What Engineers Know

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To say that what engineers know constitutes engineering knowledge, just as what scientists know constitutes scientific knowledge, is a misleading way of expressing what ought to be a truism. For surely what constitutes scientific knowledge exceeds not only what one scientist knows but even the sum total of what all scientists know – since there are scientific truths that no scientists may remember at any given time. Thus, Mendel’s laws were forgotten until they were “rediscovered”. On the other hand, it may be the case that the total of scientific knowledge is less than the sum of what all scientists know since what scientists know is not uniformly consistent. That is, what some scientists know is sometimes at odds with what other scientists know – perhaps even contradictory – hence a reduction in total knowledge.

Interestingly, the sum total of engineering knowledge does not seem to suffer from this problem. Contradictions do not seem to appear within the confines of the epistemology of engineering. There may be disagreements among engineers as to what is the most efficient solution to a problem but – given certain assumptions about the contingencies involved – it is not the case that two engineers similarly educated and experienced could be armed with sufficiently different perspectives that they would flat out contradict each other.

In this paper I examine some aspects of engineering knowledge in order to determine what it is that engineers know. A lot will depend on how we construe “knowledge”. I will argue for a pragmatic account of knowledge, in which based*on the very grounds on which the claim of superiority is made for scientific knowledge*, engineering knowledge is shown to be far more reliable than scientific knowledge – thereby exposing the lie in the traditional view that science is our best and most successful means of producing knowledge. I will begin with a quick sketch of a pragmatic theory of knowledge, followed by a look at scientific knowledge before turning to engineering knowledge. I conclude with a look at the fate of some tradition philosophical problems.

A Pragmatic Theory of Knowledge

Epistemology is an old topic and it remains stuck-in-a-rut. Since at least Plato, theories of knowledge have concentrated on one crucial factor – the inner mental state of a single individual. Prior to the work of David Hume that mental state was *certainty.* After Hume, empiricists abandoned certainty for some modified form of *justified true belief.* Nevertheless, the stress is still on what a single person knows. The view I am urging was first expressed in the work of Charles Saunders Peirce. The tradition Peirce founded extends through William James, John Dewey, C. I. Lewis, Nelson Goodman, W. V. O. Quine, Nicholas Rescher and, of course, Wilfrid Sellars, just to name a few. The simple idea they endorse in one form or another, is that to qualify as knowledge a proposition or set of propositions must be endorsed by an appropriate community. In *Thinking About Technology* (1999), I put it roughly this way: Individuals produce candidate claims for knowledge, and these candidates become knowledge once they are endorsed by the appropriate community using agreed upon standards. This gives nothing to the Strong Programme sociologists, nor to the relativists – after all, Peirce was a realist. But it does relieve us from the fruitless tedium of devising doomed criteria by which we can determine whether an individual uttering a proposition with X, Y, and Z properties can be said to know something. The criteria are doomed because they ignore contingency, historical and otherwise. A pragmatic account, on the other hand, shifts the emphasis to, for example, the criteria that the scientific community has devised. But, even here, the criteria must meet some bottom line condition. For the pragmatist the bottom line is *successful action*. According to C.I. Lewis, “the utility of knowledge lies in the control it gives us, through appropriate action, over the quality of our future experience” (Lewis 1962, p. 4).

Scientific Knowledge

The nature, structure, and justification of scientific knowledge have been topics of central importance for most of the twentieth century. While it is still not clear that there is complete consensus on the criteria for scientific knowledge, nor should there be since science is an evolving activity, several key features have emerged from the discussion. These have grown out of a reassessment of criteria initially proposed for scientific knowledge in the course of the Scientific Revolution, when the kind of knowledge the New Science was proposed to deliver was alleged to differ fundamentally in kind from what had been previously accepted as knowledge - Aristotelian in character, proceeding from esoteric definitions of fundamental concept.

From the New Science tradition, there are several treasured characteristics of scientific knowledge that recent discussions have forced us to abandon or significantly modify. Given the New Science's emphasis on the role of mathematics, scientific knowledge was described as "universal," "true," and "certain." As the special features of the different sciences – most notably the social sciences – became more pronounced, however, the universality claim had to be modified and carefully bracketed. In the social sciences the development of social relativism made this inevitable. Scientific claims to "truth" and "certainity" suffered a similar fate. But in these cases the problems were not due to specific aspects of the individual sciences. Rather, they resulted from the difficulty of demonstrating the truth of scientific claims in a non question-begging manner – on the one hand – and – on the other hand – from the recognition of the fundamentally underdetermined nature of the relation between any scientific claim and its evidence.

Given emendations formulated in light of criticism, which arose in response to these newly reconstituted problems traditional account offers some features that remain viable. For example, it characterizes scientific knowledge as produced by researchers exploring the domain of a *theory* who aim to provide an account of the relations among the objects and processes of that domain, an account which provides the basis for an explanation of phenomena generally observed or detected in another domain. If I were tempted to isolate one crucial characteristic of scientific knowledge, it would be this: Scientific claims derive their meaning from the theories within which they are associated, hence, *scientific knowledge is theory-bound.*[1]

The theory-bound nature of scientific knowledge presents additional problems beyond those noted above for some traditional assumptions about

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[1] This is not the place to explore the intriguing question of the relation between theory and the technological infrastructure of science, but it should be noted that there is a complex interaction between the theories scientists employ and that infrastructure. For example, sometimes the examination of the kinds of objects which populate a given domain may be made possible by new instruments, e.g., Galileo's telescope revealing the existence of the moons of Jupiter. Likewise, certain theories may require increasing sophistication in their supporting instrumentation. Referring again to the history of astronomy, once viewing the heavens through the telescope was possible, questions concerning the size of the universe forced the modification of the telescope by incorporating a micrometer for the purposes of making such measurements, which required the development of a theory of measurement and distance, etc. (See Pitt 1994).

scientific knowledge – in particular the view that scientific knowledge, if true, is true for all time. If scientific knowledge is theory-bound, and if – as we know from the history of science – theories change, then scientific knowledge changes. Hence, what is accepted as scientific knowledge is not true for all time, at least not all of it, not yet.[2] But this should not be a startling claim. The development of human knowledge is a process of continuous exploration in which we re-evaluate what we know in the course of new findings, and we jettison that which no longer remains consistent with the latest body of information.

We should note further that the tentative nature of scientific knowledge does not mean that knowledge is merely relative – especially in any sense that gives comfort to those opposed to the epistemic priority we traditionally give to scientific claims. The dynamic process in which scientists continuously revise what they are willing to endorse – and by which they examine their assumptions and their methods – is at the very heart of the strength of the sciences. Thus, despite the theory-bound nature of scientific knowledge, the self-critical process of scientific inquiry insures that the knowledge it claims is the best available at that time insofar as it is judged "best" according to community standards.

The ultimate aim of scientific inquiry is explanation. Thus, in the context of a pragmatic account, the ultimate success of the use of scientific knowledge is explanation. We use a theory to explore a domain of objects, sorting out their various relations for the purpose of explaining what can't be explained otherwise by appeal to the activities of the objects in that domain. Why is a tabletop hard? To answer that question we have found that we need to appeal to a scientific theory which proposes that there is a domain of smaller objects which are held together by a series of forces and that it is because of the forces and objects in that micro-domain that our phenomenological report of a hard table is possible. The aim of science is to help us understand the way the world appears to us, and it accomplishes this aim by constructing and testing theories which appeal to features of the world which are not immediately obvious.[3]

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[2] I am referring specifically here to the history of the acceptance of theories and not to the question of their truth.

[3] This account of scientific explanation appears to endorse a form of scientific realism. The theory of explanation on which this view rests was developed by Wilfrid Sellars, and he was a scientific realist – he accepted the view that the ultimately real constituents of the world are the theoretical

There are other aspects of scientific knowledge which are essential to its vitality, but they need not be of concern here. In order to have a fruitful starting point to investigate the nature of engineering knowledge we need only concentrate on these two features; (1) Scientific knowledge is theory bound, and (2) scientific knowledge is developed to explain the way the world works. Unfortunately, while the process of trial and error and reappraisal characteristic of scientific activity seems to reveal its strength, this process also serves to undermine its claim of epistemic superiority over engineering knowledge. Likewise – as we shall see – the theory-bound nature of scientific knowledge creates a number of problems that do not plague engineering knowledge.

Engineering Knowledge

In *What Engineers Know and How they Know it* **(**1988), Walter Vincenti identifies and develops a theme first introduced by Edwin Layton in his landmark paper "Technology as Knowledge." Vincenti provides an account of engineering knowledge from the point of view of a practicing and deeply reflective engineer. Both Layton and Vincenti endorse the view that engineering knowledge – and technological knowledge in general – constitutes a discrete form of knowledge that is different from scientific knowledge. In a later piece, his classic 1987 Society for the History of Technology Presidential Address, "Through the Looking Glass or News from Lake Mirror Image," Layton endorses the findings of A.R. Hall, and claims that "technological knowledge is knowledge of how to do or make things, whereas the basic sciences have a more general form of knowing." (Layton 1987, p. 603) Vincenti echoes this, invoking Gilbert Ryle's famous distinction between knowing how (technology) and knowing that (science).

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entities posited by our best confirmed theories. I accept the structure of Sellars' theory of explanation and replace Sellarsian scientific realism with Sicilian Realism. Sicilian Realism rests on two points: (1) accepting the position that the entities postulated by the current set of accepted scientific theories are *all* real, the world being a very complicated place, and (2) rejecting the principle of reduction, by which the entities of one domain are said to be nothing other than compositions of the entities of the domain of this or that scientific theory, e.g., tables are nothing more than collections of molecules. Sicilian Realism is realism with a vengeance.

Both Layton and Vincenti are concerned to defend the view that – while both science and technology may borrow from or rely on each other in various ways – they constitute two distinct forms of knowledge since they aim at different ends. Science aims to explain and technology/engineering aims to create artifices. Vincenti puts it this way, "technology, though it may *apply* science, is not the same as or entirely *applied* science" (Vincenti 1990, p. 4). He defends this claim in part with an intriguing and highly suggestive proposal. As he sees it, if we start with the proposition that technology is applied science, then there is no possibility of considering the view that technology could involve an autonomous form of knowledge that could account for those technological achievements which are science independent – such as the pyramids of Egypt and the roads of ancient Rome. Given the existence of highly visible science-independent technologies, we have good reasons to believe that we should not characterize technology as merely applied science. It is does not follow from the fact that science and technology each has occasion to rely on the other, nor that one is a subset of the other. Assuming is quasi-autonomous from, what can we say about the distinctive nature of engineering knowledge as a specific form of technological knowledge?

Starting from a wonderfully succinct definition of "engineering" by G.F.C. Rogers – which is highly reminiscent of Emmanuel Mesthene's definition of "technology" (Mesthene 1970, pg. 25). – Vincenti identifies three main components of engineering and then concentrates on the notion of design. According to Rogers (as quoted by Vincenti and augmented somewhat by me),

> Engineering refers to the practice of organizing the deign and construction (and I (Vincenti) would add operation) of any artifice which transforms the physical (and, I (Pitt) would add, social) world around us to meet some recognized need (Vincenti 1990, p.6).

One of the commendable aspects of Rogers' definition is his characterization of engineering as a practice. That is, engineering – like science – is an *activity* with specific objectives. Given Rogers' insight and Mesthene's definition of "technology" as "the organization of knowledge for the achieving of practical purposes" – by a series of substitutions we see that, appropriately enough, *engineering knowledge concerns the design, construction, and operation of artifices for the purpose of manipulating the human environment.* Vincenti proceeds to further narrow the focus of engineering knowledge to the topic of "design knowledge," by concentrating on design. It is worth quoting Vincenti's description of the design process at length because it immediately introduces an important distinction between the design as a set of plans and the design process.

> "Design", of course, denotes both the content of a set of plans (as in "the design for a new airplane") and the process by which those plans are produced. In the latter meaning, it typically involves tentative layout (or layouts) of the arrangement and dimensions of the artifice, checking of the candidate device by mathematical analysis or experimental test to see if it does the required job, *and modification when (as commonly happens at first) it does not. Such procedure usually requires several iterations before finally dimensioned plans can be released for production. Events in the doing are also more complicated than such a brief outline suggests. Numerous difficult trade-offs may be required, calling for decisions on the basis of incomplete or uncertain knowledge. If available knowledge is inadequate, special research may have to be undertaken* (Vincenti 1990, p. 7 - emphasis added).

The process Vincenti describes is "task specific" and essentially characterized by trial and error, but that still doesn't reveal the general nature of the contents of design knowledge. This is case because to capture the nature of the knowledge required for any kind of task, Vincenti must invoke a detailed model which breaks that process up into both vertical and horizontal components, thereby allowing for a precise identification of what is needed when and where in the total design process. This schema is proposed for what Vincenti, calls normal design, as opposed to radical design.[4] Normal design has five divisions beginning with the crucial aspect of any problem-solving process, the identification of the problem. Vincenti, an aeronautical engineer, draws from his own discipline for appropriate examples, but the schema is general enough to encompass a large number of design processes. For example, the design of an architectural project including sighting of the building, electrical systems, plumbing, etc., or the design of a space-based, orbiting telescope.

1. Project definition - translation of some usually ill-defined military or commercial requirement into a concrete technical problem for level 2.

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[4] Following E. Constant in his **The Origins of the Turbojet Revolution**, Johns Hopkins Press 1980)

2. Overall design - layout of arrangement and proportions of the airplane to meet project definition.
3. Major-component design - division of project into wing design, fuselage design, landing-gear design, electrical-system design, etc.
4. Subdivision of areas of component design from level 3 according to engineering discipline required (e.g., aerodynamic wing design, structural wing design, mechanical wing design).
5. Further division of categories in level 4 into highly specific problems (e.g., aerodynamic wing design into problems of platform, airfoil section, and high-life devices. (Vincenti 1990, p. 9)

The process Vincenti outlines appears simple enough. One defines the problem, breaks it into components, and subdivides the areas by problem and specialty required, as needed. What is not obvious at first glance is the way in which the levels interact. Upon further reflection, one can see that what happens at level three will have ramifications for the overall design and visa versa, but recognizing this requires some work. In short, any design project must allow for a good deal of give and take throughout the process. In this respect, if one focuses only on the give and take, the design process sounds reminiscent of the scientific process. But there is a more and it clearly marks out a crucial difference between the process of scientific inquiry and engineering design. As Vincenti says it, "Such successive division resolves the airplane problem into smaller manageable subproblems, each of which can be attacked in semi-isolation. *The complete design process then goes on iteratively, up and down and horizontally through the hierarchy.*" (Vincenti 1990, p.9, emphasis added) If – by way of example – we apply this way of thinking to an architectural problem, we can easily determine what kind of a building to design (level 1), e.g., specific or multi-purpose, as opposed to the kinds of bathroom fixtures to have (level 4), although the one will ultimately bear on the other.

At this point we can pause and take stock of this comparison of scientific and engineering knowledge. First, the characterization of scientific knowledge as theory-bound and aiming at explanation appears to be in sharp contrast to the kind of knowledge Vincenti seeks. Engineering knowledge is task-specific and aims at the production of an artifact to serve a predetermined purpose.

There is a second important difference between the two forms of knowledge thai is revealed by Vincenti's account of engineering knowledge. With engineering cast as a problem-solving activity (not in itself a characteristic which distinguishes it from other activities such as biology or even philosophy), the manner in which engineers solve their problems does have a distinctive aspect. The solution to specific *kinds* of problems ends up catalogued and recorded in the form of reference works which can be employed across engineering areas. For example, measuring material stress has been systematized to a great extent. Depending on the material, how to do it can be found in an appropriate book. This gives rise to the idea that much engineering is "cookbook engineering," but what is forgotten in this caricature is that another part of the necessary knowledge is knowing what book to look for. This a unique form of knowledge that engineers bring to problem solving. But there is more: We read the phrase cookbook engineering usually in a derogatory way. But what is wrong with it? If the knowledge in the book represents information we can use in a variety of circumstances, nay, in circumstances wherever certain contingencies hold – then isn’t this knowledge that comes close to being universal, certain and, must we say it? – true? Could it be said that those who refer to engineering knowledge as stored in books as cookbook knowledge are employing a bit of rhetoric, in order to hide the inadequacies of scientific knowledge?

Contrast this cookbook knowledge with theory bound knowledge. When the theory is shown in some way or other to be flawed fundamentally, it is replaced. That means that what we thought we knew to be the case, isn’t – which hardly sounds like knowledge to me. However, a good cookbook providing stress calculations can be used anywhere, anytime, as long as you factor in the appropriate contingencies. Just reflect on the basis of the metaphor – a good cookbook makes it possible for anyone to prepare a good meal.

Let’s go one step further and contrast Vincenti’s account of the engineering design process with the activity of science. I think it has been shown in sufficient detail in a number of places, by a number of people, that there is no such thing as *the* scientific method, i.e., that there exists one method which insures objectivity and guarantees the production of universal, certain and true knowledge. One appeal to the theory-based nature of scientific work should dispel any lingering illusions. In light of the fact that a scientist working within a theory is exploring the domain circumscribed by that theory, the direction of his or her research, i.e., the kind of research he or she will undertake, will be theory–determined. On the other hand, while the domain of the theory is necessarily where the research will be directed, there is no guide supplied by the theory as to what should be investigated and how. Further, there is no one method that works for all sciences. Consider Astronomy. Given the kind of one time only observations that we find in astronomy – replication, traditionally a cornerstone of scientific method, at least in principle, is impossible. Does this make astronomy not a science, hardly. On the other hand, Vincenti's account of the engineering design process provides specific and definite structure to the process of proceeding through the design process.

We can also go beyond Vincenti and look at the work of Larry Bucciarelli (Designing Engineers, Cambridge: MIT Press), who denies that there is one single design process in engineering. Bucciarelli observes that no single unique design is dictated by the nature of the object being designed or the problem to be solved. But his objection stems not from the denial of design in engineering, but rather from a fine-grained understanding of the nature of the contingencies associated. That is, with Bucciarelli, we can find processes whereby the give and flow of ideas and the importation of the relevant contingencies follow the kind of pattern that Vincenti suggests, only in a more complicated way, when you consider the different types of communities interacting. The important point here is that in engineering design, there is at least a beginning point, for Vincenti, it is the problem, for Bucciarelli it is the object. Both see that whatever processes are at work are dynamic and interactive, but they have a task-oriented beginning point, but no such beginning point is given for scientific research.

Philosophical Problems

Two possible consequences of the cookbook nature of engineering knowledge are: (1) That such knowledge can be transported across fields and (2) it can be used anywhere – the fundamentals of dam building do not change – the contingencies of the particular circumstances may dictate one approach over another, but the basics will remain solid. In contrast, scientific knowledge is not clearly "transportable" across fields in the same way as engineering knowledge. One crucial obstacle presents itself: The problem of incommensurability.

The problem of incommensurability is a philosophical problem that came to the forefront in large part with Kuhn's characterization of the nature of scientific change. For Kuhn, fundamental change in science occurs through paradigm replacement, with his view of incommensurability applying, primarily, across paradigms. A paradigm for Kuhn is many things. However, for the process of this discussion let us consider it as a complete system of thought, including methodological rules, metaphysical assumptions, practices, and linguistic conventions. Two paradigms are incommensurable, it is alleged, because claims in different paradigms cannot be compared so as to determine which claim from which paradigm is true.

For this view to be plausible, a particular theory of meaning must be assumed and a very dubious meta-linguistic assumption must be activated. First, let us look at the theory of meaning. Basically, the theory of meaning, behind the assumption of incommensurability, presumes that expressions receive their meaning contextually, within systems, i.e., paradigms, governed by unique sets of rules. This by itself is not so troublesome. The difficult part comes through the meta-linguistic assumption that there is no point of view common to both paradigms from which it would then be possible to compare claims from different paradigms. Such a common neutral point of view is necessary, it is argued, since the meanings of expressions are governed by the rules of the paradigm. If we shift an expression from one paradigm to another, its meaning will change since it will be determined according to different rules.

Among other difficult problems to sort through here is the apparently unjustified two-fold assumption that there is *one* fundamental theory of meaning which applies to all paradigms, i.e., the meanings of expressions within any particular paradigm are determined by the ***rules*** of the paradigm, but, by contrast there is no single theory of meaning that allows for comparison of expressions across paradigms. However, if we can assert that all paradigms provide meanings for the terms which occur in that paradigm through the specification of rules, then why can we not, in the same meta-language in which we pronounce this dictum, then create another paradigm with the express purpose of allowing for the comparison of expressions? It is, for example, not at all obvious that the ways by which terms are made meaningful is through the specification of rules. That is, however, the account we are considering, and it is the source of Kuhn’s problem of incommensurability. That much has been stipulated through Kuhn's account of a paradigm. But, unless something further prohibits us from doing so, surely we can say something like this: for the purpose of comparing two expressions, each drawn from a different paradigm: If the results of applying those expressions in the *meta-language,* according to the rules of the *meta-language*, is the same, in the meta-language**,** *then for all accounts and purposes those two expressions mean the same thing*. In short, if two expressions drawn from two different scientific theories yield the same result when transported into a third theory, then they can be said to make the same claim.

The solution is based on our account of engineering knowledge. If something formulated in the context of one paradigm can be used successfully in another, then deep philosophical problems about obscure theories of meaning recede. To treat the problem of incommensurability this way is not to solve it as much as to ignore it. This too may not be a bad thing. There are many philosophical problems still around to which we no longer pay attention since they seem beside the point, for example consider the pseudo problem of how many angels can dance on the head of a pin? It is not clear that this problem was ever solved, but who cares? And so to the problem of incommensurability. If the problem as stated was never solved it appears not to matter. This lack of concern is a function of having *shifted our ground* from worrying about providing an abstract philosophical justification for something that only philosophers worry about to a pragmatic condition of success: Consider the consequences of using this claim from this theory in this context.[5] If it solves our problem, then does it matter if we fail to have a philosophical justification for using it? To adopt this attitude is to reject the primary approach to philosophical analysis of science of the major part of the twentieth century, logical positivism, and to embrace pragmatism. This is a good thing to do, especially when we are concerned with technologies that have real world effects.

Finally, I noted that engineering knowledge was transportable, not just across fields but throughout the world (and perhaps beyond). Anticipating an objection from my colleagues concerned with various manifestations of cultural imperialism – let me attempt to forestall such issues. I am not saying that we *should* transport such knowledge. The appropriateness of such activities is a matter for policy considerations. That is not what I am talking about here.

Returning now to the issue I proposed at the beginning – that engineering knowledge is a more secure form of knowledge than scientific knowledge is, on the very grounds by which it is alleged that scientific knowledge is our best form

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[5] (c.f. Richard Rorty. The Linquistic Turn, Chicago, U. Of Chicago Press, 1967, p. 39.)

of knowledge. However briefly, we have noted that scientific knowledge is transitory – that it changes as theories change. We have also noted that scientific method is likewise not only transitory, but unstable, depending on the area of science being discussed, not only is there no method that will work across the sciences, within a science, the nature of the domain of objects being investigated may suggest different methods; compare biochemistry with botany. Finally if scientific knowledge is to be appraised through a pragmatic theory of knowledge, and given that the objective is explanation, then as theories change, explanations fail. The history of science then becomes the history of failed theories and unsuccessful explanations.

In contrast we have engineering knowledge, which is task oriented. If the application of engineering knowledge, consisting of information in books and task specific methods and techniques results in the production of objects and the solutions of problems which meet the criteria of those for who the jobs are done, then it is successful. Because it is task oriented, and because real world tasks have a variety of contingencies to meet – e.g., materials, time frame, budget, etc., we know when an engineering project is successful or not. Further, those cookbooks represent the accumulated knowledge of what works. It is universal, certain and, if it works, must be true in some sense of “true”. So, on the criteria we advocate for science, engineering knowledge seems more secure, more trustworthy, with longevity. What engineers know, therefore, is how to get the job done – primarily because they know what the job is.

References

Bucciarelli, Larry. *Designing Engineers.* Cambrdge, MIT Press, 1996.

Layton, Edward. *Technology as Knowledge.* Technology and Culture Vol. 15 1974.

_____. "Through the Looking Glass or News from Lake Mirror Image," Society for the History of Technology, Presidential Address, 1987.

Lewis, C.I. *An Analysis of Knowledge and Valuation.* La Salle: Open Court Press, 1962.

Mesthene, Emmanuel. *Technological Change: It's Impact on Man and Society*. Cambridge: Harvard University Press, 1970.

Vincenti, Walter. *What Engineers Know and How They Know It.* Baltimore: Johns Hopkins Press, 1990.