It *Isn't* The Thought That Counts[1]

MIRIAM SOLOMON

Temple University
Department of Philosophy
Philadelphia PA 19122, U.S.A.
E-mail: solomon@vm.temple.edu

Critical thinking instructors advise students that, with effort and training, they can think better, and that thinking better is worthwhile. The courses they teach are designed to foster clarity of thought and critical, selective, autonomous processes of evaluation. The goals are quite similar to those of Bacon in *The New Organon* who wrote (p. 66) that the understanding must be 'thoroughly freed and cleansed' by a 'fixed and solemn determination' to renounce the 'Idols' – the various biases and dogmas that prejudice thinking.

'Clean and free' thought is generally described as possessing some properties and lacking others. On the positive side, 'clean and free' thought is something which *individuals* can aspire to have. Even when 'clean and free thought' is the result of the help, training and criticism of others, it is something that an individual does. Also on the positive side, 'clean and free' thought typically involves the application of effective methods. And, on the negative side, 'clean and free thought' lacks social, political and cognitive biases, is free of ambiguities and unwarranted presuppositions and is not the result of influence, coercion or respect for authority (Bacon classified all these, variously, as Idols of the Tribe, Cave, Market Place and Theatre. These days they are often referred to as 'biasing factors' or 'social factors').

It is, by now, well known that getting thoroughly 'clean and free thought' is a tall order for scientific thinking. The work of Kuhn and others has shown the complexity of scientific decision making, the inadequacy of any proposed method of clear thinking, and the pervasive influence of various so-called 'biasing factors' or 'social factors'. Yet the reaction to this science studies research has been, remarkably, to continue to think that 'clean and free thought' is all that matters, normatively speaking.

Two kinds of conclusions ensue. Some conclude that since 'clean and free' thought is not possible, normative guidelines for scientific thinking are not possible, either. This is the message behind Collins and Pinch's undergraduate text, *The Golem*, and similar work in 'strong program' sociology of science. Others continue the Baconian tradition and advocate thought that is, if not perfectly 'clean and free', then 'clean and free' *enough for doing successful science*. (I think of this as the Ivory Soap model of

Argumentation **15**: 67–75, 2001.

scientific reasoning: 99 44/100% pure.) For example, it is widespread among philosophers of science to think that at crucial stages in scientific disputes, evidence and arguments in favor of one theory become overwhelming, and, in particular, overwhelm any ‘biasing factors’ or ‘social factors’ that might be present. This is Ron Giere’s (1988) story about the geological revolution, and Philip Kitcher’s (1993) story about the Great Devonian Controversy. Or, for example, much work in social epistemology discusses the improvement of individual thinking through the practices of social criticism. Helen Longino’s (1990) account of objectivity insists on the elimination of individual errors and biases through democratic practices of criticism.[2] Philip Kitcher (1993) and Alvin Goldman (1992) talk of the use of rhetoric and persuasion in getting scientists to see arguments and evidence more clearly.

I regard all these reactions as too conservative, because they continue the tradition of regarding ‘clean and free thought’ as the only thing that can matter, normatively speaking, in doing science. My aim in this paper is to put the spotlight on other ways to improve scientific decision making. ‘Clean and free thought’ is certainly nice where you can get it, but other things are no less important for attaining scientific goals. This discussion will yield suggestions for pedagogy in critical thinking courses.

I argue not as a relativist, constructivist, epistemological anarchist, deconstructionist, pluralist or rhetorician, but as a relatively mainstream (although not malestream) philosopher of science. I am even willing to argue from the domain of physics and history of physics exclusively (to address those who imagine physicists to be especially clean and pure thinkers) although for the sake of breadth in this paper, I will use a range of examples.

The examples are chosen in order to show (1) that something other than the attempt to reason without bias or error can be conducive to scientific success (2) that something other than the use of method can be conducive to scientific success and (3) that information processing other than *individual* thought processes can be conducive to scientific success.

There are times when clear thinking and good methods cannot contribute to scientific decision making. These are times when evidence and reasoning are inconclusive. Recent examples of this situation, which I will discuss, are geophysics after the publication of Wegener’s work and genetics at the birth of Mendelism. In such cases, the most that clear thinking and good methods can establish is that more than one theory deserves pursuit. How to distribute research effort over various competing theories is left unaddressed as a practical matter.

There are also times when clear thinking and good methods might be welcome, but are not, realistically speaking, forthcoming. For example, theory change for individual scientists is a major cognitive task whose causes must outweigh forces such as belief perseverence and pride. Examination of consensus on plate tectonics theory during the mid-1960s, on the 'central dogma' of nuclear dominance over the cell in the 1950s, and consensus on Copenhagen interpretations of quantum mechanics around 1930 shows that the causes of theory change need not be clear thinking and impartial methods. Bias and error are not, as some philosophers have argued, too weak to overcome rational thinking. Quite the opposite.

'CLEAN AND FREE THINKING' IS NOT ENOUGH

Wegener's evidence and arguments for continental drift deserve both sympathy and criticism. On the one hand, the geophysical similarities between South American and South Africa, the paleontological findings in the Southern Hemisphere, and the meteorological evidence all supported drift. On the other hand, the absence of an explanatory account of the mechanism for drift – and, indeed, the suggestion that drift of rigid continents through the ocean beds, as Wegener proposed, is impossible – counted against drift. The reaction of the geological community was not, however, general indecision. Instead, geologists tended to take sides. Choice was not a matter of 'clean and free thinking,' on either side: those who chose drift tended to have Southern Hemisphere field experience, or to be interested in hypotheses of lateral continental movements to explain orogeny. Those who opposed drift were continuing a Lyellian gradualist tradition, downplayed any data in favor of drift, and took the geophysical arguments against the possibility of drift seriously, often were impressed by the prestige and mathematical sophistication of the British geologist Jeffreys, who argued against drift. The taking of sides in the debate was a fruitful distribution of research effort that contributed to productive resolution of the debate in the mid-1960s. For example, Holmes, who had Southern Hemisphere field experience and was an appreciative audience for Bull's convection current theory of orogenesis, wrote the geological text (*Principles of Physical Geology*, 1944) that became the standard textbook in English outside the United States. This textbook argued for continental drift driven by convection currents (a different mechanism than that proposed by Wegener, and the ancestor of seafloor spreading). And, as Menard writes (1986, p. 85), 'The young British geologists and geophysicists who would invent so much of paleomagnetism and plate tectonics were brought up on it'. It is reasonable to conclude that Holmes' writing on continental drift was conducive to progress in the eventual plate tectonics revolution. 'Clean and free thinking' did not accomplish this. Holmes (as reported in LeGrand, 1988) was particularly influenced by a lecture on orogeny, by Bull, then President of the Geological Society of London, and by his own work on radioactivity which suggested that drift provided a mechanism for the release of heat from the interior of the earth (scientists tend to weight their own scientific concerns more heavily than other, equally important concerns). Young British geologists were educated by Holmes' textbook, thus beginning with a confirmation bias towards drift.

Personal influence, egocentric biases and confirmation bias are examples of what are typically called 'biasing factors' or 'social factors'. I want to avoid this traditional philosophical language here, and instead, call them *decision vectors*. This is intended to be an epistemically neutral term, signifying that the departure from 'clean and free thought' is not, in itself, epistemically good or bad. Decision vectors are so called because they influence the outcome (direction) of a decision. The terminology is deliberately material (physical) rather than abstract (logical), indicating my commitment to naturalistic epistemology of science.

A second case in which the importance of decision vectors is apparent is the early history of genetics. Mendelism, which began as a movement around 1920, became very successful in the United States and Britain. It was not, however, the only theory with evidence and arguments on its side. In fact, there was a central difficulty with Mendelism (which Dick Burian and Jane Maienschein have called 'Lillie's Paradox; see Burian, 1986): there is no account of embryonic segregation, or more generally, cellular differentiation: if chromosomes rule cellular processes, how can very different cells within the same organism be produced and controlled by the same chromosomes? Mendelian genetics could not – indeed did not attempt to – answer this question: mechanisms of genetic regulation were not conceived of in classical genetics. Mendelians ignored or downplayed the paradox, but others used it as the basis for alternative programs of genetic research featuring cytoplasmic control and environmental regulation.

Embryologists, and also European biologists as a group, were most likely to dissent from classical genetics. Embryologists found Mendelism particularly unhelpful for their research. The European reaction was in part because embryology had a strong tradition there and also because the universities (especially in France and Germany) were tradition bound and thus not open to the creation of genetics as a new discipline. Furthermore, it was a competitive strategy: in France, especially, alternative genetic research (e.g. research on cytoplasmic inheritance and the inheritance of acquired characteristics) was pursued in order to have an audible voice above the dominant American genetics community, and a chance of special national success (see Sapp, 1987).

Alternative genetic research produced results and research traditions that were, in some cases, important for later work in molecular genetics. In particular, Ephrussi's work (which came out of the French school of non-Mendelian genetics) on cytoplasmic processes was vital for understanding genetic regulation (and thus dissolving Lillie's Paradox). (See Burian et al., 1988.)

So, here is another case where decision vectors distributed research effort over competing theories. There were decision vectors on both sides. In the USA, there was institutional and industrial support for Mendelism, as well as support from the eugenics community. Also the ideological appeal of the Mendelian picture of nuclear dominance over the cell played a role, as Evelyn Fox Keller (1985) has documented. On the other side, there were the reservations of embryologists, the conservativeness of European universities and competitive international strategies. This distribution of research effort produced valuable results on both sides.

However, this is also a case where where decision vectors were so skewed towards one side that much valuable research on the other side was not appropriately received, and who knows how much not done at all. The work of Ruth Sager on cytoplasmic inheritance and of Barbara McClintock on gene-gene interactions took many years to be incorporated into the general consensus, and work of Sonneborn on cytoplasmic inheritance that depends on supramolecular structures (rather than cytoplasmic DNA) has been lost to the genetics community. So decision vectors, while essential for distributing research effort, do not automatically distribute research effort well. There is no invisible hand of reason. Thus, there is room for normative evaluation of the distribution of decision vectors, and also for normative suggestions to influence the social, political and institutional context in order to alter the distribution of decision vectors.

'CLEAN AND FREE THINKING' IS AN UNREALISTIC AND UNNECESSARY GOAL

The cases just discussed already go some way towards arguing that 'clean and free thinking' is an unrealistic and unnecessary goal when doing science. In typical cases of complex scientific decision making, decision vectors rather than unbiased thinking or traditional scientific methods settle the issue for individual scientists, and this can lead to a productive distribution of research effort. The role of decision vectors is not, however, limited to times of controversy and disagreement. They can also be instrumental in bringing about consensus. Sometimes such consensus is normatively appropriate, and sometimes it is not. Here, first, is an example of normatively appropriate consensus: consensus on plate tectonics in the 1960s.

As mentioned above, there was worldwide dissent on the question of continental drift before the 1960s. When new evidence in favor of drift became available – first, the magnetic data from volcanic rocks; then the magnetic anomalies over deep sea ridges, then measurements of shifting plates – geologists made different evaluations. They tended to become convinced of drift for different reasons: the data were their own (rather than read in publications) or presented in salient visual fashion at a conference; the results were produced in their own subdiscipline (the order of acceptance tended to be paleomagnetists, then oceanographers, then seismologist, then stratigraphers, then continental geologists); they experienced peer pressure or pressure from people in authority. Geologists tended to resist drift when they had themselves produced experimental results which seemed (at first) to tell against drift (e.g. Heirtzler, Worzel, Ewing, LePichon); and when their areas of research were far from the areas in which data supporting drift initially appeared (see Solomon, 1994a).

Consensus took place when plate tectonics had *universal* (in the scientific community) empirical successes so that even over-valuations (produced by various decision vectors) of some empirical successes led to the same choice of plate tectonics. That consensus was helped along by salient advertising of new results (especially at the so-called 'roadshows': international conferences to which prominent geologists were invited, which showcased visual presentation of evidence in favor of drift) and the bandwagon effect. The empirical successes of plate tectonics were necessary for consensus on the theory, but not sufficient. A model of scientific thinking as 'clean and free' thinking of individual scientists does not do well here. It makes the most sense to say that the scientific community a a whole selected plate tectonics because of its universal successes, while no individual did so. Furthermore, the scientific community did so only via decision vectors: mechanisms such as cognitive salience and availability; salience, pride and peer pressure; the influence of institutional training and institutional structure.

Now some examples of consensus that are less than normatively ideal. Consensus in the genetics community on the 'master molecule' view of DNA in the 1950s took place with the same skewed pattern of decision vectors as was present at the birth of Mendelism. In fact, it looked to most as though classical genetics had triumphed and the final material reduction of the gene – from abstract entity to portion of chromosome to nuclear DNA – had taken place. The 'master molecule' view of the internal organization of the cell was widespread and disseminated in textbooks and popular writing as well as scholarly journals. This almost-consensus failed to acknowledge two related facts: first that molecular genetics is not in fact a reduction of classical genetics (i.e. it is not the case that gene = a portion of chromosome = length of DNA); and second, that many genetic phenomena cannot be understood on the master molecule view. The work of Barbara McClintock, Ruth Sager, Sonneborn and others was either not accepted or accepted late. The largest current genetics project – the human genome project – both suffers from and exploits (for funding purposes) its description as the cracking of the code of the master molecule in humans. So, the almost complete consensus on the 'master molecule' view proved an obstacle to the acceptance of important ideas and results and still does, to a lesser extent, today.

Another example is consensus on Copenhagen interpretations of quantum mechanics in the late 1920s. Cushing (1994) argues that this consensus was a historical accident. There were alternatives to quantum indeterminacy, even at that time. Causal theories were available (albeit causal theories that allowed non-local causation). According to Cushing, the debate was settled in favor of Copenhagen interprestations because of what he calls 'accidental' factors such as the organization of the physics community around the authority of Copenhagen physicists, the unconventional intellectual climate in Germany during the interwar period (the Forman thesis) and the unnecessary weakness of de Broglie's attempt to develop a causal theory. When, later, a satisfactory causal theory was put forward by David Bohm, it was dismissed on religious grounds (e.g. by Pauli) or on metaphysical grounds (e.g. by Einstein who defended locality and by Heisenberg physicists who had a metaphysics of subjectivity), by anti-Communist feeling (communists favored causal theories) and by conservative as well as anti-metaphysical opinions among physicists, who wanted the matter of interpretation to say settled. This is quite a catalog of decision vectors, all in favor of Copenhagen and against causal interpretations.

Was the consensus on Copenhagen interpretations bad for quantum mechanics? The matter is controversial. In my opinion, it depends on whether or not Copenhagen interpretations and causal interpretations are empirically equivalent. This matter is also controversial, although causal interpretations do not, right now, have empirical content that Copenhagen interpretations lack. If the two theories are empirically equivalent (as Cushing argues), or if Copenhagen interpretations have the same empirical successes as causal interpretations and more (the only other possibility), then the consensus on Copenhagen interpretations was not bad for quantum mechanics and perhaps even good. Even though the consensus was achieved by a skewed distribution of decision vectors, no empirical successes were lost to the scientific community (unlike the case of genetics just described). This is an unusual case: usually competing theories have different empirical successes, and a skewed distribution of decision vectors has less benign results. Quantum mechanics was lucky.[3]

I mention this complex case in order to counter any impression that taking epistemic advice comes with a guarantee of success, and that ignoring epistemic advice comes with a guarantee of failure. Epistemic advice is worthwhile, but it is only advice. Here, ignoring advice (or rather, ignoring retrospective advice!) turned out just fine.

The cases discussed show that scientific success is typically achieved with decision vectors, rather than 'unbiased thought' or explicit methods. Success is also typically achieved socially, especially when decision vectors are equally distributed over competing theories. (Equal distribution of decision vectors is not the same as equal distribution of labor.) I have elaborated this normative position elsewhere (Solomon, 1994a, 1994b, forthcoming) and I call it 'social empiricism'.

These reflections focus epistemic attention at a level that has previously been overlooked. Instead of looking to refine the thinking of individuals, eliminate bias, and follow methods, we look at the distribution of decision vectors in a scientific community, and, where appropriate, suggest altering that distribution. Realistic suggestions for altering the distribution of decision vectors include the creation of additional incentives in the form of specifically targeted grants and prizes, creation of opportunities and techniques for changing ideas such as conferences and visual aids and increasing the ideological appeal of marginalized approaches. Many of these suggestions involve action at an administrative level, rather than the level of the individual research scientist. Again, this is a departure from traditional practical epistemological advice. Traditionally, critical thinking instructors train for autonomy: we have in mind a particular fantasy about the individual research scientist, usually the principal investigator. This image of the lone clear-thinking investigator is a traditional epistemic ideal, and also an American ideal, but it is not adequate as a normative ideal for science.

When I suggest 'It *isn't* the thought that counts,' I am not endorsing sloppy thinking, or saying that critical thinking courses completely waste students' time by trying to improve clarity and consistency. I'm saying that it isn't enough for philosophers, and critical thinking courses, to provide a framework for 'conceptualizing bias,' as Antony (1993) puts it. Identifying bias and addressing bias requires material (rather than simply logical or even 'thought' based) processes that, in the end, challenge the traditional conceptualization of bias. That is why I have invented the terminology of 'decision vectors'.

I'm offering a new heuristic for epistemology of science, and especially for epistemology of science pedagogy, the beginnings of a new *New Organon* for science studies. Heuristics are most useful when they are brief, easily remembered, and shorthand for a collection of rich ideas that are learned together with them.[4] Heuristics don't have to cohere with one another to be useful and meaningful.[5] Some of the best heuristics are even internally inconsistent: 'Good pitching will always beat good hitting and vice versa'. We can retain our preference for clear thinking, while offering 'It *isn't* the thought that counts' as a new chapter heading for critical thinking texts. It is time for eclectic epistemology.

NOTES

[1] About half the material in this paper comes from Solomon, 1994a and Solomon, 1994b.

[2] Granted, Longino does not think that 'clean and pure' thought is attained in this way; in fact she thinks that all thought is imbued with values. It is only individual biases and errors (rather than ideological presuppositions) that she expects to be eliminated.

[3] Those who disagree with me and agree with Cushing that the metaphysics of interpretations of quantum mechanics is also of scientific value will also think that the triumph of Copenhagen over causal theories was bad for quantum mechanics. So, they will conclude that the skewed distribution of decision vectors led (as they usually do) to a poor scientific outcome.

[4] See, for example, Mitchel Resnick's (1994) collection of heuristics for understanding complex aggregate behavior.

[5] There may be an analogy here to Cartwright's ideas on the laws of physics.

BIBLIOGRAPHY

Antony, Louise: 1993, 'Quine as Feminist: The Radical Import of Naturalized Epistemology', in Louise Antony and Charlotte Witt (eds.), *A Mind of One's Own: Feminist Essays on Reason and Objectivity*, Westview Press, Boulder, CO.

Bacon, Francis: 1960 [1620], *The New Organon*, Fulton Anderson (ed.), Bobbs-Merrill Educational Publishing, Indianapolis, IN.

Burian, Richard: 1986, 'Lillie's Paradox – Or, Some Hazards of Cellular Geography', Typescript.

Burian, Richard, Jan Gayon and Doris Zallen: 1988, 'The Singular Fate of Genetics in the History of French Biology, 1900–1940', *Journal of the History of Biology* **21**(3).

Cushing, James: 1994, *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony*, University of Chicago Press, Chicago, IL.

Giere, Ronald: 1988, *Explaining Science: A Cognitive Approach*, University of Chicago Press, Chicago.

Goldman, Alvin: 1992, *Liaisons: Philosophy Meets the Cognitive and Social Sciences*, MIT Press, Cambridge, Mass.

Keller, Evelyn Fox: 1985, *Reflections on Gender and Science*, Yale University Press, New Haven and London.

Kitcher, Philip: 1993, *The Advancement of Science*, Oxford University Press, Oxford and New York.

Le Grand, H. E.: 1988, *Drifting Continents and Shifting Theories*, Cambridge University Press, Cambridge.

Longino, Helen: 1990, *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*, Princeton University Press, Princeton, NJ.

Menard, H. W.: 1986, *The Ocean of Truth: A Personal History of Global Tectonics*, Princeton University Press, Princeton, NJ.

Resnick, Mitchel: 1994, *Turtles, Termites and Traffic Jams*, MIT Press, Cambridge, MA.

Sapp, Jan: 1987, *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics*, Oxford University Press, New York and Oxford.

Solomon, Miriam: 1994a, 'Social Empiricism', *Nous* **XXVIII**(3), 325–343.

Solomon, Miriam: 1994b, 'A More Social Epistemology', in Fred Schmitt (ed.), *Socializing Epistemology*, Roman and Littlefield, pp. 217–233.

Solomon, Miriam: forthcoming, 2001, *Social Empiricism*, MIT Press, Cambridge, MA.