

SNARES OF POOL BOILING RESEARCH: PUTTING OUR HISTORY TO USE

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ABSTRACT

We look critically at how students of boiling have missed some important lessons of history. In particular, we consider the need for understanding how we acquire scientific knowledge and how we create scientific change in our field. Sorting out our belief systems is the obvious first step toward determining what we should do next and how we should go about doing it.

1. A ROLE FOR HISTORY

1.1 Boiling Studies Today

Only during the 1930s did we engineers begin to recognize the enormous potential of boiling for transferring heat under low temperature differences. Most of us look to Nukiyama's (1934) description of the boiling curve as our point of origin. Thirty years later, Gouse (1964) traced the intervening exponential growth in the number of citations in boiling and two-phase flow in a heroic literature survey of over 6000 citations. Now a second 30 years has passed and no one is about to *try* to do what Gouse did, today. By now, any such list would be far too long.

But, ~~with all that has been done~~, few among us would claim any thoroughgoing confidence in our understanding of the physical mechanisms of this extraordinary heat removal process. The hope of predicting pool boiling heat removal rates evaporated during the 1950s. The prediction of burnout has ~~mired in acrimony since the 1940s~~ -- acrimony in which I have taken part. You and I have written tens of thousands of papers on boiling. Yet our knowledge of this terribly important subject remains terribly flawed. Something is wrong and it behooves us to back off and ask what that something might be.

Our history (see e.g., Lienhard and Witte (1985)) casts some light on why we have had such trouble. Let us consider two features of our debate which are easy to overlook. The first has to do with *where* we look for new knowledge. The second has to do with our ability to seek out real *change*.

1.2 Eye of the Forehead, Eye of the Mind

In the late 1500s, an Italian boy took up the study of art. He was Galileo and his work in art, in external observation, is the key to his science. Despite that fact, Galileo consistently expressed the belief that understanding came, not from the observational eye of the artist, but from within his own head.

We have two ancient models for those two modes of thought. Aristotle taught us to observe nature and to make scientific inferences from observation. He set up our modern scientific method. Plato, on the other hand, believed that we gained a scientific understanding by purely mental processes.

For example: Platonic Greeks used *deduction* to decide that the world was round. All heavenly bodies are round. The sun is round, the moon is round. We float in the same firmament. Earth must *also* be round.

Aristotle drew the same conclusion by generalizing a direct observation. He watched ships sail off to the horizon. The bottoms vanished first and the sails last. That meant Earth's curvature obstructed more of the boat as it got further and further away. Therefore, Earth must be round.

If you read Galileo describing how he dropped balls from the Tower of Pisa to learn they fall, he seems to be describing, not an experiment in real life, but a thought experiment -- done only within his own head. He *talks* like a Platonist.

Yet we know Galileo looked closely at the external world around him for understanding. There are details in his Tower of Pisa argument that no one could have imagined without having actually *done* the experiment (Edgerton (1984).) He really *did* that experiment, and many more as well. Galileo talked like a Platonist, but behaved like an Aristotelian. And he gave us a wonderful phrase. He spoke of seeing with the *Eye of the Forehead*, or seeing with the *Eye of the Mind*.

You and I struggle with that same dialectic tension. Which eye do we use to look upon boiling? Psychologists (e.g.: Kiersey (1984)) find that 75 percent of our population is more Aristotelian than Platonist. That's an important number. It means that most of us expect to learn about boiling by reading our meters, photographs, and computer outputs. Only 25 percent of us seek to calculate truth from within our own heads.

Two important students of boiling illustrate these two poles. For four decades, J. Westwater at the University of Illinois provided very pure observation. Using the tool of photography he provided us with state of the art pictures (See, e.g., Westwater (1955.) See also Gaertner and Westwater (1960).) Westwater scrupulously avoided imposing the constructs of his own mind upon his observations. He tried to let nature speak directly to him.

One of the purest Platonists, Yan-po Chang (1957), was first to suggest that Taylor and Helmholtz unstable waves were important to the vapor removal processes in boiling.

Chang made this suggestion in the context of his analysis of natural convection from an infinite horizontal flat plate. While his use of thermo-convective instability theory did not convince students of natural convection, Chang concluded his paper by drawing attention to the shape of the liquid-vapor interface in film boiling. That interface, he claimed, has to be the result of an instability. He verified this by noting that thermo-hydraulic wavelengths compare favorably with those observed in film boiling.

I met Chang during the 1958/9 academic year. I was a young graduate student just building a flow boiling visualization apparatus at Berkeley; he was a visiting professor. I took him into the lab to see my apparatus. He gazed at the bubble motions as they were frozen by a blinking strobe light. He sighed and said a remarkable thing to me:

"I have *dreamed* of bubbles.
But I have never *seen* bubbles."

And I realized that, for Chang, this was an aesthetic experience, and perhaps, an experience that confirmed what he already knew. But this did not seem to be a place where he would expect new truths to reveal themselves to him.

Chang's contribution is largely overlooked these days. But he deduced a truth that we are still trying to sort out, 37 years later. He did so, not by making experiments, but by creating imaginative constructs in his own mind.

This is not a simple question of experimentalist vs. theoretician. We find Platonists who conduct experiments. We find Aristotelians who invent physical models and execute computational programs. This is a more basic, and rarely talked-about, difference in human attitude. If we better understood it, we would do better at resolving many of our arguments. I return to this matter subsequently. First, however, another historical blind spot.

1.3 A Kuhnian Diagnosis

Perhaps the most striking feature of . . . normal research problems . . . is how little they aim to produce major novelties . . .

Thomas S. Kuhn

Soon after Chang first linked boiling processes to hydrodynamic instability mechanisms, Thomas Kuhn (1962) shocked historians and scientists alike with a new theory of scientific change. Kuhn flew in the teeth of scientific mythology when he said that, *if progress occurs in science, it by revolution and replacement, not by the accretion of knowledge.* The history of modern science from Galileo onward is told in a sequence of new constructs replacing old ones.

Kuhn showed, by a powerful and systematic analysis of historical case histories, that scientists normally have little interest in new ideas. Most science consists of working out the problems which current scientific tools are equipped to handle. We in heat transfer continue to do the things we have learned to do so well. We solve the heat conduction equation, we measure vapor bubble growth rates, and so forth.

It is only when scientists get into trouble that they begin to look for novel solutions to their problems. When both Rayleigh and Wien (see, e.g., Tien and Lienhard (1979)) failed to predict the spectrum of radiant energy, Plank finally did something novel that made no sense. He assumed that energy assumed one of a sequence of discrete values. That assumption was not only unreasonable, it was also incorrect. But it put us back in motion and it changed physics forever.

Kuhn says that, when a field struggles with an unresolved anomaly, it goes into a state of *crisis*. He points to the following features in what happens next:

- We don't recognize the coming crisis. We see only a speck of dust -- a small, seemingly unimportant anomaly -- a detail that we haven't quite figured out how to incorporate in our normal way of doing business.
- As we fail to account that detail, we refine our normal procedures. We measure more accurately, refine our technique, etc.
- As our determined efforts fail, we begin writing an increasing array of codicils and exceptions to our laws in an attempt to make them fit the facts.
- We splinter into various competing schools of thinking.
- Eventually people whom we consider to be marginal to, or outside of, our "profession" offer a new theory or a new way of looking at things. For those of us in the field those people, and their ideas, are invisible.
- If the theory has no obvious exceptions, the outsiders attract support to their cause. Eventually they simply replace, or drive out, the old way of thinking and a new pattern of normal science comes into being.
- Past revolutions are invisible because we systematically write them out of our textbooks. We only write about past science in the language of the *prevailing* paradigm.

We trace this process in an area of boiling, in Section 3. While our problems in, say, film boiling and film condensation may be severe, they are probably still amenable to our normal kit of analytical and experimental tools. There is, on the other hand, ample evidence that Kuhn's description of a scientific *crisis* fits us all too well in nucleate boiling, transition boiling, and in predicting the burnout heat flux.

Thus history offers at least two questions that will help us if we can answer them. One is: "How do we gain new knowledge of boiling?" The other is: "How do we overcome our own resistance to revising *old* knowledge?"

2. WHERE SHOULD OUR HISTORY BEGIN?

2.1 Boerhaave and Leidenfrost

Our conventional history begins with spit on a hot stove -- with what we call the Leidenfrost phenomenon -- the odd way that drops of water survive on a stove where it is red hot, but quickly evaporate away where the stove is cooler.

Kistemaker (1963) traces study of this phenomenon back to Leidenfrost (1756) (where we refer to Wares' and Bells' translation and discussion of the relevant portion of Leidenfrost's tract.) Kistemaker points out that Leidenfrost referred to two scholars who had done the experiment before him. They were Eller (1746) and Boerhaave (1732). Of course, Boerhaave could hardly have been first. The phenomenon was simply too familiar. But there the trail goes cold.

And we are left with a tantalizing question. Why should interest in the mechanics of boiling only have arisen 250 years ago? The reason traces directly to the matter of Platonic and Aristotelian thinking in boiling which has plagued our field since long before Leidenfrost.

2.2 Alchemy

Neo-Platonist Alchemy surfaced as the dominant scientific force in the 13th century. Alchemy was the study of chemistry as it had been done ever since Aristotle talked about *earth, air, fire, and water* (see, e.g., the works of Debus (1967,1977) and Taylor (1949).) Alchemy was the knowledge of how we *transmute* matter by the action of heat and cold, or dampness and dryness. First the Greeks developed those ideas. Then Arab scientists picked them up. Since alchemy occasionally mired itself in metaphysics, the practical Romans virtually ignored it. As civilization spread North into Europe, alchemy all but vanished until scholars began rereading the old Greek and Arab texts.

The hope of transmuting baser metals into gold may have been vain, but its spin-off was enormous. By trying to understand transmutation, alchemists learned about practical metallurgy. They learned about extracting metals from ores and about chemical reaction.

The alchemists generally used Aristotle's science, but they ignored his scientific *method* of reaching truth by observing nature. Alchemy was, in fact, a very pure Platonic pursuit.

Since heating and cooling, dampness and dryness, were fundamental alchemical processes, distillation and reflux condensation were primary tools of the alchemists. They were deeply involved with the processes we study.

Why, then, is their work so invisible to us?

2.3 17th C. Scientific Revolution

An intellectual shift had gone on from the early 15th century through the early 17th century. A new Aristotelian concept of external observation had begun with artists. Leonardo DaVinci, for example, made his art into a scientific tool. By the early 1500s he had become the best informed anatomist of his age. (see, e.g., Belt (1955) and Clayton (1992).)

Figures 1a and 1b provide a dramatic, but quite typical, contrast in how art had altered scientific vision shifted in just a few years over the turn of the 15th century. Figure 1a, taken from Schönsperger's (ca. 1485) *Herbarius* -- a very early printed book of herbs. It is the Platonist invitation to understand the etymology of the word *mandrake* and to feel its symbolic import. But it gives little guidance if you want to find a mandrake root in the forest.

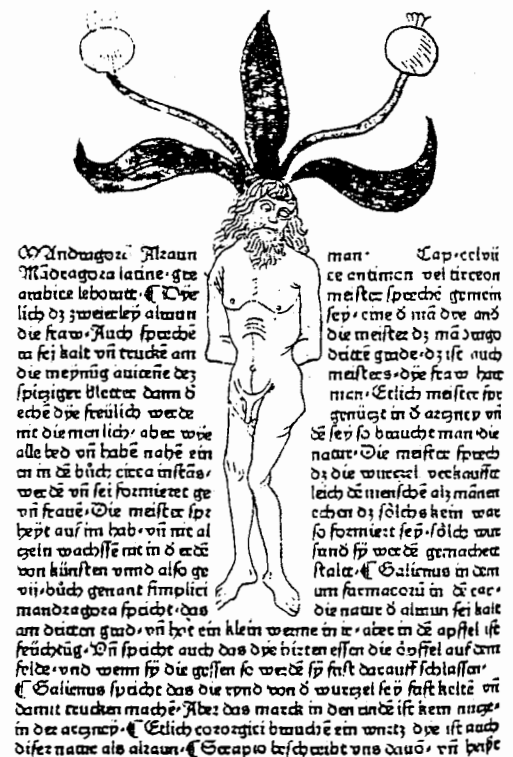


Fig. 1a Imaginative woodcut illustration of a mandrake root from Schönsperger's *Herbarius*, ca. 1485.

Only 46 years later, Brunfels (1531 or 32) published a famous book of herbs. Figure 1b shows us how far scientific observation had come in little over a generation. This is Aristotelian observation of a high order.



Fig. 1b Realistic woodcut representation of the herb, satyrium from Brunfels *Herbarium Vivae Eicones ad Naturae Imationem*, 1531 or 32

Dürer (1525, 1965) had meanwhile added two new tools to the arsenal of external observation. He codified the rules of perspective and descriptive geometry (see Fig. 2.) He also perfected the new art of copper-plate engraving (see Fig. 3.) Copper plate made it possible to include representational art of extraordinarily high quality to the new printed books. Books began reflecting the forgotten Aristotelian doctrine that we learn by observing nature.

One by one new, experimental scientists and engineers began using the new print media – first improved woodcuts, then copper plate engravings. Aristotelian Botanists, anatomists, surgeons, miners, zoologists, and geographers all began walking around the Platonist academic establishment.

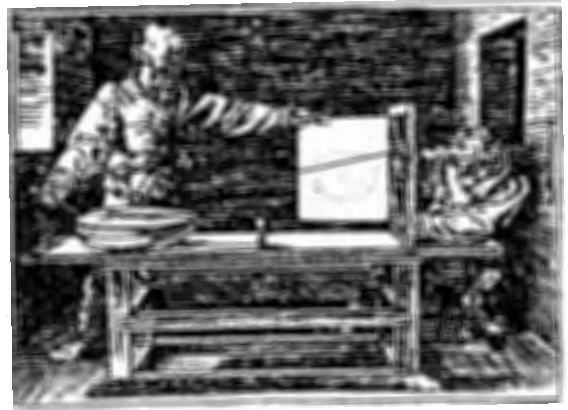


Fig. 2 In this woodcut, made in 1525, Dürer shows how to use create accurate perspective using mathematical projection.



Fig. 3 Dürer's copperplate engraving of Saint Jerome in his study, made in 1514. Dürer still reflected the symbolic wealth of alchemical thinking, but he brought representational art to a new level.

It was after Galileo's death in 1642, that the new visual Aristotelian science became the dominant tool of the academic establishment. But it should be no surprise that the old Platonic alchemy died most slowly in the field of chemistry itself. Robert Boyle (1627-1692) led the assault on alchemy in England. And Hermann Boerhaave (1668-1738) was Boyle's European disciple.

2.4 Alchemy and Phase-change

Boerhaave did not attack alchemy frontally. Rather, he refined alchemical technique (as Kuhn tells us is typical in the late stages of any scientific revolution.) He did the old alchemical distillations in increasingly refined attempts at transmutation. By failing, Boerhaave made it overwhelmingly clear that it was arguments based on the Aristotelian essences which had failed. The alchemical eye of the mind had run to the end of its tether. It was now seriously in need of help from the eye of the forehead.

Alchemical physicians had practiced what they called *iatrochemistry* -- literally the *alchemy* of healing. Boerhaave responded by taking up *iatromechanics* -- the *mechanics* of healing. He looked back to the work of the anatomist, Vesalius. Vesalius (1543) clearly broke with iatrochemistry and he very clearly sought to understand the mechanics of the human body as revealed by autopsy. Figure 4 shows a typical Vesalius illustration. No mystic essences here -- just pure observation of the mechanics of the human body!



Fig. 4 A completely representational illustration of a human dissection by Vesalius (1543)

That shift is important. It was a mark of the new empirical scientists that they looked at *mechanisms* of processes. They were not content to deduce what would happen in a chemical process -- they, and Boerhaave in particular, now turned their *eyes* upon the process to see *how* it happened.

So it is no surprise that Boerhaave finally looked at the mechanics of the phase-change process. He described how an alcohol drop floats upon its vapor over a heated iron. Twenty-four years later, Leidenfrost did that experiment quantitatively. He measured the survival times of both water and wine drops upon an iron spoon.

As he reported his experiment Leidenfrost criticized Boerhaave for failing to separate himself enough from alchemical thinking. By now the new observational science was clearly in the driver's seat. Still, Leidenfrost uttered his criticism in the cautious language of an acolyte questioning God.

Leidenfrost told us how long the droplet lingered on an iron spoon heated to different levels. Yet he added not a whit to our understanding of *why* the water survived longer at higher temperatures. Questions as to *why* things occurred drove the alchemists directly to the invention of hypotheses. Newton, by now the patron saint of the new rational science, had disposed of such questions summarily. In response to the question, "Why is the law of universal gravitation true," Newton said simply, "I do not make hypotheses."

When we stop and reflect upon that statement, most of us would agree that Newton simply knew when to quit. First we do as much as we can before we run up against our ignorance. But we sometimes go a lot further than Newton did. We often create a correlation of, say, nucleate boiling data without even asking what its rational meaning might be. Newton knew where the eye of his mind ran out. Many of us feel too little obligation to open that eye in the first place.

Oddly enough, *fire* was the last of the old Alchemical essences to fall. While Boyle and Boerhaave were putting portions of alchemy under assault, late 17th century alchemists were reshaping the the essence of fire, first into what they called "the fatty earth," then into *phlogiston*, the essence released by combustion. By the late 18th century the essence of fire *still* lingered as *caloric*.

Alchemical distillation apparatus traces back to a woman who probably lived in Alexandria before the Birth of Christ. This inventor is known to us by the name of *Mary the Jewess*. Mary gave us an elementary reflux condenser and the three-armed still shown in Fig. 5. The standard still was written about by Synesius of Cyrene -- a philosopher and alchemist who studied with another woman, the neo-Platonist mathematician, Hypatia. (He credited the invention of the still to Democritus.) By the 16th century, alchemical stills had grown quite sophisticated (see, e.g., Fig. 6.)

During the early 1650s, a Hermetic alchemist and Anglican priest named Thomas Vaughn (see, e.g., Taylor (1974)) wrote about phase-change processes involved in distillation, but he did so in the coded alchemical terms that so startle and baffle our 20th century ears.

In this impassioned poetical prayer, droplets become tears, vapors become spirits. The imagery of process alchemy is shot through Vaughn's poem. (The Italics are Taylor's):

My God! My *Heart* is so,
'tis all of *Flint*, and no
Extract of Teares will yeeld:
Dissolve it with the *Fire*,
that something may aspire,
And *grow* up in my field.

Bare Teares Ile not intreat,
but let thy *Spirits seat*
Upon those *Waters bee*,
Then I new form'd with *light*
shall move without all *Night*,
Or *Excentricity*.

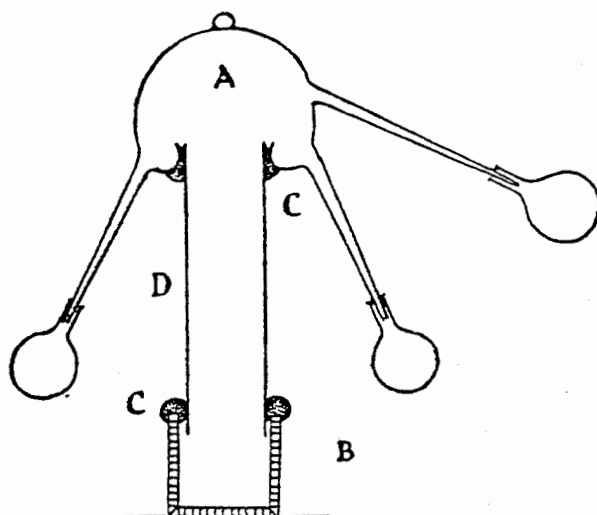


Fig. 5 Mary the Jewess' three-armed still (probably Hellenistic) (from Taylor (1974))



Fig. 6 A large-scale still from ca. 1510 (from Taylor (1974))

So distillation and reflux condensation were ancient arts, in wide use by the time Boerhaave first wrote about what we call "the Leidenfrost phenomenon" in 1732. It and processes like it were well known. By analogy: 19th century Africans correctly told the explorer, Richard Burton, that mosquitoes carried yellow fever. He laughed at natives who didn't understand that bad air simply arrived during mosquito season.

And we credit Walter Reed for having discovered that mosquitoes carry yellow fever, a half century later. That's because he dressed what Africans already knew in the clothing of the science we understand today.

2.5 History's Lesson

We have just described the quintessential Kuhnian scientific trap. When knowledge is not articulated in terms familiar and acceptable to the current scientists, it is invisible knowledge. And very often, scientists decide what is acceptable and what is not by whether it is articulated in Platonist or Aristotelian terms. We turn next to a case study from our own field of pool boiling: The continuing attempt to determine the mechanisms of boiling burnout. (This case draws heavily on Lienhard and Witte's (1985) review.)

3. THE CHECKERED HISTORY OF THE HYDRODYNAMIC THEORY

3.1 The Hydrodynamic Theory

Yan-po Chang was invited to a stay at UCLA just after he produced his 1957 paper on thermal instability and natural convection. There he discussed his ideas with Myron Tribus and Tribus' student, Novak Zuber, who was developing his dissertation on the hydrodynamic processes in boiling.

Zuber, fluent in Russian, was exploring the recent Russian literature with more care than anyone in the United States had previously given it. He had also just finished a terribly demanding course based on Milne-Thompson's (1960) book on *Theoretical Hydrodynamics*. He was as well-informed on Taylor and Helmholtz instability theory as anyone could be.

What we call the *Hydrodynamic Theory of Boiling* has been in the public eye ever since Zuber (1959, 1963) did his Ph.D. Dissertation under Tribus in 1958. The dissertation, titled *Hydrodynamic Aspects of Boiling* was an audacious and far ranging study that awakened passionate and heated opposition. It was a work which, while not correct in all its details, ranks among the small handful of true milestones in a staggering profusion of writings.

To speak of a hydrodynamic theory of boiling seems hopelessly unrestrictive since every aspect of boiling involves hydrodynamic processes in which vapor leaves a heater while liquid moves in towards it. The name generally refers to the several transitions in boiling behavior that result from hydrodynamic instabilities in the vapor escape processes.

These transitions include the *peak heat flux*, or *burnout-transition*, the transition from isolated bubbles to slugs-and-columns action in nucleate boiling, the *minimum heat flux* which may or may not be a true hydrodynamic transition, and one or more transitions in the "transition boiling" regime that may be dictated by contact angle behavior.

In addition to hydrodynamically dictated transitions, much of the regular vapor escape behavior in boiling is dictated by the cyclic collapse of Taylor-Helmholtz unstable vapor-escape structures. The two most notable examples are the Taylor wave "bubble-escapement" pattern in film boiling and the placement of liquid jets near the peak nucleate boiling heat flux.

This body of theory evolved only after observations of the various boiling instability phenomena had become sufficiently well-defined to permit its articulation, and after the post-war extensions of the Taylor-Helmholtz theory had provided the tools for its formulation. Let us trace the historic antecedents of the theory; describe its formulation and its early struggle for survival; follow its exploitation during the 1960's and 1970's, and show where the theory presently fails to provide answers to some major questions.

Zuber's original formulation of the Hydrodynamic Theory began with the least understood aspect of boiling behavior, namely *transition boiling*. For years the theory was articulated without significant reference to this regime. Now new discoveries about the behavior of transition boiling are leading us to reinterpret the theory.

3.1. Early History of "Boiling Curves"

It is significant that the earliest systematic description of boiling quoted in the literature deals with the major hydrodynamic transitions that mark the boiling process. When Leidenfrost introduced drops of water into the bowl of a heavy metal spoon heated to different temperatures, he found that if the spoon were heated only slightly above the normal boiling point of water, the water would bubble away within a second or so. But when the spoon was made red-hot, the water would pull into spherical drops in the bowl of the spoon, and take upward of a minute to convert to steam.

Kistemaker (1962) attributes the term "spheroidal state" (in reference to the droplets) to Boutigny in 1843, and credits Stark with later studies of the form of the drops in 1898. Kutateladze ((1952), Chapter 9) refers to more quantitative 19th century observations made by Gezekhus in 1876. He plotted the survival time of a standard drop of water, as the surface temperature was varied.

Nukiyama's (1934) experiment was, in a sense, a quantitative recreation of Leidenfrost's experiment and we have long regarded it as the first modern consideration of the boiling transitions. Nukiyama devised the experiment, sketched in Fig. 7, and it did a great deal to clarify Leidenfrost's observation.

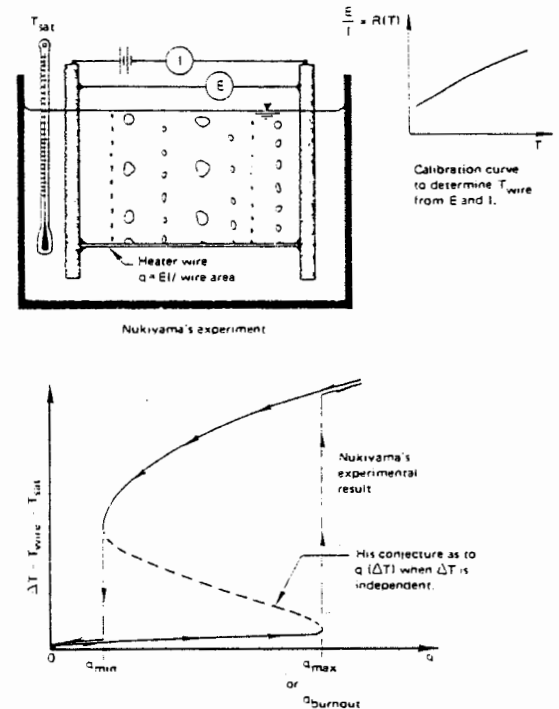


Fig. 7 Nukiyama's experiment and his boiling hysteresis loop.

Nukiyama used a horizontal wire both as a heater, and as a resistance thermometer to obtain matching measurements of the heat flux, q , and the wall superheat, $T_{\text{wall}} - T_{\text{sat}} = \Delta T$. The result was an enormous hysteresis loop. First the wall superheat increased very little as the heat flux rose to a very high value. Then it suddenly leapt by a thousand or so degrees. After that, the wall superheat stayed quite high while the heat flux was reduced to a very low value. Finally, the wall superheat suddenly dropped back to almost nothing.

He not only observed the peak and minimum heat flux limits, he also speculated that (had he been able to vary the wall superheat independently) he would have been able to measure the connecting dashed line in Fig. 7.

Of course, boiling had not been ignored during two centuries from Leidenfrost to Nukiyama. In his paper, Nukiyama cited previous measurements of the q vs. ΔT relationship in nucleate boiling by Austin in 1902, and by Jakob and Linke in 1933. Those data showed q rising with ΔT , but did not suggest that the rise might reach a limit.

Furthermore, Nukiyama did not actually present the first boiling curves. Early in this century, metallurgists concerned with the influence of cooling rates on the metal structure did quenching experiments. Pilling and Lynch (1919) quenched 0.64 cm dia. cylindrical samples with thermocouples embedded in them. Their concern was not with heat transfer, q , but with the cooling rates ($^{\circ}\text{C/s}$) which are virtually equivalent and which vary the same way q does.

Figure 8 is a copy of one of Pilling and Lynch's plots of cooling rates as a function of cylinder temperature, for several initial water temperatures. These curves -- particularly for quenches in near-saturated water -- have the same general form as the boiling curve in Fig. 7, with the coordinates reversed. Pilling and Lynch quite accurately identify the film boiling region as "Cooling in vapor," the transition through nucleate region as "Cooling by active vaporization," and the natural convection region as "Cooling in liquid."

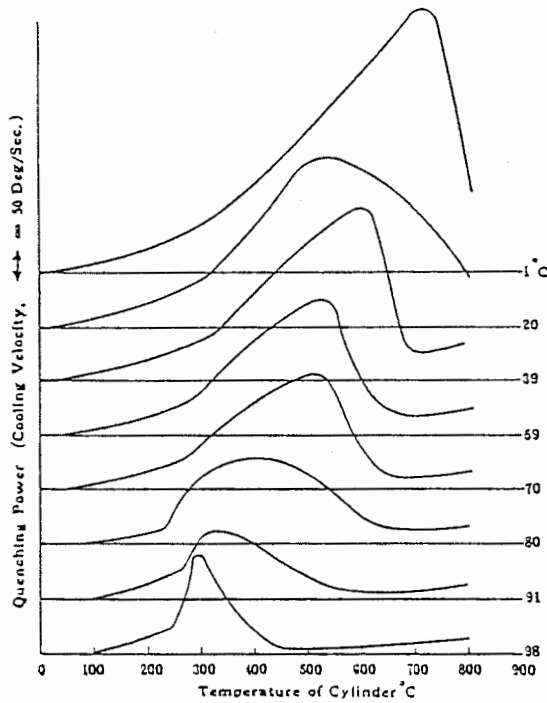


Fig. 8 One of Pilling's and Lynch's sets of quenching curves.

The presence of the Pilling-Lynch paper dramatizes a dilemma that faces any historian of science or technology when he tries to discuss *priority*. Perhaps we are tempted to claim that Pilling and Lynch do not deserve credit for discovering the multivalued character of the boiling curve because they did not explicitly address heat transfer even though they *do* address *cooling*.

You and I feel we must speak of this behavior in terms of heat flux, and their work seems out of context. Yet it is clear that, had Nukiyama known of these data, he would have been obliged to write about his own experiments in vastly different terms. Indeed, our understanding of things would almost certainly have been hastened, had Nukiyama known of the Pilling-Lynch work or, for that matter, even the earlier Gezekhus experiments.

This may seem a philosophical point, but it comes back to haunt us. Any very clever interpretation of nature inevitably has precursors that have failed to claim people's attention.

The person who eventually stands to receive credit for inventing a new "scientific" description is the one who finally manages to present it in such a way, *or at such a time* as to cause the current practitioners' view to shift discontinuously.

Three years after Nukiyama's work Drew and Mueller (1937) did a crude qualitative experiment that approximated a controlled surface-temperature situation. Their strategy involved heating a copper tube from within, with condensing steam, and boiling a more volatile liquid outside. They were thus able to fill in a few points which suggested that Nukiyama's conjectured dotted curve actually existed.

Figure 9 is a reasonable scale representation of the actual boiling curve that one might actually get in a well-executed modern experiment, using boiling water at atmospheric pressure on a large, clean, smooth, not-too-well-wetted, flat plate, not too much larger than the wave structures above it. These curves are frequently drawn horribly out-of-scale in heat transfer books. Authors who have not worked in boiling have a hard time believing how great the hysteresis effect is. We identify five regions in this curve:

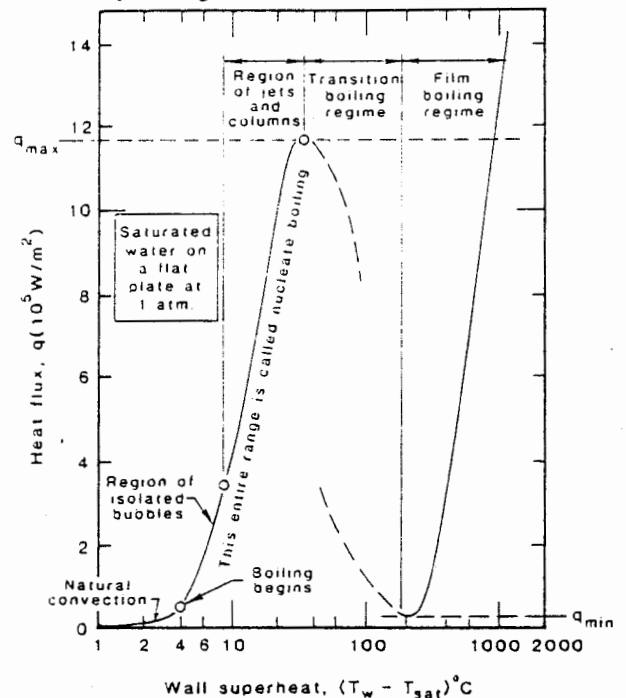


Fig. 9. Typical boiling curve and regimes of boiling for water on an some unspecified flat heater.

- The first region is that of *natural convection*, in which the slightly-superheated, single-phase liquid buoys off the heater.
- There are two distinct nucleate boiling regimes: the region of *isolated bubbles* at lower heat fluxes . . .
- . . . and the region of *slugs and columns* at higher heat fluxes. The region of slugs and columns ends at the *peak* or *burnout* heat flux, q_{max} .

- The region in which the heat flux decreases with wall superheat is the *transition boiling* region.
- The leg on the right is called the *film boiling* regime, which may or may not be limited on the left by the *minimum heat flux*, q_{\min} .

Our failure to represent transition boiling with a definite continuous line; our refusal to identify all points above q_{\min} on the right as film boiling; and our suggestion that Drew and Mueller's experiments might not actually represent a true independent specification of ΔT -- all of these matters represent changes in thinking about the boiling curve that arose many decades later. But, at the time, Drew and Mueller's crude experiment had firmly established a picture of the boiling curve in peoples minds.

Kuhn's essay on the nature of scientific revolutions does much to explain what occurred subsequently. Kuhn argued convincingly from historical precedent that scientists repeatedly see what they expect to see in experiments. Nukiyama, and Drew and Mueller, told us that the transition boiling regime should form a continuous curve between q_{\max} to q_{\min} and that is what subsequent investigators found.

3.3 The Transition Boiling Problem

The famous boiling curves of Farber and Scoriah (1948) illustrate Kuhn's claim. They offered q vs. ΔT data for pool boiling on small wires at various pressures, using electric resistance heating which, they conceded, is very unstable. They measured temperature with an *external* thermocouple. They got smooth, continuous boiling curves through the transition region and claimed that it was "frequently possible to move the boiling process through the conditions [along the transition regime]" by the artful manipulation of the electric supply -- something no subsequent investigator could do.

Sakurai and Shiotsu (1974) were still trying to penetrate the transition regime with a q -independent experiment 20 years ago. But *their* artful manipulation of the electric supply was done with a sophisticated feedback control system that controlled the temperature of the wire (a system developed by Peterson and Zaalouk (1971)) and their temperatures were obtained by measuring the resistance of the wire. Sakurai and Shiotsu's transition boiling data formed a hysteresis loop within the already multiple-valued boiling curve. But their text made it clear that parts of their curve were averages of film and transition heat fluxes co-existing on the heater.

By the same token, Farber and Scoriah gave no photos of boiling and it is possible that they too were presenting mixed-mode boiling results in the transition regime.

Zhukov and Barelko (1983) supported these suspicions when they concluded that the "usual technical means of controlling the temperature regime of a heat generating element are insufficient to provide reliable information on the true boiling curve in the transitional region."

Only by using sophisticated electrical control systems could measure what they consider true values of the burnout and minimum heat fluxes on thin wires. Even then, they could not trace out the entire transition boiling regime.

Indeed, Auracher (1992) recently reviewed such measurements and he leaves us with the unhappy conclusion that we still need better physical models and better experimental results than we've come up with so far. I might paraphrase that by saying that we need better combined sight of the Platonic eye linked with with the Aristotelian eye.

Even systems heated by condensing vapors cannot generally trace out the entire transition regime; yet Drew's and Mueller's inference led many investigators to draw continuous lines connecting points measured in the transition regime to the burnout and minimum heat flux points. This was common practice through the 1950s, 1960s, and into the 1970s. After Drew and Muller's qualitative demonstration, Pramuk and Westwater (1956) refined the technique. The uncertainty of their points in the transition regime was still high -- on the order of ± 20 percent. They also included a smooth line through these data.

Four years later, Berenson (1960) made the most carefully contrived measurements of transition boiling up to that time. He boiled several organic fluids on a thick horizontal copper plate heated by water condensing under pressure on the finned bottom side. He carefully noted the effect of varying the finish and surface chemistry of the upper boiling plate.

Berenson's system for regulating ΔT had an important weakness that was recognized a few years later by Stefan (1965, 1966), Kovalev (1968), Grassman and Ziegler (1969), and Hesse (1973), who pointed out that in such systems

$$q = (T_{\text{cond. sur.}} - T_{\text{sat}}) / \Sigma R_t = (T_w - T_{\text{sat}}) / R_b \quad (1)$$

where R_b is the thermal resistance of the boiling process and ΣR_t is the sum of the thermal resistances between the condensing steam and the saturated boiled liquid. And the conventional ΔT is the difference between the overall ΔT and the temperature drop up to the boiling surface.

$$\Delta T = (T_{\text{cond. sur.}} - T_{\text{sat}}) - (\Sigma R_t - R_b)q \quad (2)$$

In other words, the system can only reach those $(q, \Delta T)$ pairs on q vs. ΔT coordinates that lie on lines passing through ΔT on the abscissa with a negative slope equal to the inverse thermal resistance of all elements between the condensing steam and the boiling surface.

Figure 10 shows a typical set of Berenson's data with the continuous line he originally drew through the data removed. Slanting "access lines" given by equation (1) are overlaid on the curve. These are based on Berenson's copper block resistance ($0.0001514 \text{ m}^2 \cdot ^\circ\text{C}/\text{W}$) and, since the bottom side was finned, zero condensing resistance. (To neglect condensing resistance is conservative. It makes the access line less steep and more of the curve inaccessible.)

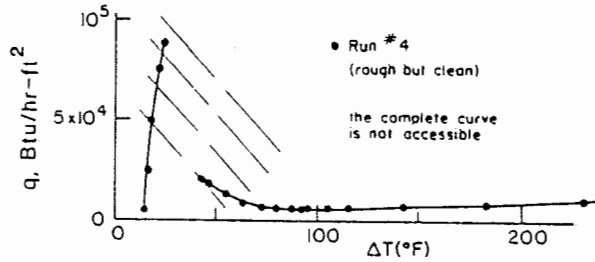


Fig. 10 A set of Berenson's data for n-pentane boiling on a roughened copper surface. Dashed lines are Lienhard and Witte's accessibility curves drawn in accordance with equation (2).

It is important to recognize that only the lowest points on the access curve can be reached from the film boiling side, and only the highest can be reached from the nucleate boiling side. When the curve is triple-valued along the access line, the middle points cannot be reached. Thus *much of the transition zone was inaccessible to Berenson, and he had no legitimate way to infer that a continuous path really existed.*

So it is clear that Kuhn's principle was at play in these early studies of transition boiling. They were done by experimentalists of unquestioned ability and integrity in the best-equipped laboratories of the day. Yet each saw the transition boiling behavior Nukiyama had said they should see.

Witte and I (1982) called this notion into doubt, claiming that the wetting characteristics of the surface could sometimes shift in the transition regime and give two distinct curves. Today, our claim is under debate, but -- as Auracher has noted -- we have grown less sure of the character of the transition boiling regime than we were 20 years ago.

Another feature of Kuhn's scenerio is manifest in the story of transition boiling. The heat transfer community credits Hesse, Grassman, and Ziegler with equation (2). In fact, the person who first noted that temperature is not the true independent variable when you condense a vapor below the heater, was Adiutori (1964).

Adiutori played a role outside of the orthodox heat transfer community and he published his work in a journal few of us took the trouble to read. Kuhn warns that ideas coming from outsiders are invisible within the practice of normal science. Today, we are still hard pressed to find Adiutori's original paper cited in our literature.

3.4 Precursors of the Zuber-Tribus Theory

In the early 1940s, Bonilla and Perry (1941) and Chichelli and Bonilla (1945) measured q in nucleate boiling up to q_{\max} , with q as the independent variable. These experiments were done in water and various organic liquids on a large horizontal flat plate under well-controlled conditions.

Although the role of geometry in the problem was not diagnosed until many years later, these formed the first large data set free from geometrical effects or other serious systematic complications.

Bonilla and Perry include one modest observation in their paper which is a starting point for the hydrodynamic theory of boiling. They say:

As film boiling is similar in effect to column flooding, a similar type of correlation may hold. In Figure 20 our maximum boiling rates are plotted on a column flooding correlation.

Their "Figure 20" shows burnout data correlating within a factor of two on the following coordinates:

$$v_g^2(2g\rho_f L) = f(\rho_f/\rho_g) \quad (3)$$

where v_g is the rate of vapor outflow and liquid inflow (at the peak heat flux) averaged over the heater area, and L is a characteristic dimension which they related to a Jakob prediction of the escaping bubble diameter.

We can find different things to criticize today about the way in which Bonilla and Perry drew the analogy between the peak heat flux and the flooding of a distillation column -- depending upon the school of thought we embrace. Nevertheless, *their perception that the escape of vapor from a heater during boiling will, in some way, strangle the inflow of liquid in the same way that it will in a distillation column, is the heart of any hydrodynamic theory of burnout.*

Yet this observation was not merely ignored. It was actively opposed by two of the greatest chemical engineers of that day. Bonilla (1979) provided me with two remarkable letters. A.P. Colburn wrote to him:

A correlation [of the flooding velocity plots with] boiling data would not serve any great purpose and would perhaps be very misleading.

And T.H. Chilton also wrote:

I venture to suggest that you delete from the manuscript . . . the relationship between boiling rates and loading velocities in packed towers.

Although the correlation did appear in print, Bonilla was clearly discouraged. He later wrote of the transaction:

. . . I dropped further effort in finding a model . . . that would be more "scientific." . . . It seems to me that a substantial number of professors, etc. saw my "burnout" correlation, but just weren't convinced enough to use it.

In the late 1940s Kutateladze (1948) began studying boiling burnout (which he called the "first boiling crisis") with the idea it should be similar to the "flooding" that occurs when a gas is forced up through a liquid. The 1952 revision of Kutateladze's book on two-phase heat transfer included three chapters on which Borishansky collaborated. Chapter 10 reflects Borishansky's dissertation on the dimensional analysis of the equations that would describe such flooding.

Kutateladze and Borishansky developed many dimensionless groups. One was a dimensionless peak heat flux, later named the *Kutateladze Number*, Ku . A comparison of Ku with the limited existing data revealed that it did not depend strongly on the other groups. Thus

$$Ku \equiv q_{\max} / [h_g \rho_g^{1/2} [\sigma g (\rho_l - \rho_g)]^{1/4}] \approx \text{Constant} \quad (4)$$

If we recognize that Bonilla's v is equal to $q_{\max} / \rho_l h_g$ and that $[\sigma g (\rho_l - \rho_g)]^{1/2}$ is a characteristic dimension associated with capillary wave action in a liquid-vapor interface, then it is clear that Bonilla's dimensionless burnout heat flux (in equation (3)) is exactly Ku . Kutateladze was aware of Bonilla's work, but he missed this point which Bonilla and Perry had, after all, advanced almost as an afterthought.

While Kutateladze's correlation was recognized in Russia, it does not appear to have caught Western attention until the late 1950s when Zuber saw its significance. But two more bricks were to be put into place before they did so. One was postwar work on interfacial instability, and the other was Chang's insight into its role in the boiling problem.

I have already mentioned Chang. The fairly comprehensive collection of Helmholtz instability solutions given us by Haggerty and Shea (1955) illustrates how rapidly hydrodynamic analysis had been embraced by the time Zuber arrived.

3.5 Zuber's Hydrodynamic Transitions

Zuber's dissertation was ambitious. It treated the entire problem of pool boiling. Its first two chapters dealt with nucleate boiling and bubble growth. The last four identify the hydrodynamic transitions. The titles of those chapters are:

3. "Hydrodynamic Aspects of Nucleate Boiling"
4. "Hydrodynamic Aspects of Transition Boiling"
5. "The Minimum Heat Flux Density in Transitional Boiling from a Horizontal Surface"
6. "The Critical Heat Flux in Boiling From a Horizontal Surface"

In Chapter 3, Zuber argued that a hydrodynamic transition occurs in the nucleate boiling regime -- a transition in which bubbles rising from a heater surface coalesce into vapor jets and columns. The conduits of vapor outflow formed by the jets subsequently become unstable and collapse causing burnout. This is the basis of the flooding process suggested by Bonilla and articulated more precisely by Kutateladze.

But Zuber was not content just to *correlate* burnout data; he wanted to *predict* them as well. To do that he had to predict the size and spacing of the jets. The key to doing this, he recognized, lay in understanding the dynamics of vapor movement in the transition region, and this was the subject of his fourth chapter. The q_{\max} and q_{\min} predictions for which the dissertation is best known were the subject of the last two chapters.

The sweep of Zuber's thesis was thus immense. He offered a complete body of theory of boiling -- a whole new scientific paradigm in in one stroke!

3.6 The Wake of Zuber's Proposal

No new theory of any substance is ever correct in all its details. Darwin's evolution trees had to be emended. Planck incorrectly took the energies of photons to assume a range of values. Einstein failed to account for the contributions of coupled vibrations to the specific heat of metals.

On this smaller scale of scientific change the same sort of thing is true. We can count several basic problems with Zuber's formulation. For example:

- 1.) He wrongly assumed that liquid does not contact the solid in the transition region.
- 2.) He used a 1-dimensional Taylor wave structure above a flat plate instead of a 2-dimensional one.
- 3.) He omitted the center jet that had to oscillate against the four corner jets in a Taylor wave structure.
- 4.) He used an incorrect averaging-technique to establish the frequency of escaping bubbles during film boiling.
- 5.) In several places he did not know whether to use a most-dangerous, or a critical, unstable wavelength. His reasons for choosing one or the other were unclear.
- 6.) Zuber's use of the Rayleigh wavelength in the escaping jet is probably incorrect -- at least in most cases.
- 7.) He presumed that both the jets in nucleate boiling and the bubbles in film boiling had radii equal to 1/4 of the most dangerous Taylor wavelength.

Not all these failings were evident at the time. Most of them *could* not have been evident since later experiments would be needed to show that they were failings. Consequently they were challenged piecemeal -- often by people more interested in proving the theory incorrect than in plumbing its possibilities. The literature of the early 1960's makes it clear that the initial challenge to the Hydrodynamic Theory of Boiling revolved about its failure to make provisions for any influences of heater-surface characteristics.

3.7 Initial Reactions to the Theory

The year after Zuber's dissertation Bernath (1959) captured people's attention with a paper that provided a variety of q_{\max} measurements on heaters of different sizes, heating-element thicknesses, and surface conditions. The values, of course, exhibited wide variability. Zuber really had failed to drive home the point that his q_{\max} model was only applicable to infinite, horizontal, isothermal heaters; and Bernath failed to provide means for isolating the influence of surface condition from the other variables that he investigated.

Consequently, Bernath's results focused the attention of many who distrusted Zuber's model. Chang himself was an early critic of Zuber's model. Though he had first pointed out the presence of Taylor waves in film boiling, and though he had discussed the relation of the Taylor process to film boiling (Chang, (1959)) three months before Zuber's thesis was published, he did not subsequently credit Zuber's stability arguments in relation to q_{\max} and q_{\min} .

Chang (1963) gave a set of hypotheses as to the cause of burnout, all of which involved the surface heavily in the process. Though he pointedly does not cite Zuber, the work is a clear challenge to the Hydrodynamic Theory. Even more pointed is Costello's discussion which appears with the paper. Costello also avoids referencing Zuber, but he says:

"I would like to compliment Dr. Chang on a realistic approach to the problem of burnout and also on clearly stating factors which might give use [sic.] to nonhydrodynamic effects."

Charlie Costello was the most vigorous opponent of the theory. He was a boiling experimentalist who, (we all learned in 1965) had been working under the early death sentence of terminal diabetes. His technical career was brief and intense. He was dedicated to his students, research, and charitable works. He left us a disturbing, and eventually useful, legacy of data and unanswered questions about burnout, and he plunged into the controversy with a fey kind of verve.

"A Salient Non-Hydrodynamic Effect on Pool Boiling Burnout of Small Semi-Cylindrical Heaters," (Costello and Frea (1963)) clearly trumpeted the general theme of his and Chang's challenge of the theory. It called into evidence several experiments showing surface effects on q_{\max} . His q_{\max} data for full and half cylinders of three sizes, in tap and distilled water, fresh and aged, with and without a wetting agent, showed wide variability. The problem, of course, was that here, as in Bernath's earlier work, the various influences were not sorted out systematically.

Indeed, those of Costello's results that were subsequently studied in the light of more complete statements of the hydrodynamic theory were found consistent with it. However, his criticisms, more than anyone else's, caused people (particularly in the Chemical Engineering heat transfer community) to turn away from the Zuber-Tribus formulation.

One of Costello's last works (1963) (with Adams) indicated that he was among the first to see the reason -- beyond surface influences -- that many of the previous experiments gave such variable results. Titled, "The Interrelation of Geometry, Orientation, and Acceleration in the Peak Heat Flux Problem," the work hinted at what dimensional analysis would eventually make clear -- that a proper scaling of q_{\max} would show that it depended on a parameter involving both size and gravity, for a given configuration.

The Hydrodynamic Theory of the several boiling transitions has run its course from its rocky beginnings in the 1940's, 50's, and early 60's, through widespread acceptance in the 1970's, to what we can probably characterize as a mature skepticism in the in the 1980's. At this point, two things seem fairly unassailable.

- Vapor removal during film boiling is a definite and highly predictable Taylor instability process.
- The slugs-and-columns transition in nucleate boiling and the q_{\max} transitions are determined primarily by hydrodynamic instabilities of some form.

However, a variety of nagging questions remain. We list some of them below. (This list is merely illustrative -- not exhaustive.)

The Question as to Where the Helmholtz Instability is Located. A pernicious problem with the Zuber model for burnout is that the vapor jet behavior is hard to identify in many circumstances -- in subcooled boiling, near burnout on spheres, etc. A decade ago Haramura and Katto (1983) suggested that the Helmholtz process is not located in the obvious jets and columns at all, but rather in a small structure of mini-jets near the surface, that feed the apparent jets from below. There is much to criticize in this theory. For example, they presume the mini-jets to collapse when their length reaches only one quarter of the Helmholtz wavelength.

Yet the message we learn from Zuber's work -- indeed from Kuhn's analysis of scientific progress, in general -- is that good ideas are not completely correct in their original presentations. The wise investigator asks, "What can be in it?" when he looks at a new idea. It may yet be that when better experimental techniques are developed, Helmholtz processes will be found in the micro-jet structure in subcooled boiling or in certain geometries.

The Chang-Costello Question. Many surface influences quoted by Chang and Costello were later squared with the hydrodynamic theory. Then Bui and Dhir (1985) began a line of work that involved non-hydrodynamic influences in a modified hydrodynamic theory. They showed that the influence of surface condition on q_{\max} really is magnified in some configurations. And those configurations should be a route to a better understanding of the hydrodynamics of q_{\max} .

Recently Ramilison, Sadasivan, and Lienhard (1992) went back and reviewed available burnout data for horizontal flat heaters and correlated the Kutateladze number with surface roughness and retreating contact angle. They found that those variables exerted consistent influences on q_{\max} over a range of ± 30 percent.

The q_{\min} Problem. Experiments with the onset of film-transition boiling show a clear dependence of q_{\min} on contact angle. It is also quite clear that film boiling is sure to occur when film-transition boiling becomes spinodal-limited. Is it therefore still legitimate to speak of a hydrodynamically determined q_{\min} ?

Ramilison's work (1987) suggests that q_{\min} can be determined hydrodynamically when the advancing contact angle is very large. However, further work is needed to identify that limit and to develop a correct nonlinear expression for the bubble frequency that can be used to predict it.

4. TWO TRAPS

I have tried to avoid arguing my own technical interests here. As I look back at 37 years of work in boiling, I can see that I have fallen into the same two traps that I have been pointing out. The trick at this stage is not to convince you that my views of boiling have been correct, but to find means by which we may more effectively put interests of that kind aside. To do that, we need to consider the traps.

4.1 The Paradigm Trap

Kuhn should have put us all on alert back 1962. I have tried to review some of the ways in which we have missed his warning and been slow to see.

Change comes from outside and it is invisible to the existing community. The metallurgists, Pilling and Lynch, produced boiling curves fifteen years before Nukiyama did. We never have acknowledged their work. A very young Carlos Bonilla offered the first Hydrodynamic Theory and was soon beaten into line by the American Chemical Engineering community. The Hydrodynamic Theory then had to be brought to America from Russia, China, and by a Yugoslavian student who would *not* be beaten into line.

Once the paradigm shift occurred, the Hydrodynamic Theory became the new *normal science* closing out opposing views just as strongly as the Chemical Engineers of the 1940s and 1950s once had. Now many of its tenets are under assault by Japanese investigators, and by Americans who have adopted their ideas about instabilities in a surface layer.

The realization that the so-called *uniform surface temperature experiments*, achieved by condensing vapor beneath the boiling surface, are not what they appear to be dawned on us in the mid-to-late 1960s.

We have credited European academic heat transfer experts for making this discovery. We have ignored the first identification of this effect by an American consulting engineer (Adiutori) who published his work in *Nucleonics*, and we continue to deny him credit.

History, Kuhn tells us, says this is how scientific change is bound to work. We create defensive fortifications for our ideas, and close our ears to the rest of the story. I would like to think we are capable of something more noble than brute Darwinian competition among ideas. We should be asking whether or not we would get there quicker if we could damp out the ego-involvement that so shapes this process.

The answer might be that there is no alternative. In the end, Kuhn reminds us that,

In the development of any science, the first [agreed-upon view] is usually felt to account quite successfully for most of the observations . . . Further development calls for . . . elaborate equipment, . . . an esoteric vocabulary and skills, and a refinement of concepts . . . That professionalization leads, . . . to an immense restriction of the scientist's vision and to consider able resistance to . . . change. The scientist has become increasingly rigid.

On the other hand, within those areas to which [the group directs attention] normal science leads to a detail of information and to a precision of the observation-theory match that could be achieved in no other way.

It is clear that the paradigm trap is not without its compensations, even as poorly as it appears to have served us thus far.

We see what we expect to see. The notion that nucleate and film boiling are connected with a single transition boiling regime was tenuously suggested by Nukiyama. The community closed in on this view and for years afterward experimentalists thought they had seen such behavior, even when they had not.

Costello looked for the cause of burnout at the surface of the heater and he found it there. I looked for the cause of burnout in a structure of jets away from the heater and I found it there. I found reasons why Costello's data fit the Zuber model. Now Japanese investigators look for hydrodynamic instabilities in a thin layer of liquid along the surface, and they find it there.

I have studied too much history to enjoy the competition that flows from this process. At this point I have every confidence in the capacity of the smartest and most honest investigators for being wrong. I have reached the point where I just wish I knew which instability *really* causes burnout.

4.2 The Platonist/Aristotelian Trap

The second trap arises when we turn only one set of eyes upon our problems. We have seen how we have absolutely severed ourselves from all we had learned during centuries of alchemical work with boiling and condensing.

When Vaughn calls droplets an "Extract of Teares" -- when he describes boiling in the words, "Dissolve it with the fire, that something may aspire, And grow up . . ." -- we tune him out. When Dürer surrounds St. Jerome with the alchemical tools of learning, we forget that we are watching an important scientist as well as an important artist at work.

And when Chang told me that he had dreamt of bubbles but not seen them, I, in my youth, overlooked that fact that his dream had already set in motion the largest revolution our field has seen.

If you look back over the past fifty years of work in boiling, you will see that almost all of us have expected the external world to supply the answers to our questions. We trust our photographs, our temperature measurements, and our computer output. We have been overwhelmingly Aristotelian in our search for knowledge.

Our one-sided Aristotelianism takes another form which, I believe, is terribly damaging. Our review papers are remarkable in their failure to superpose our own thoughts on other people's work. Our review papers often report contradictory claims without noting the conflicts. It is as though observing is sufficient, and subjective reprocessing will soil us.

Our history shows us, again and again, that the revolutionary advances of our field have been driven by the occasional Platonic inner eye -- by the creative demon that we too often fear to engage. All our revolutionary ideas have come from people who dream beyond anything they see in an external world -- by people who have sometimes made large errors, but who have nevertheless trusted their minds.

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NOMENCLATURE

g gravitational acceleration

h_{fg} latent heat of vaporization

Ku Kutateladze Number, defined in equation (4)

L any characteristic dimension of a system

$q, q_{max}, q_{max,2}, q_{min}$ heat flux; peak or "burnout" q , Zuber's value of q_{max} , the minimum heat flux

T, T_{sat}, T_w temperature, temperature of saturated liquid, temperature of the wall of a heater

v_l, v_g the "superficial" or average velocities of liquid approaching (or vapor leaving) a heater

$\Delta T_w = T_w - T_{sat}$

ρ density

R_b thermal resistance of the boiling process alone

ΣR_t sum of the resistances to the flow of heat in a boiling experiment cooled by a condensing liquid

σ surface tension

General Subscripts

f,g denoting the saturated liquid and vapor states, except as they represent superficial velocities