and the lay group, one might infer that the aggregate difficulty of the set of tests taken by each group was likely to be similar, but evaluation of the performance of any individual or subset of individuals was undermined. The point of this design appears to have been to protect any low scoring FBI document examiner from being impeached by these results, in the event his individual score was discovered. Thus, any such people remained useful prosecution

77. Moshe Kam, Joseph Wetstein & Robert Conn, *Proficiency of Professional Document Examiners in Writer Identification*, 39 J. FORENSIC SCI. 5 (1994) [hereinafter Kam I]; Moshe Kam, Gabriel Fielding & Robert Conn, *Writer Identification by Professional Document Examiners*, 42 J. FORENSIC SCI. 778 (1997) [hereinafter Kam II]; Moshe Kam, Gabriel Fielding & Robert Conn, *The Effects of Monetary Incentives on Document Examination Professionals*, 43 J. FORENSIC SCI. 1000 (1998) [hereinafter Kam III].

78. The methodology of each of these tests is fully documented and critiqued in Risinger, *supra* note 74, at § 28-2.3.6.

79. *See id.* at n.161.

80. *See id.*

expert witnesses by virtue of intentionally bypassing any design that would identify them as unreliable.⁸¹

The third strategy distorts the norms of test design and interpretation to give the illusion of positive results. Two examples will suffice, both involving fingerprints. In preparing for the first seriously litigated challenge to the reliability of fingerprint identification (in the case of *United States v. Mitchell*⁸²), the government undertook a test of fingerprint examiner reliability, that is, a test of the hypothesis that all trained examiners would reach the same conclusions when shown the same set of prints. They sent copies of the prints in the case (two partial latents they claimed were Mitchell's and a rolled ten-print card from Mitchell) to crime laboratories around the country and asked whether the latent prints were from the same individual whose prints were reflected on the card.⁸³ When seven labs of the thirty-four which responded failed to reach the "correct" conclusions,⁸⁴ those labs (and only those labs) were sent annotated blow-ups of the prints and were asked to reconsider their original opinions.⁸⁵ Not surprisingly, those labs changed their opinions.⁸⁶ This supposedly proved that fingerprint examiners were unanimous in their judgments.⁸⁷

A second study generated for the same case was designed to probe the assumption that fingerprints are unique. This study computer-compared 50,000 images of individual rolled fingerprints to each other and then claimed to calculate the probability that two prints selected at random would appear

81. Other, more straightforward, research on the same question supports the not very surprising conclusion that some professional handwriting examiners are consistently better at the task than others. See, e.g., Jodi Sita et al., *Forensic Handwriting Examiners' Expertise for Signature Comparison*, 47 J. FORENSIC SCI. 1117 (2002); see also Wolfgang Conrad, *Empirische Untersuchungen über die Urteilsgüte verschiedener Gruppen von Laien und Sachverständigen bei der Unterscheidung authentischer und gefälschter Unterschriften* [*Empirical Studies Regarding the Quality of Assessments of Various Groups of Lay Persons and Experts in Differentiating Between Authentic and Forged Signatures*], 156 ARCHIV FÜR KRIMINOLOGIE 169 (1975).

82. 199 F. Supp. 2d 262 (E.D. Pa. 2002). A previous trial and conviction resulted in a reversal reported at 145 F. 3d 572 (3d Cir. 1998).

83. See generally Simon Cole, *The Myth of Fingerprints*, 10 LINGUA FRANCA 54 (2000).

84. See *id.*; see also Robert Epstein, *Fingerprints Meet Daubert: The Myth of Fingerprint "Science" Is Revealed*, 75 S. CAL. L. REV. 605, 628-29 (2002).

85. See Cole, *supra* note 83, at 54.

86. See *id.*

87. The weaknesses of this "study" are discussed in Risinger et al., *supra* note 61, at 41-42. See also SIMON A. COLE, SUSPECT IDENTITIES: A HISTORY OF FINGERPRINTING AND CRIMINAL IDENTIFICATION 284-86 (2001).

to be the same.⁸⁸ In a comment on the study written for a statistical journal, David Kaye explains the errors in the study's design and analysis, which led to a considerable overstatement of the conclusions which the data generated by the study could support.⁸⁹ Kaye attributes the problems in the research to its being "unpublished and prepared expressly for litigation."⁹⁰ He concludes by suggesting that the study provides "a lesson about probabilities generated for use in litigation: If such a probability seems too good to be true, it probably is."⁹¹

As to the fourth item, refusal to share data in violation of the ordinary norms of science, again, one need only look to the Kam studies. Kam et al. have generated four data sets on government grants, three from the FBI,⁹² and one from the Department of the Army.⁹³ One of the authors⁹⁴ has repeatedly requested the data from those studies for purposes of re-examination, and has repeatedly been denied,⁹⁵ despite the fact that the youngest of the data sets is now well over three years old and hence well beyond the usual two-year presumptive period of exclusive use,⁹⁶ and there is serious criticism of the application of even this time-bound model of exclusive use to data relevant to public policy, especially when generated through government grants.⁹⁷ There is no doubt that this is a serious violation of the usual norms of science in

88. See Epstein, *supra* note 84, at 630-32; see also David H. Kaye, *Questioning a Courtroom Proof of the Uniqueness of Fingerprints*, 71 INT'L STAT. REV. 521 (2003). Both fingerprint "studies" were undertaken for use in *Mitchell*, and the results of neither have ever been published.

89. See Kaye, *supra* note 88, at 524-28.

90. *Id.* at 524.

91. *Id.* at 528.

92. See Kam I, Kam II & Kam III, *supra* note 77.

93. See Moshe Kam et al., *Signature Authentication by Forensic Document Examiners*, 46 J. FORENSIC SCI. 884 (2001).

94. Michael J. Saks.

95. The early history of Kam's refusal to share data is set out at length in Risinger, Denbeaux, & Saks, *supra* note 60, at 431 n.89. Kam originally agreed to share his data, then changed his mind. See *id.* The FBI then agreed to provide Kam's data, but after a long delay and multiple reminders the FBI reneged. See the details set out in Risinger, *supra* note 74, at n.161. The authors and their sometime co-author Mark Denbeaux have recently renewed their requests through a letter to Dr. Kam from their counsel Debevoise & Plimpton, who have also filed a Freedom of Information Act request with both the FBI and the Department of the Army on their behalf.

96. See Jerome M. Clubb et al., *Sharing Research Data in the Social Sciences*, in SHARING RESEARCH DATA 39, 74 (Stephen E. Fienberg et al. eds., 1985) [hereinafter SHARING RESEARCH DATA] (asserting that the norm for the period of exclusive use is one to two years).

97. See *id.*

regard to data sharing.⁹⁸ Moreover, such data sharing would ordinarily be required by the terms of most government research grants and contracts.⁹⁹ Not apparently by the FBI or the Department of the Army. Perhaps even more striking is the apparent indifference of the legal system, since judges have consistently refused to order the production of the data in discovery when Dr. Kam is proffered by the government as an expert witness regarding his research, or to condition his being allowed to testify on their production.

As for encouraging overstatement in litigation-useful soundbites (strategy five), one need only consider Kam's claim that his second study would "lay to rest the debate over whether or not professional document

98. According to the Report of the Committee on National Statistics:

If all science were conducted according to an ideal, referred to by Robert Merton (1973) as the "ethos of science," then scientific findings would be made available to the entire scientific community. Since the purpose of this availability is to allow others to assess the merits of the research, the need for careful description of study procedures is implicit. We believe that, in addition, the availability of the data for scrutiny and reanalysis should be part of the presentation of results. In the past, among the best investigators and with a journal practice open to extensive description, providing data was an honored tradition. Cavendish's classic paper on the density of the earth is a prime example.

Scientific inquiry must be open, and sharing data serves to make it so. Disputes among scientists are common; without the availability of data, the diversity of analyses and conclusions is inhibited, and scientific understanding and progress are impeded.

Comm. on Nat'l Statistics, *in* SHARING RESEARCH DATA, *supra* note 96, at 3, 9-10 (citations omitted). The same principle is recognized in the forensic science community, at least in theory: "One of the most basic tenets of science is free exchange of information. Scientists believe that the free exchange of information is the only method by which the work of another scientist can be reviewed, validated, or disproved." PETER D. BARNETT, *ETHICS IN FORENSIC SCIENCE* 49 (2001).

99. Most government grants and contracts mandated data sharing at the time the Kam contracts were given, requirements which have since been strengthened. On the other hand, even if such data sharing is not mandated by law, good public policy demands its release in the absence of some good reason to withhold it, which could not very easily apply to research the results of which have been published. "Certainly data collected by government agencies, to the extent that questions of confidentiality and national interest are not present, should be readily and promptly available for research applications. The same rule should be followed for data collections commissioned for purposes of public policy and for performance evaluations." Clubb et al., *supra* note 96, at 74. Clubb et al. even question whether the conventional one to two-year period of exclusive use should apply to such data. *See id.* In the words of Recommendation 3 of the Committee on National Statistics, "Data relevant to public policy should be shared as quickly and widely as possible." Comm. on Nat'l Statistics, *supra* note 98, at 27.

examiners possess writer-identification skills absent in the general population.”¹⁰⁰ Of course, it did not.

Then there is the recent study of Dr. Sargur Srihari et al., entitled *Individuality of Handwriting*,¹⁰¹ supported by a Justice Department grant pursuant to the call for research proposals referred to above.¹⁰² This study, the authors said, was “an effort to establish the individuality of handwriting”¹⁰³ and “conducted for the purpose of establishing the individuality of handwriting,” and the results of which the authors asserted “provide the basis for the conclusion of individuality.”¹⁰⁴ The latter statement is patently false on the face of the methodology and data. Srihari et al. used a computer to compare members of a 1500 person handwriting exemplar data base as the testing ground for establishing the validity of the claim of document examiners that each person’s handwriting is identifiably unique. A moment’s reflection will suffice to show that even perfect analysis of such a data set could not establish such a proposition, especially when the computer program with which the authors examined the handwriting never achieved complete confidence in distinguishing each person’s writing from that of each other person’s writing.¹⁰⁵ Nevertheless, these claims were the subject of a significant publicity campaign,¹⁰⁶ presumably also supported by the FBI and the Justice Department.

As to strategy six on our list, the FBI has had a stated policy requiring co-authorship with an FBI employee as a condition of access to data (at least in some cases) since at least the early 1990s, when Dr. William Thompson of the University of California at Irvine and a co-author were denied access to FBI DNA case data unless they accepted such a condition.¹⁰⁷ This policy does not appear to be applied when researchers are deemed safe, from the Bureau’s

100. Kam II, *supra* note 75, at 778.

101. See Sargur N. Srihari et al., *Individuality of Handwriting*, 47 J. FORENSIC SCI. 856 (2002).

102. See *id.* at 872. See *supra* note 39.

103. Srihari et al., *supra* note 101, at 857.

104. *Id.* at 871.

105. See *id.* at 868-69. For a more extensive review of the methodology and what conclusions can and cannot be drawn from the study, see Michael J. Saks, *Commentary on Handwriting*, 48 J. FORENSIC SCI. 916 (2003).

106. See, e.g., Adam Liptak, *Prosecutors Hope New Study of Handwriting Analysis Will Silence Skeptics*, N.Y. TIMES, May 26, 2002, at 14; see also Andrew Brownstein, *Controversial Study Supports Admissibility of Handwriting*, 38 TRIAL 91 (2002).

107. Risinger’s personal telephone communication with Dr. William C. Thompson (Jan. 2002). The reluctance to allow unimpeded access to data is related to the previously noted reluctance to share data at all (which seems to be related to a general tendency toward secrecy in law enforcement and bureaucratic culture in general). This is one reason to doubt that much of sustained scientific value can come out of research embedded in law enforcement agencies.

perspective.¹⁰⁸ A dramatic example of closing the doors on “unsafe” research is the recent scuttling by the Department of Defense and the Department of Justice jointly of a National Academies¹⁰⁹ research proposal to examine a broad range of forensic science practices. This led Dr. Donald Kennedy, Editor in Chief of Science, to write a scathing editorial in that journal entitled *Forensic Science: Oxymoron?*, which concludes “these public interests—security and justice—would be furthered by more scientific and reliable technology for analyzing crimes. The mystery here is why the practitioners don’t seem to want it!”¹¹⁰

A recent example of the likely effects of such a “friends only” policy (and of the seventh strategy) can be seen in Max M. Houck and Bruce Budowle’s *Correlation of Microscopic and Mitochondrial DNA Hair*

108. This seems to have been the case with Anthony J. Pinzotto and Norman J. Finkel in regard to their study of FBI Behavioral Science Unit criminal personality profiler accuracy, *Criminal Personality Profiling: An Outcome and Process Study*, 14 LAW & HUM. BEHAV. 215 (1990). It is true that the published study showed fairly poor performance by the profilers, but the authors’ tortured attempts to recharacterize the data in a better light and their omission of some of the most damning numbers from their explicit data analyses (though they could be inferred from the data presented) apparently satisfied the Bureau. Pinzotto was later hired by the FBI to work in the Behavioral Science Unit. See 64 FBI LAW ENFORCEMENT BULL., March 1995, at 2. As indicated in the text, it seems that the policy may not be as general as Professor Thompson was led to believe, but may reflect more of a “dependable” and “undependable,” or “friends and enemies” approach. For a full analysis of the Pinzotto & Finkel study and its various characteristics, see Risinger & Loop, *supra* note 38, at 248-50.

Sometimes “safe” researchers can become unsafe. Upon retirement, FBI metallurgist James Tobin undertook studies that called into question the validity of the lead comparison process by which the FBI linked crime scene bullets to other bullets in a defendant’s possession. See William A. Tobin & Wayne Duerfeldt, *How Probative is Comparative Bullet Lead Analysis*, 17 CRIM. JUST., Fall 2003, at 27. Thereafter, the FBI contracted with the National Academies’ National Research Council to evaluate the state of knowledge on the process. The result of that evaluation found the FBI process unreliable. See National Academies News Release, Feb. 14, 2004. One may wonder if a growing awareness of the likely result of this National Academies project might have had something to do with the rejection of the broader National Academies initiative described in the text.

109. “The National Academies” is the organizational umbrella term for the National Academy of Science, the National Academy of Engineering, the Academy of Medicine, and the National Research Council.

110. Donald Kennedy, *Forensic Science: Oxymoron?*, 302 SCIENCE MAGAZINE 1625 (2003).

*Comparisons.*¹¹¹ That study dealt with an analysis of 170 hair comparisons done at the FBI laboratory between 1996 and 2000.¹¹² In each case a questioned hair sample from a real case had been visually compared to a hair sample from a known human source to try to determine whether they were sufficiently similar that they might have come from the same source.¹¹³ Subsequently, the same samples were subjected to mitochondrial DNA comparison.¹¹⁴ The authors stated that the purpose of the study was to “use mtDNA results to assess the performance of microscopic analyses.”¹¹⁵ Perhaps the most central question in such a study concerns how often a questioned hair actually comes from the known source when the human examiner declares that they are “associated,” that is, consistent in visual characteristics. Of the seventy-eight hairs in the set which had thus been declared “associated” for which DNA analysis was successfully done, nine (11.5%) were excluded as coming from the same source by mtDNA analysis.¹¹⁶ However, this result was buried in a single paragraph in the middle of the paper, followed by the statement:

These nine mtDNA exclusions should not be construed as a false positive rate for the microscopic method or a false exclusion rate for mtDNA typing: it [sic] displays the

111. See Max M. Houck & Bruce Budowle, *Correlation of Microscopic and Mitochondrial DNA Hair Comparisons*, 47 J. FORENSIC SCI. 964 (2002). Houck is a former supervising examiner for the FBI laboratory who left in 2001 and joined the faculty of West Virginia University in their forensic science program. See Press Release, West Virginia University (May 22, 2002) (on file with authors). Budowle is still with the FBI Laboratory. So, though Houck had an in-house co-author, he may have been regarded as sufficiently friendly to be allowed access to FBI data without one.

112. See Houck & Budowle, *supra* note 111, at 966.

113. See *id.*

114. See *id.* at 965.

115. *Id.* at 966.

116. See *id.* In the text the authors report the mtDNA results as nine exclusions out of eighty pairings of hairs visually associated. However, two of the eighty failed to yield usable mtDNA results (see data table 2, *id.*) so they should not be included in the universe of visual associations subject to test. It is also true, as the authors note, that just as mtDNA can exclude a person whose hair is a visual match as the source of an unknown hair, sometimes visual examination might exclude a candidate as the source of a hair that had the same mtDNA sequence as the candidate, either as the result of descent from the same maternal line, or otherwise. However, that does not affect the fact that mtDNA *exclusion* is practically absolute, and provides a perfectly proper basis for inferring a false positive rate from these data in regard to visual matching. It simply means that the rate may be mildly *understated* because of the small possibility of a coincidental *failure* to exclude by mtDNA in the seventy-eight member (eighty minus two) set of visual associations for which there were mtDNA results.

limits of the comparison of the hairs examined in this sample only and not for any hairs examined by any particular examiner in any one case.¹¹⁷

This passage unjustifiably equates the presumptive epistemic validity of human-judgment-based microscopic examination of gross features and exclusion by mtDNA testing, and asserts that there is no possibility of externally valid inferences being made from the data drawn from a fairly large sample. It was apparently inserted in an attempt to make the data unusable in the cross-examination of visual hair comparison experts in court.

Thus we have seen favorable findings with weak data and methodological warrants declared to “end all debate” and “establish uniqueness,” while unfavorable results from much stronger data are declared to have “no implications” beyond the four corners of the study reporting them. Both these extremes are seen infrequently in research contexts other than those involving research done with at least one eye on their partisan use in litigation.

We make no claim that the above examples are the result of any organized review of the literature. They are merely instances we have run across as we labored down in our little corner of the forensic science mine, where we have for years examined reliability issues in regard to various forensic identification claims. However, enough canaries have died in our corner of the mine to suggest that such law enforcement-sponsored research should be approached with caution by anyone interested in maximizing the rationality of the results of criminal prosecutions.

117. Houck & Budowle, *supra* note 111, at 966.