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THE SOCIAL CONSTRUCTION OF EXPERTISE

By Roger Koppl

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# **The Social Construction of Expertise**

Roger Koppl  
Director  
Institute for Forensic Science Administration  
Fairleigh Dickinson University  
Madison, NJ 07940, USA  
[koppl@fdu.edu](mailto:koppl@fdu.edu)

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## **Abstract**

In *The Social Construction of Reality*, Berger & Luckmann discuss the division of knowledge and the consequent emergence of experts. They contrast the stabilizing monopoly traditionally enjoyed by “universal experts” with the destabilizing competition of a modern pluralistic society. “When a particular definition of reality comes to be attached to a concrete power interest, it may be called an ideology.” The current institutions of forensic science illustrate the claim that monopoly in expertise is associated with political power. Much of the analysis of universal experts in *The Social Construction of Reality* translates nicely into an analysis of forensic science.

## **Introduction**

The title of Berger & Luckmann's book *The Social Construction of Reality* is so powerful that it has often been appropriated for uses that are not consistent with the authors' careful development of the idea. The phrase often gets a metaphysical or epistemological meaning even though their very first paragraph clearly expresses their desire to steer clear of questions about "ancient philosophical preoccupations" regarding what is real and how one can know. They explicitly invite the reader to "put quotation marks around" the terms "knowledge" and "reality" (p. 2).

Nevertheless, the phrase "the social construction of reality" is often used without a citation to its authors or, sometimes, knowledge of them. And when the book is cited, it is often given an interpretation that has at best a peripheral connection to what the authors actually said. In other words, it is a classic.

A classic is more often cited than read, which is a great pity as a rule. Unlike *Thus Spake Zarathustra*, *The Social Construction of Reality* is no exception to the rule. Social thought would be better if the people citing it would trouble themselves to read it. Nevertheless, the book's powerful influence has changed social thought, especially the sociology of knowledge, permanently and for the better.

*The Social Construction of Reality* brought Alfred Schutz's notion of "the social distribution of knowledge" (p. 16) to the sociology of knowledge. As important as the notion is, Berger & Luckmann did not give it the "central place" Schutz attributed to it. Their central concern was, as the book's title suggests, the social construction

of “reality.” They were interested, in other words, in the social processes that generate “knowledge” in society. Nevertheless, the division of knowledge is a fundamental idea for any sociology of knowledge as the contents of *The Social Construction of Reality* reflect.

The fact the knowledge is dispersed widely in civilized societies influenced some of the most architecture features of Berger & Luckmann’s classic book. Once we acknowledge the dispersion of knowledge we recognize another problem, experts. “I require not only the advice of experts, but the prior advice of experts on experts. The social distribution of knowledge thus begins with the simple fact that I do not know everything known to my fellowmen, and vice versa, and culminates in exceedingly complex and esoteric systems of expertise” (p. 46).

Berger & Luckmann are not the only scholars who have considered the problem experts pose for novices. Turner (2001, pp. 123-129) provides a valuable overview of some of this literature. He addresses the concern that expertise might threaten the democratic values of equality and neutrality. He notes that Michel Foucault and others in the tradition of “cultural studies” tend to the view that expert actions and categories “constrain” consumers “into thinking in racist, sexist, and ‘classist’ ways” (p. 126). Although Foucault’s writings are opaque to me personally, it seems fair to say that his model of “power/knowledge” (Foucault 19) operates in grand categories that are disconnected from both the individual meaning structures described carefully by Berger & Luckmann and the social processes that give rise to them. Jürgen Habermas may probably be criticized in a similar way. Turner characterizes his position as saying, “There is an unbridgeable cultural gap . . .

between the world of illusions under which the ordinary member of the public operates and worlds of 'expert cultures'" (p. 128). (Turner cites Habermas [1985] 1987, p.397.) Schiemann (2000) challenges Habermas's distinction between communicative and strategic action. Finally, in Turner's review, Merton (1976) is represented as adopting a moderate position that recognizes a problem of cognitive authority, without representing experts as *ipso facto* coercive. Merton emphasizes the "ambivalence" in the situation (Merton 1976 as cited in Turner 2001, p. 128). Merton's (1937) analysis of the social structure of one form of expertise, science, is a moderate view in which science has epistemic merit, but only because of the social structure of science rather than the personal merit of scientists.

The sociological and methodological literature on science is of relevance to the economics and sociology of experts. Butos and Koppl (2003) review this literature including Bloor (1976), Kitcher (1993), Kuhn (1970), Latour (1987), Latour & Woolgar (1979), Merton (1937), Pickering (1992), and Polanyi (1962). This literature considers research science, however, rather than expertise in general.

Adopting the perspective of veritistic social epistemology, Goldman (2001) examines the problem of "how laypersons should evaluate the testimony of experts and decide which of two or more rival experts is most credible" (p.85). He places his discussion in the context of the philosophical literature on testimony. Unlike many other philosophical analyses of the issue, however, Goldman gives some attention to the social context of testimony. He notes, for example, that novices may be able to make reliable judgments of interests and biases (pp. 104-105). Goldman

might have reached a more optimistic position if he had given even greater attention to social structure. I will note below, for example, the importance of competition in the market for expertise.

Goldman neglects ways that details of context can matter. When two competing experts present alternative interpretations of a given issue, the truth may hinge on factors a novice is capable of judging. In DNA profiling, for example, the judgment about whether the genetic information in the crime-scene sample matches that of the suspect's sample depends in part on the interpretation of an "electropherogram," which is nothing more than a squiggly line. In at least some cases, novices will be able to see for themselves whether the two squiggly lines have peaks at the same locations. In such a case, modularity in expert knowledge allows novices to make reasonable judgments without acquiring all the specialized knowledge of the contending experts.

Peart and Levy (2005) emphasize the importance of "analytical egalitarianism" for the economics of experts (Feigenbaum & Levy, 1993, 1996; Froeb & Kobayashi, 1996; Levy, 2001; Levy & Peart, 2006, 2008; Milgrom & Roberts, 1986; Peart & Levy, 2005; and Posner, 1999a, 1999b). Experts are no less influenced by ordinary incentives than people in other areas of human action. This methodological principle, which harmonized nicely with the sociological analysis of Berger & Luckmann, follows from the underlying logic of public choice theory. People are the same in commercial markets and political markets. The economics of experts pushes the same basic idea in a new direction by assuming experts are

driven by the same motives as non-experts. In particular, we must abandon the idea that experts seek only the truth without regard to motives such as fame and fortune.

Investigations such as those of Turner, Goldman, and Peart & Levy could be improved by a closer attention to the analysis in *The Social Construction of Reality*, which traces social meanings back to the individual knower and, importantly, the social processes that generate the “typifications” guiding the individual knower. Of the works we have briefly reviewed only those of the economists and of Merton pay a reasonably close attention to the social processes that create and maintain expertise. Merton’s analysis, however, does not constitute a full-blown sociology of expertise. The economists give us more of the close-grained specificity that Berger & Luckmann’s theoretical analysis calls for. And in the case of Peart & Levy that specificity is joined to a sophisticated psychology of the agent. Nevertheless, unlike Berger & Luckmann, they do not fully engage the “social stock of knowledge” in its objective and subjective dimensions. Thus, they may be somewhat insensitive to the importance of experts in not merely providing “expertise,” “advice,” “judgment,” and so on, but also in “defining reality.” As we will see below, competition in the market for expertise may do more than help to reduce bias. It may threaten a privileged group’s power define reality. Such threats may be met with strategies not easily explained with metaphors of supply and demand.

As Berger & Luckmann note, experts create a problem. We don’t know what they know, but we need to evaluate them and their knowledge claims anyway. The likely results of such evaluations are a function of the overall social structure including, importantly, the structure of the market for expertise. In competitive



markets with free entry experts may have an incentive to adapt their expert judgments to the preferences of the consumers of their advice. But not all experts are subject to entry and competition.

Berger & Luckmann recognize the possibility that experts may be privileged in the case of “universal experts” who, by definition, “claim expertise in the ultimate definitions of reality as such” (p. 117). They point out that such experts may hold “an effective monopoly,” in which case “the monopolistic tradition and its expert administrators are sustained by a unified power structure” (p. 121). Competition produces a pluralism of interpretations and a very different social situation for universal experts. When reality becomes subject to competing definitions, they say, “One can readily sympathize with the experts in the traditional definitions of reality when they think back nostalgically to the times when these definitions had a monopoly in the field” (p. 125).

The difference between competition and monopoly is fundamental in economic theory. Berger & Luckmann have shown that the difference between competition and monopoly is no less important in the context of universal experts. Their analysis of universal experts carries over largely unaltered to the context of non-universal experts. I will explore that carryover with special reference to a case I have learned something about, forensic science. If my analysis is more or less on target, then it shows that *The Social Construction of Reality* remains a valuable sourcebook for social theory in general and the sociology of knowledge in particular.

## **The division of knowledge in social thought**

F. A. Hayek is generally credited with the insight that knowledge is dispersed. Hayek seems to credit himself with this discovery when he described his essay "Economics and Knowledge," which identified the division of knowledge as the central issue in economics, as "the most original contribution I have made to the theory of economics" (Hayek on Hayek, p. 79). I think the common attribution is correct, but Socrates and others anticipated Hayek in some degree.

Socrates' Apology explains how he became a gadfly to Athens. Chaerephon had asked the oracle at Delphi whether there was anyone wiser than Socrates and, Socrates reports, "the Pythian prophetess answered that there was no man wiser." Socrates represents that he was shocked, shocked by this report and that he was driven to question his fellow Athenians to prove the god wrong. His first stop was a politician. Their exchange left Socrates thinking "he knows nothing," a plausible report given the man's profession. Other politicians were no better. He moved from politicians to poets, whom he found incapable of explaining their own works. Much like the politicians, they thought themselves wise when they were not. Finally, he arrived at the artisans who, Socrates says, "knew many fine things" and were thus wiser than Socrates. But, like the poets and politicians, "they thought that they also knew all sorts of high matters" of which they knew nothing. From this experience he draws his famous conclusion, "He, O men, is the wisest, who, like Socrates, knows that his wisdom is in truth worth nothing."

Socrates' conversations with artisans revealed a social division of knowledge. Each knew his separate art and was, in this regard, "wise." Socrates blasts "philosophical pretensions" and explicitly esteems practical, humble, workmanly knowledge over theoretical knowledge. "I found that the men most in repute were all but the most foolish; and that some inferior men were really wiser and better." In his conversations with the poets, moreover, he discovered that some knowledge is tacit. He says, "not by wisdom do poets write poetry, but by a sort of genius and inspiration." They know how to write poems, but can explain nothing of it to others.

One wonders, incidentally, whether Socrates mightn't have paid for the oracle's answer to Chaerephon's question. Herodotus records at least two instances of bribes given to the oracle (V 63, VI 66, Fairbanks 1916, pp. 40-41). Reeve (1990) gives some details of the operation of the oracle and notes that there were "two methods of consulting the oracle" (p. 29). One was expensive, "involving the sacrifice of sheep and goats," and the other cheap. The existence of an expensive method strongly suggests that Delphic pronouncements were up for sale, a suggestion that seems consistent with the material position of Delphi. Reeve says Chaerephon was "notoriously poor" and probably used the cheap method (p. 29). But if someone sent him to Delphi to get the desired answer, he might well have brought money, gifts, or livestock he could not have provided out of his own apparently meager resources. Indeed, what better agent to deliver the bribe than one "notoriously poor"?

The "noble lie" (as others have labeled it) of *Republic* was the myth that God has mixed gold with the rulers, silver with the guardians, and brass and iron with

“husbandsmen and craftsmen.” Plato’s use of the noble lie suggests that he did not fully absorb the epistemic lesson of Socrates’ *Apology*. Nor is Descartes’ *Meditation on First Philosophy* consistent with the epistemic humility implied by the division of knowledge. Bernard Mandeville and Adam Smith were more skeptical philosophers who came closer to the idea without quite arriving at it.

Mandeville says,

The Arts of Brewing, and making Bread, have by slow degrees been brought to the Perfection they now are in, but to have invented them at once, and *à priori*, would have required more Knowledge and a deeper Insight into the Nature of Fermentation, than the greatest Philosopher has hitherto been endowed with; yet the Fruits of both are now enjoy’d by the meanest of our Species, and a starving Wretch knows not how to make a more humble, or a more modest Petition, than by asking for a Bit of Bread, or a Draught of Small Beer.

He expressed appreciation for the role of informal knowledge, but without a clear recognition that such knowledge is irretrievably dispersed.

In book I, chapter I of the *Wealth of Nations*, Adam Smith gives two distinct accounts of the division of knowledge. On the one hand, the division of labor applies to “science.”

In the progress of society, philosophy or speculation becomes, like every other employment, the principal or sole trade and occupation of a particular class of citizens. Like every other employment too, it is subdivided into a great number of different branches, each of which affords occupation to a peculiar tribe or class of philosophers; and this subdivision of employment in philosophy, as well as in every other business, improves dexterity, and saves time. Each individual becomes more expert in his own peculiar branch, more work is done upon the whole, and the quantity of science is considerably increased by it.

Of more humble forms of knowledge, Smith says, "Observe the accommodation of the most common artificer or day-labourer in a civilized and thriving country, and you will perceive that the number of people of whose industry a part, though but a small part, has been employed in procuring him this accommodation, exceeds all computation." He goes into some detail on the variety of distant persons and tasks required to such "accommodation." He invites us to consider "all the knowledge and art requisite" to provide a "woollen coat" or a glass window. "[I]f we examine," he says, "all these things, and consider what a variety of labour is employed about each of them, we shall be sensible that without the assistance and co-operation of many thousands, the very meanest person in a civilized country could not be provided, even according to what we very falsely imagine, the easy and simple manner in which he is commonly accommodated."

Smith's discussion of the division of labor reveals some appreciation of the division of knowledge in two aspects. First, he has a very clear statement of the sort of knowledge "philosophers" pursue. Second, he has a rather less-clear appreciation of dispersed knowledge of time and circumstance such as merchants and artisans have. The complex division of labor, he says, "exceeds all computation." But he has not explicitly given us the divided knowledge that Berger & Luckmann develop at such length.

The notion of the division of knowledge may have received its first clear modern statement by Ludwig von Mises. In his 1920 essay, "Economic Calculation in the Socialist Commonwealth," he explains why socialist economies would not be able to match the output of capitalist economies even if all workers were

determined to work for the greater good. It would be impossible, he said, to compute relative values without the aid of a unit of calculation, i.e., money. You cannot figure out how many fuzzy slippers are worth one blast furnace unless you reduce both to some unit of value. In other words you need money. Moreover, for the attributed values to reflect the relative scarcity of fuzzy slippers and blast furnaces they must emerge from decentralized voluntary exchange. In other words, you need market prices. This argument led to the socialist calculation debate of the 1930s and 1940s. In working up his argument, apparently, Mises was driven to an explicit recognition that the knowledge driving the division of labor is decentralized and impossible to collect and compute centrally.

Mises says,

[T]he mind of one man alone—be it ever so cunning, is too weak to grasp the importance of any single one among the countless many goods of a higher order [i.e. capital goods such as blast furnaces]. No single man can ever master all the possibilities of production, innumerable as they are, as to be in a position to make straightway evident judgments of value without the aid of some system of computation. The distribution among a number of individuals of administrative control over economic goods in a community of men who take part in the labor of producing them, and who are economically interested in them, entails a kind of intellectual division of labor, which would not be possible without some system of calculating production and without economy.

Mises insight into the “intellectual division of labor” set the stage for Hayek’s development of the idea in his 1936 essay “Economics and Knowledge.” Hayek says, “Clearly there is here a problem of the division of knowledge, which is quite analogous to, and at least as important as, the problem of the division of labor. But, while the latter has been one of the main subjects of investigation ever since the

beginning of our science, the former has been as completely neglected, although it seems to me to be the really central problem of economics as a social science.” In a footnote to the phrase “division of knowledge,” Hayek quotes Mises (in the German) saying, “In societies based on the division of labor, the distribution of property rights effects a kind of mental division of labor, without which neither economy nor systematic production would be possible” ([1922] 1981, 101). In his 1945 essay on “The Use of Knowledge in Society,” Hayek clarifies that the division of knowledge applies not only to scientific knowledge, but also “the knowledge of the particular circumstances of time and place,” with respect to which, “practically every individual has some advantage over all others” because of his unique position in the division of labor.

As far as I can tell, the idea of the division of knowledge was not explicit in Alfred Schutz’s 1932 book, *The Phenomenology of the Social World*. In retrospect at least, it seems implicit in his description of “the social stock of knowledge.” But Schutz did not raise it to an independent theme until after Hayek articulated the idea. Thus, Schutz seems to have gotten the idea of the division of knowledge from Hayek.

In his essay “Common Sense and the Scientific Interpretation of Human Action” he says, “With the exception of some economists (e.g. F. A. Hayek . . . ) the problem of the social distribution of knowledge has not attracted the attention of the social scientists it merits. It opens a new field of theoretical and empirical research which would truly deserve the name of a sociology of knowledge, now

reserved for an ill-defined discipline which just takes for granted the social distribution of knowledge, upon which it is founded.” (n. 29a , vol. II, p. 15).

Berger & Luckmann provide two quotes from Schutz that capture his understanding of the division of knowledge. In them Schutz speaks of “the social distribution of knowledge” and describes knowledge as “socially distributed.” He notes that the sociology of knowledge has considered the topic “merely from the angle of the ideological foundation of truth . . . or from that of the social implications of education, or that of the social role of the man of knowledge. Not sociologists but economists and philosophers have studied some of the many other theoretical aspects of the problem” (B&L p. 16). Berger & Luckmann build on this Schutzian notion of “the social distribution of knowledge,” which he seems to have derived from Hayek.

There is some irony, perhaps, in the fact that “social constructionism” would owe so much to followers of the Austrian school of economics. The first of the Austrians, Carl Menger, defended “abstract theory” in the *methodenstreit* of the 1880s. Later Mises would say, “In the course of social events there prevails a regularity of phenomena to which man must adjust his actions if he wishes to succeed.” The sovereign (whether literal monarch or a democratic electorate) knows that it cannot suspend the law of gravity by fiat. But the sovereign rarely understands that the laws of economics are just as binding and just as independent of human will. “One must study the laws of human action and social cooperation,” Mises averred, “as the physicist studies the laws of nature” (Human Action, p. 2).



There may be little or no incongruity between the “social constructionism” of Berger & Luckmann themselves and the “naturalism” of Mises. The term “social constructionism,” however, often refers to an anti-realist view of the world that puts any notion of truth under a cloud. One textbook treatment says, for example, “the notion of ‘truth’ becomes problematic. Within social constructionism there can be no such thing as an objective fact. All knowledge is derived from looking at the world from some perspective or other, and is in the service of some interests rather than others” (Burr 1995, p.4). Lest the reader mistake his comments for nothing more than healthy skepticism, he continues, “The search for truth . . . has been at the foundation of social science from the start. Social constructionism therefore heralds a radically different model of what it could mean to do social science” (p. 4). This anti-realist position is traced back to Berger & Luckmann’s classic work. The author says, “the major social constructionist contribution from sociology is usually taken to be Berger & Luckmann’s (1966) book *The Social Construction of Reality*” (p. 7). Unfortunately, Burr gives us no hint of the important fact I noted earlier that Berger & Luckmann explicitly invite the reader to “put quotation marks around” the terms “knowledge” and “reality” (p. 2).

The term “social construction of reality” has come to identify that view that all social reality—and in some versions all reality—is socially constructed and can, therefore, be made and unmade at will. In other words, there is no scientific theory of society. Such a view, however, is in tension with the view that knowledge is dispersed. To reformulate society at will, we would have to predict the consequences of our designed institutions. Those consequences depend on the

interaction between dispersed bits of knowledge and future contingencies, which cannot be predicted without those very bits of knowledge, which, however, are unavailable to the planner if knowledge is dispersed.

Berger & Luckmann are probably not best classified as constructivists in the modern sense, although the term has an elastic meaning. They do say at one point, however, “the social world was made by men—and, therefore, can be remade by them” (p. 89). We now know what they could not know in 1966, namely, that the social world is not “made by men” if that phrase remains unqualified. Advances in ethology and psychology have shown us just how much of human social life is derived from our pre-human ancestors and is, therefore, a product of Darwinian natural selection rather than free human invention (Cosmides, Tooby & Barkow 1992). Informed opinion in 1966 held that human sexuality, “is pliable both in the objects toward which it may be directed and in its modalities of expression” (p. 49). Our current understandings of homosexuality, transgendered persons, and incest all require a model that, while more complex than certain old-fashioned models of what is “natural,” represents human sexuality as far less pliable than we could reasonably have imagined in earlier decades.

Some of the scientific presumptions of *The Social Construction of Reality* have not held up. Remarkably, however, advances in science have done little or nothing to alter the relevance or applicability of the analysis in *The Social Construction of Reality*. Their treatment of “universal experts,” for example, is powerful and worthy of close re-examination. It is useful in understanding the role of expertise more generally in society.

## Experts

Berger & Luckmann introduce experts with the problem of knowing which expert to trust. As we saw in the introduction, they say, "I require not only the advice of experts, but the prior advice of experts on experts. The social distribution of knowledge thus begins with the simple fact that I do not know everything known to my fellowmen, and vice versa, and culminates in exceedingly complex and esoteric systems of expertise" (p. 46).

How do you judge expertise without having the expertise you are judging? Berger & Luckmann identify the strategy we generally use in the "natural attitude." I assume my everyday reality conforms to that of other members of my society. "I know that there is an ongoing correspondence between *my* meanings and *their* meanings" (p. 23). I apply this strategy to experts as well. I assume, for example, that forensic science is "science" and that the forensic scientist shares my view of science as objective and reliable. We rely on the label since we don't have the expert knowledge itself.

Whether the default strategy of the natural attitude is effective in coping with expertise depends on the details of the social process governing the sort of expertise in question. If there is a tight feedback loop between the experts' claims or actions on the one hand and the public's experiences on the other hand, then a kind of

conformity is likely between the expectations of the public and the competencies of the experts.

Turner (2001) provides the useful example of the massage therapist. “The massage therapist is paid for knowledge, or for its exercise, but payment depends on the judgements of the beneficiaries of that knowledge to the effect that the therapy worked. The testimony of the beneficiaries allows a claim of expertise to be established for a wider audience” (p. 131). This type of expertise, Turner explains, “shades off into the category of persons who are paid for the successful performance of services” (p. 131).

Competition among experts may not be sufficient to keep the expectations of novices aligned with the competencies of experts if the novices cannot independently judge the results of expert advice or practice. This can be the case with experts in used cars, as Akerlof (1970) pointed out. But even when autonomous learning by consumers is difficult, entrepreneurial market processes may create effective feedback loops anyway. Used car dealers in the U.S., for example, now offer reliable warranties on their used cars, thus substantially reducing the risk of getting a lemon. Private rating agencies may be an effective surrogate for consumer judgment of outcomes. The Insurance Institute for Highway Safety, for example, probably does a good job of supplying naïve consumers with expert knowledge of automobile safety. Earl & Potts (2004) consider the “market for preferences” when demanders are interesting in the most satisfactory combinations of consumer goods and experts on such goods are engaged in entrepreneurial competition.

Berger & Luckmann focus most of their attention on universal experts. The fact that such experts claim special knowledge that applies to all things in the universe does not seem to be the driving factor in Berger & Luckmann's analysis. The driving factor seems to be the relative independence of their knowledge from either empirical control or reasoned judgment by non-experts. In the extreme, these two characteristics create a "[h]ermetic corpus of secret lore" (p. 87).

In such cases, the division of knowledge is not easily overcome. In the market for universal experts competition may not improve veracity. Berger & Luckmann note, the "practical difficulties that may arise in certain societies," when, for example, "there are competing coteries of experts, or when specialization has become so complicated that the layman gets confused" (p. 78). Pluralism in systems of expert knowledge may create "socially segregated subuniverses of meaning" in which "role-specific knowledge becomes altogether esoteric" (p. 85).

Universal experts are "the officially accredited definers of reality" in general (p. 97). But experts in modern society can acquire the power to define reality in a particular realm. "All legitimations, from the simplest pretheoretical legitimations of discrete institutionalized meanings to the cosmic establishments of symbolic universes may, in turn, be described as machines of universe maintenance" (p. 105). Thus, experts may be engaged in subuniverse maintenance.

When either the universe or a subuniverse is at stake, experts are eager to claim a monopoly and reluctant to allow competition. It is very important who is to be considered an expert. "The outsiders have to *kept out*" and the insiders "have to be *kept in*" (p. 87). Berger & Luckmann note, "there is the problem of keeping out

the outsiders at the same time having them acknowledge the legitimacy of this procedure” (p. 87).

In addition to pumping up your own expertise, you disparage the expertise of others. With nihilation “The threat to the social definitions of reality is neutralized by assigning an inferior ontological status, as thereby a not-to-be-taken-seriously cognitive status, to all definitions existing outside the symbolic universe” (p. 115). As a part of this strategy, the disparaged group may be taken to know who the “real” experts are. They do not own the truth, we do. And, “Deep down within themselves, they know that this is so” (p. 116).

A body of experts may defend its monopoly with “intimidation, rational and irrational propaganda . . . mystification,” and “manipulation of prestige symbols” (p. 87). Physicians, they note, manipulate prestige symbols, mystify, and propagandize on the power and mystery of modern medicine (p. 88). The “general population is intimidated by images of the physical doom that follows” from rejecting a doctor’s advice. “To underline its authority the medical profession shrouds itself in . . . symbols of power and mystery, from outlandish costume to incomprehensible language.” Medical insiders are kept in, that is kept from “quackery,” “not only by the powerful external controls available to the profession, but by a whole body of professional knowledge that offers them ‘scientific proof’ of the folly and even wickedness of deviance” (p. 88).

Berger & Luckmann are right to put inverted commas around the phrase “scientific proof.” Although science is one of our best sources of organized knowledge (along with common law and other codifications of tradition), it is far

from error free and medical knowledge is much less reliable than popular models of science might allow. Berger, Matthews, & Grosch (2007, pp. 4-7) give three examples of “inappropriate yet regimented research methods.” In the most striking of the three examples “run-in bias” is created by deleting adverse events prior to randomization. “In randomized treatment trials,” they explain, “it is common to pre-treat the patients with the active treatment, evaluate their outcomes, and determine which patients to randomize based upon those outcomes. Bad outcomes (even deaths) prior to randomization do not make it into the analysis and do not count against the active treatment under scrutiny” (p. 4).

Ioannidis (2005) explains “Why Most Published Research Findings Are False.” The ecology of tests in a field may drive the “positive predictive value” (PPV) of a given result below the “significance level” of the researchers’ statistical tests. He denotes by  $R$  the ratio of true relationship to objectively false relationships “among those tested in a field” (p. 0696). Thus,  $R$  depends on both the objective phenomena of the field and the ideas of researchers about the field. Those ideas determine which possible relationships are and the ratio, therefore, of true to false relationships “among those tested.” In this case the chance of a positive result being true (the PPV) is  $\frac{(1-\beta)R}{R-\beta R+\alpha}$ , where  $\alpha$  is the chance of a false positive (or Type I) error  $\beta$  is the chance of a false negative (or Type II) error (p. 0696). (Ioannidis keeps things simple by assuming these error probabilities are the same in all tests.) Clearly the PPV can be smaller than the standard  $p$ -values reported in research papers.

Reporting bias exists when researchers report what “would not have been ‘research findings,’ but nevertheless end up presented and reported as such, because of bias”

(p. 0697). Adding in a parameter to reflect reporting bias reduces the PPV further. “The probability that at least one study, among several done on the same question, claims a statistically significant research finding,” will be well below the already-reduced PPV values Ioannidis computes. Ioannidis draws several conclusions beyond general skepticism. Social scientists should humbly note two of them. First, “The greater the number . . . of tested relationships in a scientific field, the less likely the research findings are to be true.” Second, “The greater the flexibility in designs, definitions, outcomes, and analytical modes in a scientific field, the less likely the research findings are to be true” (p. 0698). It is not surprising to learn that “The greater the financial and other interests and prejudices in a scientific field, the less likely the research findings are to be true.” It is surprising, however, to learn that “The hotter a scientific field (with more scientific teams involved), the less likely the research findings are to be true.” Hotness tends to create reporting bias and a poor testing ecology.

The sorts of problems I have discussed in connection with medicine exist in other areas of specialized knowledge including forensic science, to which I now turn.

### **Forensic science**

Forensic science had enjoyed a status similar to that of the medical field. And it has used similar techniques to keep outsiders out and insiders in. It is easy to paraphrase what was said above about medicine and apply it forensic science.

Forensic scientists manipulate prestige symbols, mystify, and propagandize on the power and mystery of modern investigative science. The general population is



intimidated by images of the civic doom that follows from constraining a forensic scientist's freedom to identify the bad guys. To underline its authority the forensic science profession shrouds itself in symbols of power and mystery, from outlandish equipment to incomprehensible language. Forensic science insiders are kept in, that is kept from "quackery," not only by the powerful external controls available to the profession, but by a whole body of professional knowledge that offers them "scientific proof" of the folly and even wickedness of deviance.

The prestige and mystery of forensic science is reflected in many television shows including CSI and Forensic Files. These shows represent forensic science as a kind of infallible wizardry. Forensic science claims two powerful mantles. First, by its name it declares itself to be "science." Second, crime labs are generally organized under law enforcement agencies (NAS 2009, p. 6-1). Thus, forensic science claims the prestige of law enforcement and, indeed, of the state itself. Its power is awesome and mysterious. Without it, rapists and murders would rage uncontrolled through our now peaceful streets.

Forensic scientists often seem to see themselves as something like Plato's men silver, the guardians of the *Republic*. The silver in their nature comes not from the gods, but from their scientific training. The best and most sophisticated forensic scientists do not harbor such grandiose and ultimately silly ideas. Many workaday forensic examiners, however, seem to have been socialized to view of this sort. Moreover, some of the claims made by important figures in the field are greatly exaggerated. In particular, FBI fingerprint examiners have argued in court that their error rate is zero. Such claims of infallibility encourage an exaggerated view of the

power of forensic science to extract the truth, as does the epistemic monopoly enjoyed by the typical crime lab.

Forensic science in the U.S. today is characterized by a twofold monopoly. First, evidence is typically examined by one crime lab only. In this sense the crime lab receiving a bit of evidence has a monopoly on examination of that evidence. Second, that same lab will normally be the only one to offer an interpretation of the results of the examination it performs. No other experts in forensic science will be asked to judge what the evidence means.

The prestige of forensic science once went nearly unchallenged. Recent years have seen increasingly effective challenges to the epistemic claims of forensic science. A growing body of evidence supports the view that the monopoly forensic science has not generated reliable results. Forensic science errors and false or misleading forensic science testimony may be contributing to over 20,000 false felony convictions per year in the United States. Saks & Koehler (2005) examined 86 cases where people were wrongly convicted. There were forensic science testing errors in 63 percent of the cases and false or misleading testimony by forensic scientists in 27 percent of the cases. Michael Risinger (2007) has shown that the wrongful conviction rate for rape-murders in the 1980s is at least 3.3 percent, and likely higher. With over a million felony convictions a year, these numbers point to over 20,000 false felony convictions per year attributable at least in part to forensic

science testing errors.<sup>1</sup> Even if this estimate is twice the true value, we are still looking at over 10,000 felony convictions per year.

Faulty forensic science played a role in the wrongful execution of Cameron Todd Willingham in Texas in 2004. Charged with murdering his three small children by arson, he was convicted with forensic techniques that were current at the time of the fire in 1991, but had been discredited by the time of his execution in 2004 (Mills and Possley, 2004; Willingham v. State, 897 S.W.2d. 351, 357, Tex.Crim.App. 1995). Beginning with investigation of the John Kennedy assassination, the FBI performed “compositional analysis of bullet lead” (CABL), better known as “bullet-lead analysis.” In this technique, “The FBI examiner takes three samples from each bullet or bullet fragment and analyzes them . . . to determine the concentrations of seven selected elements” (NRC 2004, p. 1). These concentrations were compared with those for bullets associated with the suspect. “If any of the fragments and suspect’s bullets are determined statistically to be analytically indistinguishable for each of the elemental concentration means, the examiner’s expert court testimony currently [2004] will indicate that the fragments and bullets probably came from the same ‘source’”(NRC 2004, p. 2). A 2004 study concluded, however, “Variations among and within lead bullet manufacturers make any modeling of the general manufacturing process unreliable and potentially misleading in CABL comparisons” (p. 5). In other words, the technique could not

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<sup>1</sup> Imagine we had precisely 33,000 false felony cases and that forensic science testing errors contributed to 63 percent of them and false or misleading forensic science testimony contributed to 27 percent of them. These assumptions do not let us infer the number of cases in which forensic science testing errors or false or misleading forensic science testimony contributed to the false conviction. If we knew that all cases of poor testimony coincided with a testing error, then our value would be 20,790 (=33,000 X 0.063). If there were no overlap, then our value would be 29,700 (=33,000 X [0.63+0.27]). In any event, the value exceeds 20,000.

sustain the evidentiary weight attributed to it by the FBI. Solomon (2007) suggests that there may be hundreds of American prisoners today who were wrongly convicted on bullet-lead evidence.

The failure of rational scientific control in forensic science has been documented in some detail by the National Academy of Sciences (2009). The NAS report cites a host of problems, including unproven scientific methods (pp. 5-5, 5-12, 5-17, 5-21, 5-26, 5-30, 5-37,), lack of uniform standards (S-4), fraud (1-8), overclaiming (S-3, 1-8), bias (4-9 to 4-11), dependence on law enforcement (6-1), lack of scientific culture (4-11), and poor quality assurance and quality control (7-10). According to the NAS report, the courts have not adequately disciplined bad practice within forensic science. “The bottom line is simple: In a number of forensic science disciplines, forensic science professionals have yet to establish either the validity of their approach or the accuracy of their conclusions, and the courts have been utterly ineffective in addressing this problem” (NAS 2009, p. 1-14). William Thompson and others have shown that even DNA profiling is subject to error because unscientific practices (Thompson 1995 & in press, Thompson & Cole 2007).

The key fact that generated most of the literature on forensic science errors was probably the growing number of DNA exonerations in the U.S., most of them conducted by the Innocence Project. In the wake of these exonerations the epistemic monopoly of forensic science has been challenged. In 2003, for example, then editor of *Science*, Donald Kennedy, wrote a scathing editorial in *Science* entitled “Forensic Science: Oxymoron?” Different elements of the forensic science community have responded in different ways to challenges to the epistemic

privileges of their field. Public records reveal at least some use of several inappropriate practices that support the epistemic monopoly of crime labs.

**Evidence destruction** prevents a second lab, possibly in the employ of a defense attorney, from retesting forensic evidence. By thus blocking redundant testing, evidence destruction supports the forensic science monopoly.

Investigative journalists at *The Denver Post* uncovered “141 prisoners . . . whose bids for freedom have stalled because officials lost or destroyed DNA” (Greene & Moffient 2007). The report says, “the system routinely mishandles biological evidence.” The law often requires that evidence be preserved, but a 1985 Supreme Court ruling that no violation of rights can be claimed without evidence the police acted in bad faith. In the wake of this decision the article claims, “police and prosecutors nationwide are free to trash the evidence prisoners seek to prove their innocence.” The report indicates says, “some district attorneys and police chiefs lobbied Congress to maintain their right to destroy evidence,” which strongly suggests that evidence destruction is a regular practice of at least some police departments.

**Resistance to case review when errors are revealed or suspected** prevents other parties from finding evidence that a give forensic analysis may have been mistaken or that forensic testimony was false or misleading. Thompson & Dioso-Villa (2008) examine in detail the capital case of Robin Lovitt as an example of judicial failure. They find, “The DNA evidence that was a key part of the government’s case was presented in a misleading and unfair manner” (p. 101) and decry the system’s failure to properly review

the scientific evidence for errors or for false or misleading scientific testimony. In this instance resistance to case review came from the lab's oversight body.

Virginia's crime lab is known at the Department of Forensic Science. "In response to the scandal over the mistyping of DNA evidence in the Earl Washington case, the Virginia Assembly passed legislation in 2005 creating a Forensic Science Board and a Scientific Advisory Committee to oversee operations of the state's Department of Forensic Science" (p. 139). In the Lovitt case, however, a defense request that the Scientific Advisory Committee review the DNA evidence in the Lovitt case was refused by the Forensic Science Board. Thompson and Dioso-Villa (p. 140) reproduce a revealing portion of the refusal letter, which was written by the Board's Chair, S. Randolph Sengel.

The manner in which counsel can present and argue from evidence at trial is not within the statutory scope of Committee review, which includes the "analytical work, reports, and conclusions of scientists employed by the Department." Accordingly, I cannot find that the review authority of the Scientific Advisory Committee extends to encompass a review of the manner in which prosecutors and defense attorneys attempt to present evidence at trial, or to [the] assessment of the objectivity or propriety of arguments made from such evidence by trial counsel. For these reasons I find that your request for the review of the case *Commonwealth v. Robin Lovitt* does not fall within the scope of the review authority of the Committee.

As Thompson and Dioso-Villa point out, the DNA issues in this case "went well beyond the manner in which counsel presented and argued from the evidence in court" (p. 140). By construing the issue in narrow legal terms, the Board was able to resist review and acted thereby in defense of its monopoly on the analysis and interpretation of DNA evidence in this case.

The **failure to comply with Coverdell oversight requirements** is yet another way to prevent alien eyes from reviewing and reconsidering evidence once it has been examined and given an interpretation by a crime lab. The Paul Coverdell Forensic Science Improvement Grants Program requires any crime lab receiving federal grant money under the program to certify that there is an outside independent agency who could investigate potential and actual error and malfeasance committed by a forensic lab, that the agency has the technology and resources to investigate the potential error or malfeasance, and that the agency has effective processes to carry out its responsibilities. A 2008 report of the Justice Department's Office of the Inspector General found, however, that the oversight requirements of the Coverdell program are often ignored. The report found that of the 231 programs certified in 2005 and 2006, receiving a total of \$31.5 million in grants, 78 (34 percent), failed to meet the basic intent of improving results from forensic laboratories by identifying, investigating and rectifying errors, i.e., creating an oversight and feedback mechanism (Office of the Inspector General 2008).

**Over-claiming in forensic science testimony** exists when a technique or result is represented as more probative than it really is as when a vague similarity between samples is represented as, say, a "near-perfect match." Over-claiming supports the forensic science monopoly by creating and supporting the view that crime labs have powerful and definitive techniques that can extract precise truths from ambiguous evidence.

Friedman (2003) seems to have coined the phrase "over-claiming." He and Cole (2007) have identified over-claiming as an important problem in forensic science. In a

review 137 exoneration cases for false or misleading forensic science testimony, Garrett & Neufeld recount several cases of over-claiming in microscopic hair analysis. In one Illinois case, the forensic scientist testified, “I couldn’t distinguish if I was looking almost at two hairs. They looked just like one.” He elaborated, “What I saw was a hair like this where I could see all the pieces like a puzzle. Like in the previous hair. A line would just fit in. In other words it wasn’t a type of situation of sitting down and looking for it. Just like if you dropped two dollar bills and you see two dollar bills on the floor. You see two one dollar bills. It’s obvious. And that’s how it looked there” (Garrett & Neufeld, p. 56). Two distinct human hairs from the same head may have similar characteristics such as color and diameter, but they will not have the number of precise and detailed points of strict correspondence that are present by design in two dollar bills.

Over-claiming of the sort Garrett & Neufeld document may remind the reader of a scene from *To Kill a Mockingbird*. Three young children are curious about their secretive neighbor who is rumored to be dangerous and, perhaps, mentally unstable. The children are Scout, her brother Jem, and their neighbor Dill.

**Scout.** I wonder what he looks like?

**Jem.** Well, judging from his tracks, he’s about six and a half feet tall. He eats raw squirrels and all the cats he can catch. There’s a long, jagged scar running all the way across his face. His teeth are yellow and rotten. His eyes are popped. And he drools most of the time.

**Dill.** Aw, I don’t believe you.



Jem seems to have had little prior status in Dill's eyes and Dill easily shows a healthy skepticism regarding Jem's ability to infer a scar on Boo's face from tracks in the ground. Judges and juries find forensic science as it structured today hard to view with a similar skepticism in part because of the high status of the field, which is reinforced by its epistemic monopoly.

The **reliance on subjective techniques** implies that the personal judgment of a forensic scientist is the principle criterion for judging what the evidence means and whether a known and unknown sample have a common origin. The NAS report notes that many forensic disciplines rely on subjective judgment, including "impression evidence" such as shoeprints and tire tracks, toolmarks and firearms identification (the later commonly called "ballistics"), traditional hair microscopy, the handwriting comparisons of questioned document examiners, bloodstain pattern analysis, and fingerprint examinations (NAS 2009).

Nichols (2007) attempts a spirited defense of subjective judgment in firearms and toolmark identification, which is commonly known as ballistics. He insists that standard techniques "rooted in firm scientific foundations" and "critically studied according to the precepts of the scientific method." And yet, Nichols says, "Currently, the interpretation of individualization/identification is subjective in nature, founded on scientific principles and based on the examiner's training and experience" (p. 587). In other words, they match when we say so and that's what we call science! It is understandable that such defenses to a discipline would be accompanied by resistance to redundancy and oversight and strong demarcations between outsiders and insiders (Gestring 2009).

## **Now what?**

In a crucial remark, Berger & Luckmann say, “To understand the state of the socially constructed universe [or subuniverse] at any given time, or its change over time, one must understand the social organization that permits the definers to do their defining” (p. 116). Experts, they note, “claim ultimate jurisdiction of the stock of knowledge” which is their specialty (P. 117). We have seen the some of the consequences of epistemic monopoly in forensic science. As Berger & Luckmann suggest, however, a different social organization is possible.

The organization of forensic science in the U.S. is a case in which the dictum of Berger & Luckmann holds fully. They say, “the social world was made by men—and, therefore, can be remade by them” (p. 89). It is possible and necessary to break the forensic s science monopoly (Koppl 2005, 2010; Koppl, Kurzban, & Kobilinsky 2008).

This is not the place to examine how the job is to be done. We must recognize, however, that the current organization of forensic science gives the forensic scientist a privileged place in defining crucial aspects of social reality. They will not relinquish such privilege without a fight. They will fight in part because monopoly is comfortable. They will fight in part because monopoly gives power. But they will fight mostly because they wish to defend a definition of reality in which they sincerely believe. When that definition is challenged, chaos looms. “The legitimation of the institutional order is also faced with the ongoing necessity of keeping chaos at bay” (p. 103). Forensic scientists will fight for their monopoly and for the old ways because they believe themselves to be men of silver, keeping the

dogs of chaos at bay. Berger & Luckmann's sociology of knowledge may not provide any ready solutions to the problems created by the epistemic monopoly of forensic science, but it has performed a mighty labor in indentifying them.

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