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Alliances between Styles: A New Model for the Interaction Between Science and Technology

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Biotechnology and nanotechnology have acquired, or almost acquired, a paradigm status of what science is today. Science is technoscience now and philosophers of science are catching up with the recent status of technology vis-a-vis science. For better or for worse, the university is no longer the home of pure science. 'If pure science ever existed', many would assert. But as Paul Forman reminded us, before 1980 neither scientists nor engineers, neither philosophers nor historians of science, and politicians the least of all, doubted the cultural primacy of science over technology.¹

If the cultural primacy of science was a myth, it was a myth with real consequences, not just in the 20th Century, but in the early 17th Century as well. At the very least, it contributed to forge a social relationship between science and technology, when Baconian science rallied with technology to improve upon human earthly existence. For this, it was enough that science should be useful, and this idea is older than that science should lead to technological innovation. The very existence of a relationship between science and technology is of more importance than the question to which belongs primacy, every answer to which is always a reflection of the cultural standards of the age. Primacy has been taken to mean priority in discovery but this it is not. The latter is an empirical, historical question to which no generalizable answer can be given. The once popular 'linear model' of pure science leading to applied science leading to technology has justly been exposed not just as a myth but as wrong. It has never been true, but neither is or was its opposite. It is an extraordinary social fact that technology now enjoys cultural primacy over science. While this may have sensitized us to generally accepted examples of

¹ Paul Forman, 'The Primacy of Science in Modernity, of Technology in Postmodernity, and of Ideology in the History of Technology', *History and Technology* 23 (2007),

science developing out of a reflection on technology, and led us to the acknowledgment of the primacy of technology in several cases, this model is not generalizable either.

There is also a ‘model’ of the science-technology relationship which simply denies that there is one. The idea of the autonomy of technology vis-a-vis science, has been defended by a number of historians of technology, most recently by Thomas Misa.² The mere fact that a good history of technology can be written without reference to science is telling. Yet, it could not say more than that the autonomy is relative and that there are analytical distinctions to be made between science and technology. A subtle, yet pervasive mutual influence of science and technology in the 17th and 18th Century has recently been demonstrated in *The Mindful Hand*, a collection of essays.³ The historians in this volume acknowledge a social distinction between natural philosophers and artisans, yet they also identify intermediaries, through whom skills and concepts were exchanged. Rich as their historical treatment is, from a systematic point of view, the authors seem to do little more than vindicating Edgar Zilsel, who argued in 1942 that natural philosophers received their experimental skills from artisans and artists-engineers. What natural philosophers did with their newly acquired skills remains open for investigation.

But we could also read *The Mindful Hand* as saying that in concrete historical cases, alliances are forged between the various forms of thinking and practice of science and technology. The model which I propose in this article is also a model of alliances, in which science and technology remain analytically distinct. But in addition it breaks up science into six forms, six styles of science, in the way they were first proposed by Alistair Crombie in his 1994 grand overview of the history of science, *Styles of Scientific Thinking in the European Tradition*.⁴

In a nutshell, Crombie’s idea is as follows. In classical Greece, an idea of science was developed which inaugurated a search for first principles. Ultimately, known phenomena should be derived with certainty from the first principles, hence deductive science. During the Renaissance, several new styles of science developed: the experimental, the taxonomical, and a new form of theoretical science which deduced from hypotheses rather than from first principles: the analogical-hypothetical style. The deductive style, while remaining in place as an ideal, was restricted in practice to a few areas of mainly mathematics and an steadily decreasing number of topics in physics. During the 19th Century, two further styles came into being: the statistical and the historical-evolutionary style. Crombie obtained the number of six inductively, by historical investigation. There is no a priori reason why there could not be seven or eight, and perhaps there will be in the future. No style has as yet disappeared.

² Thomas Misa, *Leonardo to the Internet*, Johns Hopkins University Press, Baltimore, Md, 2004.

³ Lissa Roberts, Simon Schaffer en Peter Dear (eds.), *The Mindful Hand: Inquiry and invention from the late Renaissance to early industrialisation*, Koninklijke Nederlandse Akademie van Wetenschappen, Amsterdam, 2007.

⁴ A.C. Crombie, *Styles of scientific thinking in the European Tradition*, 3 vols., Duckworth, London, 1994.

Each of the styles has brought together, in Crombie's words, 'conceptions of the rational, the possible, the desirable, and the acceptable'. Ian Hacking observed that each style is a 'rather timeless canon of objectivity'.⁵ Each style determines in its own way what qualifies as truth. Or falsity, as criteria for falsity are established along with criteria for truth, without which a certain kind of statement would not be recognized as a candidate for being 'scientific'.⁶ Without being exhaustive, logical certainty belongs to the criterion of truth of the deductive style, while the idea of reference of scientific concepts originated in the analogical-hypothetical style.

Crombie did not treat technology at all. Of the six styles of science, he gave separate treatments, thereby demonstrating their coming-into-being as in a sense 'pure' styles, or at the very least the possibility to treat them as idealtypes. Nowhere did he put them in conjunction. Although to Crombie's credit it can be argued that the styles remained analytically distinct, later history of science abounds with examples of such conjunctions between styles.⁷ In concrete cases, the truth criteria of two, sometimes three, styles can operate at the same time and interact, but in each historical case the way they do so has to be solved ad hoc by the scientists who are involved. The conjunctions may therefore be seen as alliances, sometimes uneasy, sometimes more easygoing. An example of the latter is the alliance between the analogical-hypothetical style and the experimental style, an example of the former are the various ways in which statistical thinking is integrated into experimental practice. If we transpose this idea of alliances between styles of science to the science-technology relationship, we obtain in fact six theoretically possible varieties, perhaps more if we would allow for more styles of science to enter the equation. At any rate, the way to think of science-technology relationships becomes enormously diversified.

But we have to first answer the question whether technology can be treated as style, on more or less equal footing as the six styles of science. Is there a single technological style, are there perhaps styles, in plural? Distinctions such as science-is-knowing and technology-is-doing do not work, now that we know that much of science is doing, too, and that technology involves a good deal of knowing only part of which is derived from science. The concept of style in fact brings together ways of knowing and ways of doing. In the case of technology, it should be able to identify what its 'knowledge' aspect is.

⁵ Ian Hacking, "'Style' for Historians and Philosophers", *Studies in the History and Philosophy of Science* 23 (1992), 1-20 reprinted in Hacking, *Historical Ontology*, Harvard University Press, 2002.

⁶ Ian Hacking, 'Language, Truth, and Reason', in: *Historical Ontology*, Harvard University Press, 2000, pp. 159-177 (orig. 1982).

⁷ See also Hacking, 'Styles'. Hacking also suggests that styles combine as 'mixtures' rather than as 'compounds', in which case there would be a new style.

Eugene Ferguson, in his *Engineering and the Mind's Eye* (1999) argued persuasively that design is *the* technological style.⁸ More precisely: *disegno*, or the engineer's drawing, by which a spatial arrangement of working components is achieved. Linear perspective, *chiaroscuro* and the cut away view were all Renaissance inventions, greatly enhancing the explanatory power of the drawing. The orthographic projection was developed by Gaspar Monge and James Watt in the late 18th Century. The notebooks of Leonardo da Vinci and, among many other's, Thomas Edison's threehundred years later, show that thinking by drawing has become the engineers' culturally entrenched habit.

Ferguson relates how from the late 1950s onwards, instruction in engineering drawing began to disappear from engineering schools, by a dual effort of replacing drawing by computing and economizing on time spent on drawing. His own book played its part in restoring the balance toward the 'art' aspect of engineering. We may also note that, compared to the 1960s and 1970s, the computer changed roles and now enables visual thinking rather than suppressing it.

High science, low science, and technology

The cultural shift of about 1980 is more complicated than a shift from a primacy for science to a primacy for technology. The picture of science itself changed, too. Prior to 1980, one style of science stood for science as a whole: either deductivist science or the analogical-hypothetical style. Ernest Nagel's structure of science leaned toward the former. Popper's picture of science was the latter with strong deductive overtones, married to the experimental style of science which stood in a completely ancillary relationship to theory. In Kuhn's hands, hypothetical reasoning became more loosely built on analogies, shown to be 'world views', and pragmatically accepted as tools of discovery.

The hegemony of deductivist science contributed in no small part to the development of the linear model: it posited a logical relationship between science and technology. The precedence in time of science over technology would be more or less necessarily true on this account. technology was a logical derivative of science, either on an analogy between experiment and a working technology (in Popper's view, if he would have discussed technology, which he did not), or by subsuming a working technology under a universal theory on the model of the reduction of experimental laws to theory (in the similar counterfactual case for Ernest Nagel). It was Mario Bunge who, leaning mostly on the Popperian approach, devised a philosophy of technology in an article published in 1965.⁹ Applied science, according to Bunge, is specifying the initial conditions in order that a

⁸ Eugene Ferguson, *Engineering and the Mind's Eye*, Cambridge MA: MIT Press, 1992.

⁹ M. Bunge, 'Technology as Applied Science', *Technology and Culture* 7 (1965), 329-347.

universal law becomes applicable to a desired process. Technology is using that process to some specified goal.

The inclusion of styles of science such as the experimental style, but also taxonomy and statistics, into the picture of science contributes to make the Bunge-model implausible. Crombie published his work on styles of science in 1994, but his ideas had been circulating among historians and philosophers of science already for decades. Ian Hacking, in his *Representing and Intervening* (1983), acknowledges Crombie's styles as a source of inspiration for his own successful attempt to liberate the experimental style from its subordinate position to theoretical science. Again, in his 1990 *The Taming of Chance*, which offers a history of statistics, Hacking asserted that statistics 'fixes the sense of what it investigates', a statement which conveys adequately how styles of science set their own criteria of truth.

Hacking's work can be taken as a sign of a new interest in the 'low' styles of science, the sciences which are dominated by data rather than by theory, and in which various forms of inductive reasoning are considered valid. Technology was not alone to profit from the loss of cultural legitimacy of 'high' theoretical science. The taxonomical style of science is another case in point. Recently, taxonomy has received ample attention from cultural analysts such as Geoffrey Bowker and from historians of science.¹⁰ Ursula Klein and Wolfgang Lefèvre highlighted the importance of the taxonomy of chemical substances in mediating between the practice of artisans and speculation by natural philosophers.¹¹ While historians discovered this for the eighteenth century, 21st Century scientists of various denominations are putting taxonomy in the center of their work. The maps produced by the Human Genome Project, and by several other genomic and proteomic projects are ways of organizing data which are preeminently taxonomical. At the same time, the most salient characteristic of the HGP was the enormous effort put in developing gene sequencing machines, producing the flood of data which were organized in maps. The HGP, therefore, may be considered as an example of an alliance between the taxonomical style and technology.

Technology and experimentation

Ian Hacking argued that 'to experiment is to create, produce, refine and stabilize phenomena'. He mentioned the photoelectric effect as an example of a phenomenon not existing anywhere in the universe before it was made in the laboratory. Hacking did not discuss the relationship between experiment and technology. While some of Hacking's examples come close to what is commonly designated as technology (mainly in the form

¹⁰ G. Bowker, *Memory Practices in the Sciences*, MIT Press, Cambridge, Ma., 2005.

¹¹ U. Klein and W. Lefèvre, *Materials in Eighteenth Century Science: A Historical Ontology*, MIT Press, 2007.

of measuring devices), the context in which he discussed the specificity of experimentation was clearly science, not technology.

There is a similarity. Both the experiment and a working technology are interventions in the normal course of nature. The priority sequence between science and technology can therefore seemingly be reversed: experimental science is modelled on technical intervention. Modern science is an ‘uncanny manipulation of reality’ (*unheimlich eingreifende Bearbeitung des Wirklichen*), wrote Heidegger in 1955.¹² And twenty years later, Heidegger asserted that ‘science ... is already the incessant incursion of technological representation into the realized’ (quoted in Forman, op cit).

Seen from the historical perspective of the adoption of the experimental method by science in the late 16th Century, Heidegger was right. But we cannot conclude that therefore, science *is* technology, even though experimentation and technology obviously have several features in common (intervention in nature, synthetic activity of the experimenter/ technologist), as Hans Radder argues. The difference is not social uses, but rather that technology aims at a level of stabilization of phenomena that scientific experimentation does not need in the relatively disturbance-free laboratory. Moreover, scientists usually seek to ‘replicate’ an experiment by different experiments.¹³ The evaluation of whether the experiments deal with the same phenomenon will always refer to at least some theoretical notions.

Another point of communality between experimentation and technology may reside in the shared notion of design. But ‘design’ in the case of experimentation is a way of speaking differently conceived than in technology: it refers to a ‘plan’, like in “it was her design to become acquainted with him”. Experimental design involves the skillful imposing of certain *ceteris paribus* conditions through certain actions, rather than originating from a drawing. Some famous experiments, like the Michelson-Morley experiment, do involve elaborate spatial set-ups, but very many, like in the biomedical sciences, don’t. I believe, therefore, that we should see experimentation as one way of mediation between science and technology, rather than as a basis for equating them.

As I argued above, we need to put the science-technology relationship on a broader footing than between technology and experiment alone, even if it is the most salient bridge between technology and science. We surmise that there are possible relationships between technology and all of the other styles of science as well. The multi-faceted analysis of aeronautical engineering by Walter Vincenti, in his *What Engineers Know and How They Know It*, will serve as an entry.¹⁴

¹² M. Heidegger, ‘Wissenschaft und Besinnung’, in: *Vorträge und Aufsätze*, Neske, Pfullingen, 1985, pp. 41-66, p. 52.

¹³ Hans Radder, ‘Toward a More Developed Philosophy of Scientific Experimentation’, in id. (ed.), *The Philosophy of Scientific Experimentation*, University of Pittsburgh Press, 2003, 1-18; Radder, ‘Technology and Theory in Experimental Science’, in id. (ed.), *The Philosophy of Scientific Experimentation*, Pittsburgh, Pa: University of Pittsburgh Press, 2003, pp. 152-173.

¹⁴ Walter Vincenti, *What Engineers Know and How They Know It: Analytical Studies from Aeronautical History*, Johns Hopkins University Press, 1990.

A second-order analysis of Vincenti's case studies

Vincenti's first case study is about the procedures of finding the best shape for the fore-and-aft section of the wing of an airplane, the shape of the air foil. Until the late 1930s, a catalog of shapes were available for airplane designers. Wind tunnel testing was the only selection procedure available, but usually far more shapes were on offer than could possibly be tested. Even then, to test 2000 shapes for a new airplane was not unusual. By 1945, however, air foil design had become "a logical, essentially rationalized process", according to Vincenti, even when many uncertainties remain to this day (wind tunnel testing has not disappeared). The original idea, leading to the new rational procedures, was to link air foil design to fluid mechanics, the intuition being that it would be advantageous to postpone the moment when the flow of air along the wing changed from laminar flow into turbulent flow. The intuition was successful though theoretically wrong, but the forged link between fluid mechanics and wing design was lasting. Fluid mechanics is now used to calculate air pressure distributions according to specific design requirements. The relationship between fluid mechanics and wing shapes is between a science which functions here according to the model of deductivism, even if incomplete.

Vincenti's second case study involves the establishment of consensus in the 'flying-quality community' of what is a stable but not too stable airplane. Just like a bicycle, an airplane should be unstable because otherwise steering and correcting manoeuvres would be too difficult; it should be stable because otherwise it would demand continuous supervision of the pilot, which is inpracticible for any flight longer than a few hours. Please mark the relationship of analogy between the bicycle and the airplane.

For decades, nobody had the faintest idea to translate this subjective design requirement into a workable rule. During World War II, an engineer (Robert Gilruth) in service of the (US government) National Advisory Committee of Aeronautics, happened on the following "measure": "stick force per g", which makes the force a pilot should exert on the control stick a function of the airplane's acceleration. Stick force per g should be constant for one particular airplane, but it may vary across airplanes of different types, fighters and bombers typically being on extreme ends of the stability scale. Vincenti calls the ratio of stick force to acceleration "a rational criterion for manoeverability". At any rate, once proposed, it was quickly adopted by the flying community.

This simple mathematical expression was not derived from any theory. Formally speaking, we are dealing with induction. It was proposed by someone who was familiar with lots of data on preferences by pilots, but also had a good understanding of how an airplane behaves in flight, in particular when accelerating. Vincenti himself puts most emphasis on the long process of refining the specifications of "what pilots want", and selecting away theoretical considerations that proved of limited importance only. Gilruth's

proposal (I avoid the term ‘discovery’) thus appears to be based on “sophisticated theoretical reasoning”, as Vincenti has it. But apparently, an intuitive selecting of which theoretical considerations were important among a wealth of others was at stake, and I propose therefore a similarity with Kepler’s “discovery” of the laws of the planetary orbits in the wealth of data he had at his disposal. As Hanson made plausible, this was not pure induction. Something Gestalt-like was involved. In principle, then, we are in the realm of the analogical style of reasoning.

Vincenti’s third case study involves the way aeronautical engineers, and other engineers as well, incorporate theoretical considerations from thermodynamics and fluid mechanics into the design process. Vincenti notes that engineers work with the notion of a ‘control volume’, volumes such as cylinders of steam engines through which fluids pass and on the borders of which thermodynamical relationships and processes can be specified. He observes that ‘control volume’ is entirely absent in physics textbooks, in which control mass is used. Yet, translations between the engineer’s approach and the physicist’s approach can be easily made, there is a close proximity between the ‘first principles’ of science as Vincenti calls them and the thinking practice of the engineer.. Therefore, he argues, it would be a gross injustice to treat the control volume approach as a more applied version of the standard treatments of physics. Yet the control volume approach has a feature the control mass approach has not: a consideration of fluid flows. Thus we see thermodynamics put in a spatial mode of reasoning, typical for the design process.

Vincenti’s fourth case, centers on selecting the the most efficient propeller blade for a given aircraft, from 1916 on, when systematic testing began. There were propellers with straight blades and with curved outlines, each in dozens of varieties of shape. No theory was available, hence testing was necessary, but testing was much more rational than simply cut-and-try. According to Vincenti, forms of experimentation were developed that were specific to engineering, not science, yet were not ‘applied science’. The two outstanding features of propeller testing were the use of scale models and parameter variation. For a long time, full scale tests which aimed to identify the best propeller blade could be undertaken in flight only. Experimenting with scale models in wind tunnels was a practical solution, but the use of scale models requires ‘laws of similitude’, to scale up results. The experiments themselves mainly revolved around parameter variation, the parameters being identifiable features of the blade design plus some conditions of blade operation. In a sense, the design features served as the ‘theory’ guiding the experiments.

In the fifth case study , we find technology ‘pure’, without involvement of science except in rather elementary form and for self-evident reasons. It concerns the development of flush riveting. Protruding rivets are detrimental to smooth air flow, and hence build resistance, causing speed reduction. While this was recognized very early, no action was taken for a long time, since many other improvements promised greater gains. When these were achieved, however, by 1938, virtually all the aircraft companies in the US set out to get rid of the protruding rivets at the same time. Several aspects of technological

practice work together in the case. The central design problem was finding the optimum angle of the rivet head. Joining thin sheets of metal apparently required wide angle rivet heads, lest the sheets would crack. The technique of hammering mattered, one blow giving better results than several softer blows. An artisanal experience with materials, therefore, was indispensable for solving the design problem.

Vincenti defines design as the content of a set of plans (usually in the form of drawings) and the process by which those plans are produced. He further analyses the various aspects of the design process as follows: fundamental design concepts, criteria and specifications, theoretical tools, quantitative data, practical considerations and design instrumentalities. On the basis of his own case studies, I have focussed somewhat more narrowly on the spatial thinking embodied in drawings. Yet, this spatial thinking is more integrative than the pure geometrical features of the drawing by itself: I assume for instance that no engineer can contemplate a drawing without thinking of the materials which are involved and hence, the doability of putting them together.

Vincenti also offers a number of categories on how to think the way science and engineering interact mutually: transfer from science, invention, theoretical engineering research, design practice, etc. These I have replaced by the simpler scheme of alliances between design/ technology and various other styles of science, different ones in each case.

Conclusion

While the idea of alliances between technology and a scientific style recognizes the specificity of technology as a style in its own right, it goes against the claim that technology is 'autonomous', at least in very many cases of technological development. The model of alliances between styles implies that neither technology can be subsumed under science, nor vice versa, and this is entirely in keeping with Vincenti's own stance. As we have seen, there is in fact not one alliance between technology and science, but several: six styles of science allowing for the possibility of, in principle, six different alliances and possibly more would more styles of science be involved in one alliance.

The model of alliances also allows us to qualify Paul Forman's thesis of a cultural shift, a reversal of cultural primacy with technology now leading at the expense of science. Even when technology is the 'leading partner' in an alliance with science, we would not expect a total eclipse on the part of science.

But there is another point as well. While the shift from a cultural primacy from science toward technology may be experienced as dramatic, an exclusive focus on this shift alone would blind us to an equally dramatic shift which operated within science itself. This is the shift in dominance from the 'high' styles to the 'low', from a focus on theory to one on data and ways of producing data.

It may be expected that both shifts reinforce one another. The data producing styles in science are dependent on various forms of technology to produce and handle data. Yet, alliances may not be stable over long periods of time. The relationships between science and technology, and within science itself, are too diverse to expect that one single configuration will be dominant for a long time to come.