To Engineer Is to Risk and to Err. Aspects of Henry Petroski`s Philosophy of Engineering

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Abstract

This paper has two purposes: (1) to show that American civil engineer Henry Petroski, without blatantly recognizing it, is undertaking since mid-1980s a reflection upon engineering activity that seems in many aspects inspired in the epistemology of science developed about half-century before by renowned Austro-English philosopher Karl Popper, focused in the claiming of two chief theses: the scientific method and engineering method share the same purpose and structure, namely they try to solve noteworthy problems through a rational process of empirically falsifiable conjectures; and the engineering knowledge evolves in accord with a success-failure dialectics; (2) to briefly reconstruct and critically evaluate two other theses implied in the former epistemological ones and working in tandem: (a) professional engineers have the duty to learn from past errors (a moral thesis); (b) a reform of the engineering curriculum is needed as condition for the fulfillment of that duty (a pedagogical thesis).

Keywords: Henry Petroski; Engineering method; Design process; Error.

1 The engineering method: conjectures and refutations.

According to the American civil engineer Henry Petroski, if we want to understand what engineering is we have to get ourselves acquaint with its method. However, since the method of engineering, in his view, is in many aspects identical to the scientific method, it is the later that has to be known by us.

Although Petroski makes no reference to Karl Popper in his *To Engineer is Human*, where he first presented that view in 1985 – actually, in all the seventeen books he has already published, only one contains a single reference to that author (Petroski, 2006, p. 112) – the description he makes in there of what he believes to be the true method of science closely resembles the so-called method of "conjectures and refutations" depicted by that Austrian-English philosopher in his classical essay *Logic of Scientific Discovery* published more than a quarter of century before, in 1959 (as a revised translation of the original German edition entitled *Logik der Forshung: Zur Erkenntnistheorie der modernen Naturwissenschaft*, 1935).

Key passages drawn from both such texts will allows us to realize how close from Popper`s conception of the scientific method the one embraced by Petroski is.

In several places of their respective works Popper and Petroski let us know that they regard the scientific method as a time-honored way of solving challenging problems. This, of course, implies assuming, as they do, that problems of that worthy kind have first to be found, something which depends on a process not entirely rational or sometimes largely irrational. For them, then, problems are what drive scientific research and prompts the need for use of the scientific method.

In a strict sense, therefore, the scientific method can only be applied after some thought-provoking problem have been discovered and there is a wide consensus that it represents a standardized way to help solving problems of such kind in a successful way through a set of rational steps.

To Popper the first methodological step to follow consists in imagining non-trivial, bold, tentative theories or hypotheses – "conjectures" in author´s jargon – as solutions to recognizably relevant problems. He says that "the work of the scientist consists in putting forward and testing theories." ([1959] 1992, p. 7) and that theories "are nets cast to catch what we call "the world": to rationalize, to explain, and to master it." (id., p. 38). Similarly Petroski utters that "[t]he object of science may be said to be to construct theories about the behavior of whatever it is that the sciences studies." (1985, p. 42).

Thus, they both conceive that imagination is at the root of scientific activity. However they also agree that, as happens with the discovery of problems, so too there are multiple and diverse ways for hypothesis generation. That means

that, in their view, the first methodological step cannot be fully described in logical terms and consequently cannot be formally reproduced and teach, since irrational factors decisively intervene in it. As Popper puts it, "[t]he initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it." ([1959] 1992, p. 7); and Petroski concurs affirming that "[t]his aspect of science, the concoction of theories, has no universal method." (1985, p. 42).

Moreover, they are equally persuaded that after a hypothesis has been devised the rest of the process is to be entirely guided by logic. That means that since the moment we have in our mind a promising solution to a problem under research, we can logically derive from it empirically testable consequences and evaluate the results thereafter. In Popper`s words

> "[f]rom a new idea, put up tentatively, and not yet justified in any way — an anticipation, a hypothesis, a theoretical system, or what you will — conclusions are drawn by means of logical deduction. These conclusions are then compared with one another and with other relevant statements, so as to find what logical relations (such as equivalence, derivability, compatibility, or incompatibility) exist between them. ([1959] 1992, p. 9);

and in the same fashion says Petroski

> once a theory has evolved, perhaps from a half-backed idea to a precise and unambiguous statement of the scientist`s entry in the great universal cook-off, the scientific method may be used to judge the success or failure of a given theory or the relative merits of competing theories. (1985, p. 42).

The two authors are also in accord that after experiments have been made to test a hypothesis, two possible outcomes may ensue: either the results constitute positive evidence that favor the hypothesis veracity or negative evidence discrediting it. And they both claim that, if the first outcome is the attained one we cannot interpret that result as confirmation of the hypothesis, since the *modus ponens* rule of reasoning states that positive facts in support of a hypothesis, be their number small or large, will always fall short in the establishment of its confirmation, and thus that at most we have to take the hypothesis as corroborated, i.e., still awaiting to meet any contradicting facts. If, in turn, is the second outcome the resulting one, Popper and Petroski together claim that the hypothesis should be abandoned, revised or substituted for a better one, because, as *modus tollens* rule of reasoning stipulates, a single negative fact is enough to refute it. The first says

> "[m]y proposal is based upon an *asymmetry* between verifiability and falsifiability; an asymmetry which results from the logical form of universal statements. For these are never derivable from singular statements, but can be contradicted by singular statements. Consequently it is possible by means of purely deductive inferences (with the help of the *modus tollens* of classical logic) to argue from the truth of singular statements to the falsity of universal statements. Such an argument to the falsity of universal statements is the only strictly deductive kind of inference that proceeds, as it were, in the "inductive direction"; that is, from singular to universal statements. ([1959] 1992, p. 19);

and more concisely Petroski asserts:

"[y]et no matter how many examples of agreement one may collect, they do not prove the truth of a hypothesis, for it may be argued that one has not tested it in the single case where the theory may fail to agree with reality. On the

other hand, just one instance of disagreement between the hypothesis and reality is sufficient to make the hypothesis incontrovertibly false. (1985, p. 42).

The finding of evidence contrary to a hypothesis means that it probably contains errors that have to be eliminated, which, in turn, requires the invention of better solutions where those errors are absent.

Popper ([1972] 1979) represents the main elements of the scientific method through a very simple schema (using "P" for problem, "TS" for tentative solutions, "EE" for error-elimination): $P \rightarrow TS \rightarrow EE \rightarrow P$ (this schema was later changed to $P \rightarrow TT \rightarrow EE \rightarrow P$, where "TT" stands for tentative theory; cf., e.g., p. 119). The method, then, seems to work in reiteration, going back to the problem again and again as errors are detected in the tried answers.

Petroski believes, as above said, that this method can be extended to engineering; but it does not mean he thinks that engineering is mere applied science, only that both epistemic domains share a similar method. To him, engineers differently from scientists make conjectures not about natural phenomena, but about artifacts, particularly their behavior. (The word "artifact" is used here as shorthand for structure, machines, device, product, and the like.) Therefore, when engineers have a problem in hand and try to devise a hypothesis to solve it, what that really means, in author`s view, is that they make educated guesses about the behavior of assemblages of materials "that they arrange into a world of their own making." (1985, p. 43), analogous to the positing of scientific theories. If what they face, for instance, is the problem of bridging two pieces of land separated by a river, they will almost certainly opine the construction of a bridge, and that, the building of a new bridge, claims Petroski, "may be considered to be a hypothesis in its own right." (ibid.).

Subsequently, the created artifact will be put to trial, or to the test of time, we should perhaps say. Being true that no natural or artificial thing is made to last forever, on the other hand they have to endure for a reasonable while, or, in the case of the bridge example, to "stand for so many years without collapsing." (ibid.). That means that as engineered artifacts withstand for an intended suitable time they acquire a status similar to a confirmed hypothesis, though "never [to] be *proven* until the so many years under the original plan had elapsed." (id., p. 44), but at some moment in time, preferably after than before their envisioned life end, they all have to break up to definitely attest the falsifiable nature or epistemological authenticity of the hypothesis.

Prima facie the scientific method, then, looks paradoxical. In fact, seems rather odd to put effort in imagining solutions to specific problems required in advance to be only provisory or to take its epistemological value from being falsifiable. Nevertheless, each time a conjecture is refuted and revised the cognitively relevant outcome produced is that its content becomes more and more rich and detailed. Bryan Magee, a former assistant of Popper and authorized commentator of his work, gives the following pungent example: when we succeed to falsify the hypothetical statement "water boils at 100° Celsius" testing that supposed fact in a closed container – inside which the liquid reaches its boiling point 1 or 2 degrees before that temperature – we have to rewrite it saying that "water boils at 100° Celsius in open containers"; and when we once more succeed to falsify this last putative statement testing its truth above sea level – where the boiling point temperature decreases as altitude raises – we have again to rewrite it affirming that "water boils at 100° Celsius in open containers at sea level" (1973, p. 24). And similar examples can be easily given in the field of engineering as, for instance, the one that tells us that the conjecture "the external load a beam can carry varies in direct proportion to its length" is falsified by the fact that if we double the length of some beam it will break if we put over it the double of external load he could carry before, since its own weight has also to be taken in account, forcing the modification of the hypothesis to "the external load a beam can carry varies in direct proportion to its length, thickness and breadth".

A critique addressed to Popper that Petroski seems to have in part prevented is the one that accuses him of being too simplistic in considering how scientific hypotheses are actually tested. French physicist and historian-philosopher of science Pierre Duhem and United States philosopher Willard van Orman Quine, independently and separate by more than half century, argue that scientific hypotheses can only be tested in a holistic manner, a thesis that, being true, has the nefarious consequence of impeding us knowing if a conjecture was in fact falsified. Indeed, according to the now called Duhem-Quine thesis a hypothesis can never be tested in isolation, meaning that no empirical predictions can be deduced from it unless in conjunction with some information about the initial conditions of what is conjectured and some auxiliary hypotheses relative to background theoretical, practical and instrumental knowledge. In fist-order logical language, letting "H" for hypothesis, "IC" for initial conditions, "AH" for auxiliary hypotheses and *p* for prediction, we have, then, $[(H \& IC) \& AH] \rightarrow p$. Now, if we succeed in falsifying *p*, we have to infer, *modus tollens oblige*, the negation of [(H & IC) & AH]. However, by the second de Morgan logical rule of distribution, the negation of a conjunction is equivalent to the negation of each one of its parts put in disjunction or $\neg[(H \& IC) \& AH] \equiv [\neg H \vee (\neg IC$

v ¬ AH)]. And, of course, the problem is that we are not able to know exactly what *p* refutes; it could be H, some IC or perhaps some AH (cf., e.g., Ladyman, 2002, pp. 77-81).

More aware of the holistic nature of hypothesis testing says Petroski "one hypothesis of a structural engineer might be that so and so bridge across such and such river under these and those conditions of traffic and maintenance will stand for so many years without collapsing." (1985, p. 43). We have, in this example, a hypothesis H (a bridge will stand for so many years without collapsing), some information about the initial conditions IC of what is conjectured (it will be located across a river and has to stand a certain volume of traffic and maintenance requirements) and implicit auxiliary hypotheses AH (the laws of mechanics, knowledge of the behavior of the materials to be used, etc.). From all that put together to work as premises, we can deduce the prediction *p* that cars and/or trains will pass safely over the planned bridge. And "if such bridge were built and were to carry traffic year after year without trouble", remarks the author, "the hypothesis would be confirmed time and time again"; "[b]ut should the bridge collapse suddenly under no extraordinary conditions before those so many years were up", he adds, "there would be no doubt in anyone`s mind that the original hypothesis was incontrovertibly wrong.» (id., pp. 43-4). Unfortunately, however, like Popper Petroski fails to see that negative evidence cannot by itself alone falsify a hypothesis.

2 The evolution of engineering knowledge: success-failure dialectics.

The question about how scientific knowledge evolves has always been at the centre of epistemologists` attention. Oddly enough Petroski, once more without making any reference to Popper, characterizes it in a way akin to the model the last think of to describe it.

Popper begins to work out his model for the growth of scientific knowledge in the 1930s, but only almost four decades later seems to have found its more or less definitive structure. In *Objective Knowledge* – a compilation of essays from the late 1950s to the early 1970s where he tries to marry his general view of science with evolutionary Darwinian concepts – Popper, after recognizing that the schema $P \rightarrow TS \rightarrow EE \rightarrow P$ represents "a sequence, not a cycle" and that the second problem results different from the first (…)", asserts it should be rewritten to become $P_1 \rightarrow TT \rightarrow EE \rightarrow P_2$, or to take into account the "the multiplicity of the tentative solutions, the multiplicity of the trials" it should finally look approximately like this:

$$\begin{array}{c} \nearrow TS_1 \searrow \\ P_1 \rightarrow TS_2 \rightarrow EE \rightarrow P_2. \\ \vdots \\ TS_n \end{array}$$

([1972] 1979, p. 243).

To Petroski, design, "to make something stand that has not stood before, to reassemble Nature into something new, and above all to obviate failure in the effort." (1985, p. 9), is the essence of engineering practice and, therefore, almost synonym of the engineering method. In fact, most of engineering work seems to be focused on dealing with design problems, i.e., in trying to find ingenious solutions to solve them or to discover clever ways to get rid of design errors present in those solutions.

He thinks of it as a process, going from the conceptualization to the realization of artifacts, passing through their modelization and experimentation, "essentially a timeless, placeless, and even thingless human activity that succeeds only insofar as it anticipates failure" (1994, p. 120). Is has, however, a cyclic nature, since "success ultimately leads to failure and engineering practice to be made more conservative after a failure, and in this way failures lead to new successes." (1985, p. 222; 1994, p. 167).

Petroski shares with Popper the metaphor of the cycle to portray the development of engineering knowledge – a know-how, an understanding of procedures required to manufacture artifacts, complemented by a know why, an understanding of the principles underlying the construction of those artifacts, and a know what, an understanding of the uses and of the users they can have (Garud, 1997, p. 82). That means not only that he believes it evolves in a non-linear or non-cumulative way, i.e., from time to time it goes through more or less revolutionary changes, but also, and more fundamentally, that errors, though they should be anticipated and avoided – it is perhaps the chief duty of all

engineers – play nevertheless a major role in encouraging the renewal of the design process. He calls it the "paradox of engineering design", the idea that "successful structural concepts devolve into failures, while the colossal failures contribute to the evolution of innovative and inspiring structures." (1985, p. 163; also 1994, p. 166), or perhaps it will be better call it the "apparent" paradox of engineering design, because

> "(…) when we understand the principal objective of the design process as obviating failure, the paradox is resolved. For a failed structure provides a counterexample to a hypothesis and shows us incontrovertibly what cannot be done, while a structure that stands without incident often conceals whatever lessons or caveats it might hold for the next generation of engineers." (1985, p. 163).

That success-failure dialectics, says Petroski, can be particularly witnessed in the evolution of bridges. In fact, he adds, each time an innovative design is introduced in such artifacts, for instance the concept of "suspension bridge" around the third decade of the 20th century – after arch bridges and truss bridges – a climate of optimistic confidence and progressive easing on safety standards seems to surround its development that ends changing into the opposite climate of pessimistic distrust about it when a spectacular, sometimes very distressing, and apparently unpredictable failure happens (1985, p. 163 and ff.).

But a similar pattern of "back-and-forth interplay between success and failure" (1985, p. 163) can be followed in the computer industry: the 1910-30 electromechanical paradigm was substituted by the vacuum tube one during the period 1940-50, which in turn was superseded by the transistor paradigm during the 1960-70, and this last one by the integrated circuit since the 1980s. The failure here, although not as impressive and certainly much less disturbing than collapses of bridge have to do with the reaching of miniaturization thresholds in the successive technologies.

Inspired in a research paper by Sybly and Walker (1997) upon the causes of collapses on some of the biggest bridges, Petroski even wonders if the paradox of engineering design does not seem to obey a sort of quasi-law. In fact, an interesting result of Sibly and Walker`s study is that due apparently to the progressive relaxation of the parameters of a much used bridge design type beyond the bounds of safety, a major bridge failure occurs about every 30 years, since mid- 19th century at least, something that suggests the existence of some link between that thirty-year cycle and professional socialization gaps between generations of engineers (1994, p. 168). And, although Petroski didn`t argue for its generalizability, we may certainly ask ourselves if similar temporal cycles are driving too other technological fields (for instance, look at the evolution pattern of the computer industry referred in the last paragraph).

3 Engineers` moral duty: learning from the past.

Engineering, as any other domain of human activity, has a moral dimension, mostly translated into the obligation that each engineer has to be fully prepared to exemplary exert is profession. To Petroski it means that engineers not only are responsible for being constantly updated about the state of the art of their domain after having done their graduate studies and become professionals, but also be entitled to have knowledge of their field`s past at undergraduate level, before enter professional practice, to prevent fall in the intellectual trap warned by the American philosopher George Santayana, here slightly paraphrased, of "those who cannot remember the past errors, were condemned to repeat it" ([1905] 1920, p. 284).

To make his point more clear, Petroski especially in *Design Paradigms* revisited the history of engineering, in particular of structural engineering, focusing the attention in relatively well-known episodes of failure, namely infamous bridge collapses, to identify types of errors, i.e., errors that tend to repeat, some quite elementary, because in the process of transmission of an ever increasing engineering knowledge, is his conjecture, the new generations became ignorant of the field`s past, a situation that obviously has moral consequences.

3.1 Lessons from the history of engineering: a repertoire of errors.

Petroski (1994), I think, is more an inventory of types of errors extrapolated from historical cases of error and judgment in engineering than a clear-cut taxonomy. The author claims there that "errors can occur at all stages of the design process" (p. 15) and that the catalogue of errors he presents is not unique, exhaustive or definitive (p. xi). That been said, let us see some of the sorts or engineering design errors identified by him.

In my understanding, the design errors collected in Petroski (1994) can be gathered in two major sub-types: **logical errors**, broadly understood as thinking errors and, therefore, not reducible to deductive reasoning errors or to logical fallacies; and **psychological errors**, i.e., errors made under the influence of factors other than strictly rational ones.

Belonging to the logical sub-type we have in first place **naïve** – meaning "insufficiently criticized and unsuccessfully scrutinized"(p. 28) – **concepts for improving on familiar ways of doing things**. The historical case favored by the author to illustrate it was take from the oldest surviving text for ancient Greece and Rome architectures, Vitruvius` *De Architectura libri decem* (The Ten Books on Architecture) written in the first century B.C.. It is an anecdote about a certain engineer-contractor named Paconius who bid on the job of moving the pedestal for a statue of Apollo, a stone block with more than 3.5 meters by a little less than 2.5 meters by almost 2 meters, weighing about 50 tons, from the quarry to the temple of Apollo in Selinous. Between the two places, of course, there were several obstacles such as bodies of water, uneven and undeveloped terrain. The Romans know for long at the time how to successfully perform such tasks. They learn from Chersiphron, a Greek architect from the 6th century B.C., that to move a cylindrical column they have to drill axles into the ends, then tow it like a big roller, and also from his son, Metagenes, himself an architect, that to move a square column, they have to fit wooden wheels around the ends, then build a frame around the wheels and tow it like a cart. Paconius imagined what he thought to be an enhancement on those schemes: he built a 4.5 meters diameter horizontal wooden spool around the stone block, then he wrapped the spoon with rope, and after he attached to several oxen the end of the rope coming from the top center of the spoon. What happened was that as the oxen pulled the rope, besides it was very hard to make it start and stop, or to take curves and turns, the spool didn`t roll straight forward, but kept swerving out to one side, demanding constant realignment on the road. The sad result was that Paconius went to bankruptcy (Petroski, 1994, p. 16 and ff.). The lesson to draw is that innovative design ideas should be welcome and praised, but only after public severe examination and testing.

However, as Petroski points, the same kind of error seems to have been made many centuries later when a new design concept introduced in England by the beginnings of the 1960s to erect buildings not through conventional means, but using a system of assembling large precast components (walls, floors, stairways, etc.) transported to the construction site proved to be disastrous after, on May 16, 1968, a block of twenty four story apartments built in East London collapsed in consequence of a gas explosion have blown out of the corner walls of an eighteenth-floor flat and a chain reaction follow on (id., p. 26 and ff.).

A second logical sub-type design error is that of **inferential mistakes about size or scale effects on things performance**. Petroski exemplifies it recalling a story found again on Vitruvius *De Architectura*, about an engineer named Callias that went to Rhodes and after gave an eloquent lecture about a model of a revolving crane designed to sit on the city's wall and capture any enemy siege machine was hired to replace Diognetus as the town architect. But when Rhodes was sieged and the enemies pushed a Helepolis, a 150 tons blockade machine, towards its direction Callias was unable to make a full prototype of the revolving crane with the excuse that the model he had imagine cannot be scaled up to the expected magnitude. The citizens of Rhodes then asked Diognetus for help, who first refused but later agreed to, and using more conventional well-tested means started piercing the wall and pour water, excrement, and filth in front of it through pipes projecting through the opening all night, completely immobilizing the Helespolis and making it an easy prey for the soldiers of Rhodes (id., pp. 32-4).

The story is instructive of design models limitations. However, it seems that the same kind of error was repeated many centuries later when early on World War II the so-called Liberty ships, unprecedentedly large vessels constructed to transport supplies, made not of wrought iron but of steel, and not with riveted but welded joints, start to have catastrophic factures in its structure (id., pp. 40, 56-7).

A third logical sub-type design error may be called **incognizance of design original assumptions**. Petroski select to illustrate it a case taken from Galileo`s seminal work on strength of materials and dynamics *Dialogues Concerning Two New Sciences* (1638). In that book Salviatti, Galileo's alter ego, recounts to his interlocutors Sagredo and Simplicio a story about a seemingly excellent idea that had very unexpected disastrous consequences. It occurred to a watchful engineer when he look at a large marble column laid out so that its two ends rested each upon a support that in order to be doubly sure of its not breaking in the middle by its own weight, it would be wise to lay a third support midway. What happed, in fact, was that the supports at the extremities sunk on the ground and the marble column broke into two pieces. The lesson here is that although the engineer have had a good intention in mind he disregarded the original assumptions made to laid out the marble column, namely that the marble lying on the ground can be soiled, discolored and hard to lift again, that inclining it against awall can cause crack or it may fall, and that the best way to preserve its structural integrity is to setting it down in just two support pieces (id., p. 47 and ff.).

To Petroski the tragedy of the Hyatt Regency Hotel in Kansas enters this same category of design errors. In fact, on July 17, 1981 after had been open for a year, a local radio station organized a dance competition to happen in its

interior atrium and when a crowd between 1500 and 2000 people was there gathered, the second and fourth floor walkways collapsed making many spectators of the event watching it from that place fall and die or get injured as other people that were at the ground level. The cause of disaster was imputed to the puncture of the box beams by the washers and nuts below it that where changed during the building construction, not matching therefore with the original design, provoking the unthought and indesirable consequence of doubling the load on the upper box beam (id., pp. 58-61).

The last logical sub-type design error in author`s list is the much known **fallacy of confirmation**. Petroski goes once more back to Galileo`s *Dialogues* to find a clear example for it. In that work the Italian engineer face the now widely known "problem of the cantilever beam". He formulated it more or less like follows: imagine that a large timber juts out from the ruins of a masonry wall and that a large rock hangs from a hook attached to the end of the timber; if we understand the relation between the strength of the wood, the size of the timber and the weight of the rock, then we can know whether the timber will be able to support the load or will break because of it. Galileo assumed that the cantilever beam will work as a bent lever and that it would break at the fulcrum in the wall if the weight of the rock tending to pull down the timber was two times or more bigger than the area of this last multiplied by half the height of it (admitting that the resisting forces of the material under stress were distributed uniformly). Initially the formula start to be used including a safety factor of 2, i.e., allowing that a beam intended to support a specified weight was designed to hold one several times greater, but some time later, after acquire incontrovertible reliability and the safety factor become progressively lessen, unexpected failures start to happen. That lead to a review of Galileo's assumptions and almost three quarters of century later to the recognition that he had oversimplified the distribution of stresses in the beam, and thus that the safety factor should be 6 and not 2 (id, p. 65 and ff.).

An error of the sort occurred three centuries later, claims Petroski, when some De Havilland Comet, a class of passenger jet airliner developed and built by British aircraft engineers to be the first to operate between continents, after a number of successful flights, in 1952, start inexplicably exploding in midair. More oddly, ruled out weather conditions and pilot errors as probable cause, and introduced systematic design changes in the aircraft, the tragedies continued to happen. However, along the inquiry to the accidents a basic design assumption "that metal fatigue would not be a determining factor in the structural integrity of the aircraft" (id., p. 77) was continually reaffirmed as being sound. Only when a less conventional researcher carried out loading tests on a Comet until its pressurized fuselage failed catastrophically from metal fatigue the cause of the disasters became evident (ibid.).

The errors of the second sub-type are all psychological in nature, and I will limit myself here to briefly characterize each of them without making any reference to the cases of the history of engineering surveyed by Petroski. The first class of such errors can be designated **fixation on normalcy**, meaning roughly that a sort of exaggerate mental disposition to look only for the customary may hinder the discovery of significance of some details or anomalies in design failures (Petroski, 1994, chap. 6). **Tunnel vision** calls the author the second kind, i.e., errors largely caused by an excessive narrowing of the view regarding the design solutions to the point peripheral or alternative visions become obliterated (id., chap. 7). The third category may be denominated **fixation on success** and like the first can be thought of as a sort of pathological *habitus* of the mind to be mainly focused on what works well, failing to anticipate failure (id., chap. 8). Lastly we have a variety of design errors due to an **overconfidence climate**, a group-shared optimism in the apparent ceaseless potential of some design paradigm that grows into hubris, recklessness (id., chap. 9).

Table 1: Petroski`s types of design errors

Logical	Psychological
naïve concepts for improving on familiar ways of doing things	fixation on normalcy
inferential mistakes about size or scale effects on things	tunnel vision
performance	fixation on success
incognizance of design original assumptions	overconfidence climate
fallacy of confirmation	

We can abstract two things from all those types of design errors: their historical recurrence and repetitiveness, especially in the field of structural engineering; and the notion, in retrospect, that they could and therefore should be avoided to spare losses of human lives and destruction of material assets. That, of course, demands explanation. Petroski`s one is that such twofold pattern is rooted in a truncated professional socialization of new generations of engineers.

3.2 Towards a reform of the engineering curriculum: how to train reflective engineers.

If a number of paradigmatic engineering errors are being historically reiterated, it means to Petroski that something is crucially failing in the knowledge transmission between successive generations of engineers. In fact, he is worried with the excessive emphasis of present engineering curriculum in causing students to learn the state of the art of their domain to the detriment of the respective history (1994, p. ix). This educational phenomenon, although not entirely new is intensifying in the last decades, according to him, mainly due to "[p]ressures to reduce overall degree requirements" (id., 185) combined with the widespread view that history of engineering courses are "a cultural luxury, perhaps helpful for giving students a professional pride but not important enough to require in the curriculum" (ibid.) and the sad understanding that "[s]ince such courses did tend to be celebrations of engineering success (…) it was hard to see how they related to designing space satellites or suspension bridges that did not fall down." (ibid.).

Engineers as other professionals are committed to be *a la page*, so to speak, to keep abreast with the state of the art of its discipline. But they also have the responsibility to be as knowledgeable as possible with the past of it. As Petroski says in conclusion:

> History of engineering, especially in a form containing case studies of failures grouped under unifying themes relating to various aspects of the engineering method, would be an invaluable addition to the modern curriculum. And if some of these courses could be taught by reflective engineers who have experienced engineering first-hand, then students could not only learn something of the culture of their profession, they could come to learn from the masters how to develop engineering judgment. In this way, some great lost lessons of engineering could be rediscovered and engineering thinking and judgment sharpened rather than dulled in future engineers (id., 186).

Obliviousness as also ignorance has moral consequences, as ancient Greeks after Socrates were already aware. It is, thus, time, so Petroski thinks, to reform engineers` education empowering it with an indispensable historical perspective and philosophical depth both required to get it rid of unnecessary evils.

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