COMPACT HEAT EXCHANGERS

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A Festschrift for A. L. London

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COMPACT HEAT EXCHANGERS

PREFACE

Compact heat exchangers employ surface geometries that have high heat transfer surface area per unit volume, loosely defined over 700 m^2/m^3 (200 ft^2/ft^3). Plate-fin and tube-fin exchangers and compact regenerators are generally compact exchangers, although a tubular exchanger with a tube diameter less than 5 mm would be classified as a compact exchanger. Substantial cost, weight and volume savings are achievable with compact exchangers when they can replace shell-and-tube or plate heat exchangers.

Compact heat exchangers played a major historical technological role in the development of light weight, minimum volume, highly efficient exchangers for aerospace, vehicular, and marine transportation systems. Serious research and development efforts started just after World War I and accelerated with the introduction of aluminum brazing after World War II. Today compact exchangers continue to play a dominant role in cryogenics, air-conditioning and refrigeration, waste heat recovery (from hot gases), and in high tech applications. Since the energy crisis of the early 1970s, increasing use has been made of compact heat exchangers in many energy conversion, conservation and recovery systems. Now they are also being utilized for offshore applications and process industry special applications.

From the early days of heat exchanger development, it was realized that successful implementation and operation of a compact exchanger required an operationally convenient design procedure, in addition to heat transfer and flow friction design data for surfaces. The fin efficiency concept was introduced by Harper and Brown (1922) for extended surfaces. Gardner (1945) considered many fin geometries. The basic heat exchanger design was done by the log-mean temperature difference (LMTD) method before 1940. The effectiveness—number of heat transfer units (ϵ-N_{tu}) design methodology was introduced by London and Seban in 1941 as an operationally more convenient alternative to the LMTD method.

The following is a brief historical background provided by Professor London on the ϵ-N_{tu} method.

> My experience with the ϵ-N_{tu} approach started with my M.S. Thesis on cooling towers (U.C. Berkeley, Summer 1938) which resulted in a paper with Professors Mason and Boelter, presented at an ASME meeting in San Francisco during the summer of 1939, London et al. (1940). The ideas stemmed quite directly from a study suggested by Professor Boelter, of the *Principles of Chemical Engineering* by Walker et al. (1937), in their treatment of water coolers and adiabatic humidifiers. Also, one of their proposed design methods for packed towers used the concept of H.T.U., the "height of a transfer unit" as equal to the height of a column packing divided by the number of transfer units. I was at Stanford by this time and Professor Seban was at Santa Clara University. We had many discussions on the subject of exchanger design, and this joint effort led to the preparation of an unpublished paper, entitled *A Generalization of the Methods of Heat*

Exchanger Analysis in 1942. Most of the concepts presented in this paper had been available in the literature at that time. However, these ideas were not commonly employed in either industry or teaching. We believed that the generalization and consolidation of these concepts (the ϵ-N_{tu} method) would encourage their use.

This 1941–42 vintage paper was finally published in 1980. It should be noted that Professor London had originally symbolized the number of transfer units as NTU, but changed to N_{tu} following recommendations by Professor Max Jakob and Mr. Hosmer Norris. Nevertheless, in the present literature, NTU is most commonly used.

The first serious attempt to obtain heat transfer and flow friction design data for compact exchanger surfaces was started at the U.S. Navy Bureau of Ships in 1944 for a gas turbine recuperator/regenerator. This effort was then continued at Stanford University by Professor London and his students for the next 24 years until 1971. Among the many publications that derived from this effort, the most notable is the monograph *Compact Heat Exchangers* by Professors Kays and London which is now in the third edition and has design data for over 100 compact surfaces. It is still considered the authoritative source in the field and has been translated in seven languages.

The outstanding contributions by Professor London are the introduction of the ϵ-N_{tu} design theory and a wealth of compact exchanger surface design data obtained over a life-long research effort. In his honor, professionals and experts in the field, and his students, have contributed to this festschrift to bring forth the latest developments in compact heat exchangers. As this monograph reflects, the design theory and applications of compact heat exchangers have grown significantly in the last 50 years. The major areas of compact heat exchangers covered in this festschrift are: Basic ϵ-N_{tu} Analysis for Complicated Flow Arrangements; Analysis of Complex Extended Surfaces; Single-Phase Heat Transfer and Pressure Drop Measurements, Correlations, and Predictions; Two-Phase Flow in Compact Heat Exchangers; Irreversibility in Heat Exchangers; Operating, Manufacturing and Transient Problems, and Applications of Compact Heat Exchangers. The breadth and depth of the coverage in this festschrift indicates how much the subject of compact heat exchangers has grown in recent years.

This festschrift is an outcome of papers presented at the A. L. London Symposium at Stanford University during March 23–24, 1989. Most of the contributors to this monograph are international experts in the field. It is their dedicated efforts to provide high quality papers in a very short time that made it possible to publish this festschrift, a unique contribution to the literature, in a timely manner. We gratefully acknowledge their fine contribution.

We are also thankful to Professors W. M. Kays and R. J. Moffat for their excellent organization of all professional and social activities conducted at the A. L. London Symposium at Stanford University.

We greatly appreciate the superb and careful typesetting of this festschrift by Jim and Pat Allen of ALLEN CompuType. We are grateful to Ms. Florence Padgett and Mr. William Begell of Hemisphere Publishing Corporation for their encouragement and support throughout the preparation of this festschrift. Finally, our heartfelt thanks

to Professor London for his continued keen interest in the subject and for inspiring the new generation to continue and accelerate research efforts in this important engineering field.

The editors, each of whom have claimed Professor A. L. London as their inspiration have been privileged to have been a part of this festschrift endeavor. Each of them has made a significant contribution to the success of the festschrift and the order of editorship was established by a coin flip.

R. K. Shah

A. D. Kraus

D. E. Metzger

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FIFTY YEARS WITH LOU LONDON— A PERSONAL REMINISCENCE

by W. M. Kays
Stanford University

Lou London and I both arrived on the Stanford campus in September of 1938, he a new instructor in Mechanical Engineering, and I a very naive and innocent freshman with a vague notion that I wanted to be a mechanical engineer. Lou was moving from the University of Santa Clara, a few miles away, where he had also been an instructor for a year or so. Interestingly, his place at Santa Clara was taken by Ralph Seban who a couple of years later also came on to Stanford.

It was not until about the spring of 1940 that I became aware of Lou's existence. There was an annual ME-CE softball game and picnic out at Searsville Lake. A short, stocky member of the ME team caused a great stir among the students whenever he came to bat—hoots, catcalls, and all the rest. It was Lou, and he was obviously somebody they badly wanted to get "out." Some of them kept shouting "Alexander is a Swoose," which apparently referred to a particularly inane popular song of the time. I wondered what all this commotion was about. I was soon to find out.

The next year I took the first thermodynamics course (not from Lou) which I found fairly simple, and then took a laboratory course in which Lou was one of the three instructors. On my first report I got a 90, and on the second a 95, both from one of the other instructors. Then we had an experiment on a steam engine with Lou as the instructor. I was shattered to find a grade of 78 on the report. But what was more disconcerting was that he graded my report using red ink, and in the middle of the Discussion he wrote "complete nonsense." I began to understand the London legend, but the fact that I would 48 years later be standing here talking about Lou would have been an impossible thought.

The following autumn I took the second thermo course, this time from Lou. Again, the first problem set was all scratched up with red ink, and the remarks suggested that I wasn't cut out to be an engineer at all. Lou was all concerned with something called "irreversibility"; I had never even heard the term before. Well that was the low point; somehow I managed to pull myself together and respond to the challenge, and I think I actually ended up with an A in the course. And like it has been for so many others, this was undoubtedly one of the major turning points of my life. I had finally found something at which I could be successful.

The remainder of my senior year was a joy as I gobbled up everything Lou had to offer. By spring the war was on and Lou wanted me to take a job at Douglas Aircraft following graduation. I still remember his description of the job. The oil coolers on aircraft engines were using up 15 percent of the power, and something had to be done about it. Could we have been talking about "compact heat exchangers"? Well, that was to come later because I was commissioned in the Army from the ROTC and left to get involved in a shooting war. I remember coming around to Lou's office in my uniform to say goodbye. He told me to check in with him after the war and he thought he could find me a graduate fellowship.

After the war I did come back, but I didn't need the fellowship because Uncle Sam had given everybody the GI Bill. I came in and talked to Lydik Jacobsen, the then department head. It was the spring of 1946 and Lydik told me that they didn't yet have much to offer because "our theoretical man" Louis London was not going to return until September. Lou had gone into the Navy and was on duty at the Bureau of Ships in Washington where I am sure he wrote "nonsense" on not a few reports. But the important thing as far as I was concerned was that he was in fact coming back.

Lou brought back with him something very new—two research contracts sponsored by BuShips and ONR. The concept of government sponsored contract research, and use of the contacts to support graduate students, was a turning point in the history of engineering at Stanford. We were a little bit ahead of everybody else (except MIT). When I finished my Master's degree in the spring of 1947 Lou offered me a full time job as a Research Associate to run one of the contract projects, the one involving gas turbine heat exchangers. I had no idea of pursuing a PhD degree, or of going into teaching; it was just to be a two- or three-year project. But Fred Terman had different ideas; he wanted to combine work on sponsored projects with dissertation work. It is hard to realize today that this was a radical idea and at most universities at that time dissertation research was kept scrupulously separate from sponsored research. At any rate, I soon found myself on a PhD program and also taking on a regular teaching schedule. It was becoming apparent that my association with Lou was to continue much longer than I had originally thought.

My principle recollection of that period was continually writing reports for ONR, reports that were widely distributed throughout the country, and I think this series of reports was what originally put us on the map. English composition had always been one of my weak points, but here is where Lou was a teacher par excellence. I would lay out the report and write a draft, and then turn it over to Lou. A few days later he would call me in and we would sit down and go over it. Red ink everywhere! Not a single sentence was left untouched. It was agony, but it was a great learning experience, and one for which I will always be indebted to Lou.

It was easy to convert these reports into ASME papers, so we started writing and presenting papers, especially at the ASME Annual Meeting in New York. Lou always insisted that I be the first author and make the presentation, of course after practice sessions with him. It was another great learning experience, and of course I got a lot of national exposure, another debt I owe Lou. In those days engineers didn't write so many papers as today, so it was not difficult to look good.

I remember particularly my first paper which I presented at the Heat Transfer and Fluid Mechanics Institute in Berkeley in 1949. I was very proud of it, but the reviewer initially rejected it and said the whole thing was absurd. Lou saw the rejection before I did, and without telling me, he went to see the reviewer (fortunately in the Bay Area) and fought it out with him. The reviewer proved to be wrong, but had I first had to face that rejection and the written review I think I would have been so devastated I would have given up the whole academic enterprise.

Lou London was an immensely energetic person. Most of his energy was put into his teaching and research, but he could also pursue non-academic interests with exhausting zeal. I am reminded of a trip I took with him in June of 1948. Lou had purchased a new Packard, his pride and joy. This was the "inverted bathtub" model that apparently led to the demise of the Packard motor company, but never mind, Lou loved to drive it. The first meeting of the Heat Transfer and Fluid Mechanics Institute was being held in Los Angeles and Lou offered to drive several of us down to LA. We got started about 9:00 o'clock on a Sunday morning and after a few miles Lou suggested that we take a detour to see Point Lobos. At that point he suggested that we take the San Simeon Highway down the coast. By noon we were only at Big Sur, so we stopped for lunch. Most of the afternoon was spent on the long winding road along the coast, although Lou was insisting that we should have dinner at a place called the El Paseo in Santa Barbara. We pulled into San Luis Obispo about 5:00 with everybody hungry, so we stopped for an hour to have something to eat. We finally arrived in Santa Barbara between 9:00 and 10:00, but there was nothing for it but to go to the El Paseo. At 2:00 o'clock Monday morning we got to Cal Tech where we were signed up to stay in a dormitory. Of course this involved finding and waking up a student caretaker who couldn't imagine anybody coming in at that time. At 6:00 we were up again because the first day's meeting was at UCLA and we had a long ride through the commuter traffic of LA.

During the meeting Lou met an old friend from the East and invited him to ride back with us at the end of the three-day meeting, although that now meant six in the car. We were relieved to find that Lou was driving back via Highway 101 and we made pretty good time for most of the trip. But somewhere south of King City Lou asked his new passenger if he would't like to see Point Lobos. Out came a map and Lou found some kind of a road that went over the mountains and entered Carmel Valley the back way. The next two hours were spent on every description of dirt road through the mountains, but we did get to Point Lobos. We reached Palo Alto about 9:00 pm, Lou smiling as always and fresh as a daisy.

Virtually everything I know about teaching came from Lou. In the summer of 1947, just as I was settling in designing the compact heat exchanger test system, Lou announced to me one day that I was going to have to teach M.E. 132, the beginning thermodynamics course, starting in about two days. [For the first few years after the war we operated all year round with a full schedule in the summer.] I didn't have a clue about how to teach a lecture course, and it having been five years since I had taken a thermo course my thermo was a little shaky too. But I needn't have worried. Lou loaned me his notes and all I had to do was teach his course and stay a few days ahead of the class. The notes were absolutely complete and totally intelligible. My teaching style and methods were almost completely established that summer and have served me well for over 40 years. But in reality they were Lou London's style and methods. Most of the grade was on the problems, and you didn't do a problem without a statement of the problem and a discussion at the end. Lou's problems were no mere exercises, they were complete analyses of a significant engineering application. And that is the thing that really set Lou London apart from so many other teachers; his problems were drawn from real life situations, and his great curiosity and energy provided him with endless examples from which to choose.

In the fifties we used to have arguments in faculty meetings about whether particular courses were "theoretical" or "practical," the presumption by some faculty members being that "theoretical" was a perjorative term. Lou would snort and cry out that theory was as practical as pudding pie. But that is not to say that Lou was primarily a theoretician; he was first and foremost an engineer. If a theoretical approach to an engineering problem was the most efficient, then that to him was the most practical approach.

One cannot talk about Lou London without mentioning the NTU. He has always claimed that he didn't invent the NTU, but like it or not the NTU will always be associated with his name. I recall going on a field trip with Lou and we saw a heat exchanger incorporated into some kind of system. Lou looked at it for a couple of minutes and then told me that its effectiveness was probably about 65 percent. He had done some quick mental arithmetic, and with his "feel" for the NTU he could easily estimate performance. That's one of the great virtues of the NTU concept. You can develop a feel for heat exchanger performance. It's not the kind of thing we usually teach in the classroom, but it's very handy in the field.

My favorite NTU story concerns an ASME meeting session one time when NTU = AU/C appeared on the screen. Max Jacob was in the audience, and he walked up to the equation and said "I don't understand why you can't cancel this U on the left side with that U on the right."

The story of life with Lou London would not be complete without discussing one of his most dominating characteristics—his amazing integrity, both personal and professional. Surely this must have something to do with the rigors of a childhood on a farm in East Africa, but I am not qualified to figure out such things. I do know that Lou lived and worked according to sets of principles, and nobody could ever persuade him to compromise or violate those principles. He could get terribly upset with students who attempted to violate the 1st or 2nd Laws of Thermodynamics, but he was equally devoted to principles of behavior. One result of this was that Lou never sought an administrative position in the University. I think the reason for this was that there was no way he was going to compromise his principles to please Fred Terman, or anybody else. And there was no way Lou was going to involve himself in the inevitable politics of an institution with all its deviousness and compromises.

It has been an honor and a great privilege to have been so closely associated for over 50 years with Lou London. I think few people have had my kind of good luck.

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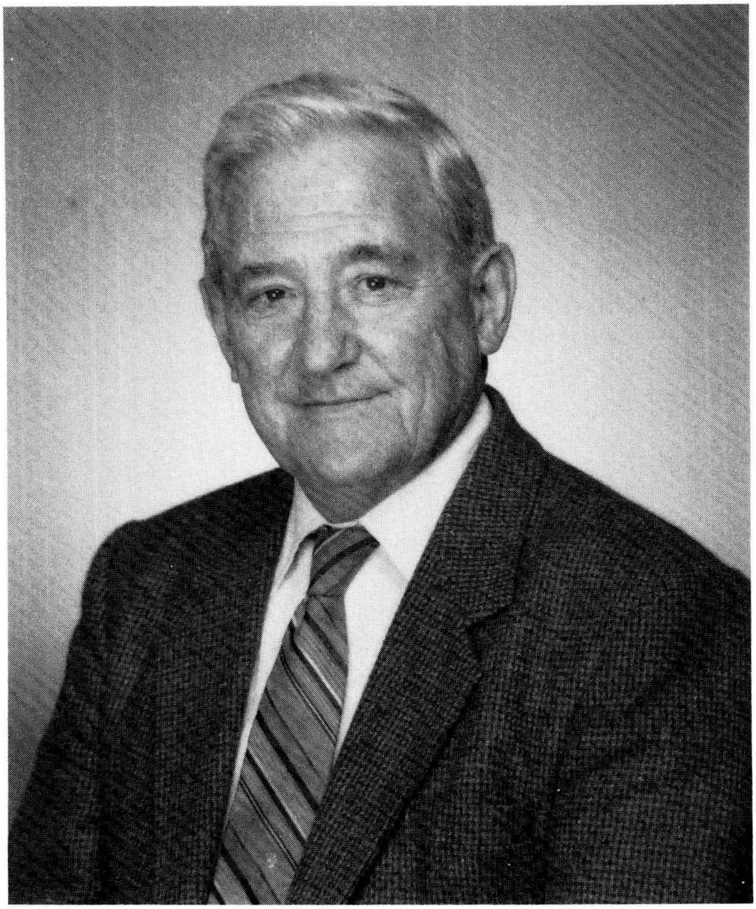
PROFESSOR ALEXANDER LOUIS LONDON—ON HIS 75TH BIRTHDAY

by R. K. Shah, R. J. Moffat, and A. D. Kraus



PROFESSOR ALEXANDER LOUIS LONDON

The 75th birthday of Professor A. Louis London of the Department of Mechanical Engineering at Stanford University, Stanford, California was on August 31, 1988.

Professor London was born in Nairobi, Kenya. His Lithuanian father and German mother brought the family (four sisters and a brother) to the U.S.A. in 1921. His father, after 25 years of ostrich raising and coffee exporting, decided to retire at the age of 45 and raise the family in the country to which he immigrated in 1876. Professor London received his primary and secondary education in the Oakland, California grade schools and Oakland Technical High School, graduating in 1931 and entering the University of California shortly thereafter.

After receiving his B.S. at the University of California - Berkeley in 1935, he worked for a year at the Standard Oil Company of California and then taught for a year at Santa Clara University. In 1938, he received his M.S. at the University of California - Berkeley prior to coming to Stanford.

Professor London has been at Stanford since 1938, except for three years during the Second World War, when he was at the Bureau of Ships, Washington, DC, working on new developments in marine propulsion machinery and power auxiliaries. He was Director of a 24 year program (1947-1971) sponsored by the Office of Naval Research, concerned with many heat transfer and thermodynamic investigations. He taught graduate and undergraduate courses in heat transfer, thermodynamics, and propulsion systems at Stanford and a course on compact heat exchangers at various ASME conferences.

During leaves of absence from Stanford, he performed research in nuclear reactor engineering at the Argonne National Laboratories and, in vehicular gas turbine development at General Motors Research Laboratories. He has been active in the ASME Heat Transfer and Gas Turbine Divisions, and was chairman of the Gas Turbine Division in 1966-1967.

Professor London demanded careful attention to detail from his students. Each analysis had to start from basic principles. Each step had to be scrutinized. His training provided insight into engineering analysis and developed a clear problem solving methodology. His insistence on rigor and completeness was backed up by a limitless supply of red ink! He was known as a 'tough' teacher, but even those who complained about the work appreciated the careful thought that Professor London put into the grading of each problem set. A course from Professor London was an unforgettable experience. The influence of his teaching can be found clearly in the textbooks written by some of his students. These include *Convective Heat and Mass Transfer* by W. M. Kays, *Thermodynamics* by W. C. Reynolds, *Engineering Thermodynamics* by W. C. Reynolds and H. C. Perkins, and *Conduction Heat Transfer* by G. E. Myers.

In choosing graduate students for research, he urged that they complete an Engineer degree first (1-2 years beyond M.S.) before deciding on further work leading to a Ph.D. degree. Many of his students are now well known in the heat transfer field. The following is a list of his Ph.D. and Engineer students: