Structural Stability
Research Council

# ANNUAL TECHNICAL SESSION

## Proceedings

1986

STRUCTURAL STABILITY RESEARCH COUNCIL

# PROCEEDINGS

# 1986 ANNUAL TECHNICAL SESSION

## "STABILITY OF PLATE STRUCTURES"

**ORGANIZED BY:**

*STRUCTURAL STABILITY RESEARCH COUNCIL*

**CONFERENCE SPONSORED BY:**

*FEDERAL HIGHWAY ADMINISTRATION*

*AMERICAN IRON AND STEEL INSTITUTE*

April 15-16, 1986
Washington, D.C.

STRUCTURAL STABILITY RESEARCH COUNCIL

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# FOREWORD

It is inevitable that, following four hectic days at our Annual Technical Session in Washington, D.C., I procrastinate as long as possible before writing this Foreword. However, that is not necessarily detrimental. On the contrary, as I look back several months, I believe that I see our activities and deliberations in truer perspective than I did immediately following the session.

The Executive Committee frequently asks, "What is the formula for a successful Annual Technical Session? How is success to be measured?" The criteria include the quality and relevance of the presentations, the extent to which useful discussion was promoted, whether first time attendees were sufficiently interested to wish to participate in SSRC, the level of attendance and of course, profitability versus a loss. With each ATS we learn. But the lead time we now need is such that the Washington experience with respect to theme, sponsors and location, cannot be applied completely until 1989, since the 1987 and 1988 sessions have been agreed upon and booked. Increasingly, we members of the Executive Committee find ourselves applying what appear to be marketing strategies in planning Annual Sessions that will be viable financially. We have to select themes for which we feel there will be sponsorship and to choose locations which are both accessible and sufficiently relevant to the theme to provide local participation. By the measures of general attendance and quality of technical presentations, our 1986 ATS was most successful. However, the levels of local participation and sponsorship fell short of my expectations.

My personal preference is for short specialty conferences such as ours. I am sure that many engineers can attend the one or two day meeting which has relevance to their work while the more general four or five day conference is out of the question. It remains my opinion therefore, that our format is correct for SSRC, but we must maintain an adequate level of attendance and sponsorship to support the fixed costs of the conference.

The lesser relative importance of traditional steel building concerns such as columns, beam-columns and frame stability, continues to be evident from the more varied nature of our program. Aside from our theme session on plate structures, plate buckling was a stability criterion which appeared frequently in other presentations. This, together with seismic effects, seem to have been dominant aspects of our meeting.

Usually, the coherence of the theme session permits me the luxury of concentrating for an entire afternoon on one facet of stability. Although I may not understand it all, the consistent theme seems to have a synergistic effect. It is refreshing, of course, to have speakers from outside our usual orbit provide a different perspective to stability problems.

Completely new topics with well illustrated presentations are fascinating. Knowing nothing about ships, I could have listened, and Donald Liu could have continued, for much longer with his introduction to the structural design problems of ship hulls. The panel discussion was as it should be, presentations on applied stability problems to promote discussion and interchange of ideas.

While a luncheon speaker is not a complete innovation, a speaker whose intent is to entertain us as well as to teach is new. Mario Salvadori certainly drew on our aquaintance with structural engineering, but in a cleverly light-hearted vein, to provide more than a few laughs before we resumed serious consideration of stability. Although he was recorded, his wit will not be included in these Proceedings. Nevertheless, we all thank Dr. Salvadori for showing us how amusing we can be.

Our Annual Technical Session does not run itself by any means. Sam Errera and his sub-committee performed their unenviable task of selecting the speakers with customary skill. Our Technical Secretary, Graham Stewart, contributed significantly to the smooth running of the sessions by monitoring the speakers from their acceptance through submission of Manuscript for publication. As we have come to expect, our Administrative Secretary, Lesleigh Federinic, plans the Meeting in detail, negotiates, and at times, remonstrates with the hotel to ensure that her plans succeed. We are indebted to Dr. Sabnis and his students from Howard University for local arrangements, publicity and for operating the projector and manning the book table; many thanks for their assistance.

Financial sponsorship is essential at our ATS if it is to continue in its present form. We are indebted especially to the Federal Highway Administration, and the American Iron and Steel Institute for financial assistance for this ATS. We thank these organizations heartily, for without their aid, the unique form of our annual meeting would be altered dramatically.

Chairmen come and go, but the Director stays on forever. I do not expect this actually to come to pass, but doubtless Lynn Beedle will make his best efforts. Dr. Beedle's uncanny skill both in monitoring the meeting and knowing when to provide direction are rare qualities that we value highly.

My sincere thanks to those of you who spoke at this Meeting and to all of you who came; although it is obvious that without you there would be no meeting, too frequently you seem to be overlooked. While we have not managed to provide the Proceedings for your summer reading, it is hoped that they will be on your desks as you return refreshed and enthused to your fall regimen.

John Springfield

J. Springfield
Chairman

Toronto
August 5, 1986

# "Buckling, Buckling ...Buckled"

By

**Dr. Mario G. Salvadori**
**Chairman of the Board**
**Weidlinger Associates, New York**

Dr. Salvadori is the author of five books on Applied Mathematics and ten books on Structures in Architecture. He has had over 150 technical papers published in the U.S. and foreign journals.

Dr. Salvadori received his Doctorate in Civil Engineering and Doctorate in Pure Mathematics from the University of Rome, Italy.

He is Professor Emeritus at Columbia University and is active in the New York City School System as a teacher.

He is also a member of the National Academy of Engineering and a fellow of many professional organizations.

Dr. Salvadori has kindly consented to have his speech included in the Proceedings.

**Introductory Comments from John Springfield:**

We're extremely privileged today to have an eminent structural engineer who has been involved in the design of some of the most fascinating buildings on this continent and in Italy, Dr. Mario Salvadori, to address us, more or less, on the subject of his choice, but which will be, of course, of interest to me.

It's been a particular pleasure for me to listen to the anecdotes of Dr. Salvadori and Bruce Johnston over lunch. Bruce will introduce Dr. Salvadori -- they have known one another for many years, but perhaps I would just mention that Dr. Salvadori wrote a book called "Structure in Architecture" which explains the various structural forms that can be used without any mathematics and, from time to time, if I have a building of a very unusual shape and I'm searching around for possible solutions that I have missed, I invariably refer to this book and thumb through the pages -- I may have forgotten how a saddle works, or something of that kind -- and I commend that to all of you.

So I now call upon Bruce Johnston to introduce our speaker.

**Bruce Johnston:**

Although I've known Mario for possibly close to fifty years, my most vivid recollection occurred a little over 40 years ago. For some reason or other, which we can't remember, we were both walking on the street in lower Manhattan when the news came out in the "Extra" papers: the bomb had been dropped on Hiroshima. And Mario turned to me, and he said: "This is insane." He, like Einstein, immediately saw the fact that we had opened Pandora's Box. Since then, I have seen Mario off and on, and he has written five books on mathematics. These are specially interesting because they are probably the most understandable books on mathematics that have ever been written. And they have been very useful to me in the past years. He has also written ten books (two have been printed recently) related to architecture, and as John just said these books are noteworthy, because they are devoid of mathematics.

Mario has a doctor's degree in pure mathematics from the University of Rome, and also a degree in civil engineering from the University of Rome. The rest of his accomplishments, including his membership in the National Academy of Engineering, are listed on the program for the meeting you all have.

So, without any further hold-up, Mario Salvadori.

**Mario Salvadori:**

The reasons my books do not contain much mathematics is because, having a Ph.D in mathematics, no-one can tell me: "You don't know math, that's why you don't use it." It was very wise on my part to do that. When my good friend, Gerry Fox, asked me to address you today, I had misgivings. In the first place, I told him, I haven't contributed to the field of instability for the last 25 or maybe 30 years, and secondly, I thought of the various luncheon speakers I've heard and tried to see to which categories they belong. And I found that there was a category of the people invited to be speakers at luncheons because they were not good enough to present a technical paper -- that's me. So I felt kind of discouraged. Then I remembered a number of speakers who usually have taken courses in public speaking, and they make you happy in the beginning by saying: As I was coming to this meeting, I saw a funeral, and it just reminded me of the joke of the consulting engineer, who died and had been so honest that he went straight up to heaven, and was met by St. Peter, and St. Peter said, "Welcome, we are greatly honored - you are the first consulting engineer accepted into heaven at the age of 130." The engineer said, "Who, me? I died at 62!" And St. Peter said, "I didn't look at your death certificate, I just got all of your time cards from your clients!" I like to tell jokes, but I think that this is a very serious meeting, so I can't tell jokes!

Finally, Gerry came up with a very, very convincing idea. He said (very diplomatically, of course): "You were in at the very beginning of the knowledge of instability in the United States, so you don't have to give a technical talk, just tell us what you remember of the old times". In other words, he was telling me I belong to the third category of people, who are so old, that they are invited because of their age. But finally, and privately, I thought that since we have a son in Washington, who amazingly enough, loves to see his parents, we would see him, (that was a good incentive), and having a free lunch in this time of inflation is not to be dispised (and this was a very good luncheon). So I decided to say, "Yes", and that's why I am here. Now the luncheon was so good that I think you should go to sleep so I'll talk in a very low tone of voice not to disturb you and don't be ashamed if you just fall asleep.

I don't know how many of you do know that there is a very famous Italian playwright, Pirandello, who got the only well-deserved Nobel Prize in literature among the Italians, and he is most famous for one of his plays, called "Six Characters in Search of an Author". In thinking about what happened to Euler in 1744, I became convinced that here was a mathematician and physicist who "had a solution in search of a topic". Because with the kind of materials at that time used in construction, certainly nothing buckled. And instead of being very honest, and saying, as he should have said: "I have found a solution to an eigen-value problem of a second-order differential equation with boundary conditions..." (and he was wrong, because the equations were of the fourth order, not of the second order), he wrote the paper (as we all do) upside-down saying: "I have solved the problem of the buckling of a compressed, straight member." Well, this happened in 1744 -- we are now in 1986, so it was 242 years ago, because by the rules of the algebra of real numbers, if you subtract from 1744 from 1986, you get 242 whether the numbers represent apples, atom bombs, or years. On second thought, I stand corrected because when you count atom bombs, the operation of substraction is forbidden. In nuclear algebra, you may add to the stock pile, but you never substract. So, let's forget the bombs. For all other cases, the rules of algebra hold. And the interesting thing about his solution, of course, is that at that time the modulus of elasticity was more or less known, but some of the basic concepts of structures were in their infancy at best. For example, the concept of moment of inertia had not been crystallized yet. And here comes Euler who writes, in modern symbols, that the buckling load of a bar is $\Pi^2 EI/L^2$. Now I think that, in a sense, he was also lucky because eventually people found the problem for his solution, but also because his formula, if you think about it, is very sexy. First of all, it does have $\Pi^2$. You know, $\Pi$ is a strange number to come up in buckling: when I taught instability to undergraduates, the question that came up all the time was: "All right, you tell me that you push on the bar, and the bar is going to buckle, but will it buckle this way, or that way?" And I said, "Well, that is a purely probablistic concept and we cannot predict which way the column will buckle." (In 1940, my colleague at the University of Rome, Professor Carlo Tagliacozzo, obtained the buckling load of a bar by purely probablistic methods and, sure enough, he found it to be $\Pi^2 EI/L^2$.) In addition, the formula is very simple. Our students have tremendous memories: for example, they remember the formula for the solution of a quadratic equation, which is a nightmare: $(-b \pm \sqrt{b^2 - 4ac})/2a$, but to remember $\Pi^2 EI/L^2$ is easy. It is so easy that to the undergraduate students, whether the bar is elastic, inelastic, or elasto-plastic, whether it has simple supports or other kinds of boundary conditions, when you ask "What is its buckling load?", they answer immediately, $\Pi^2 EI/L^2$.

Having talked of the good luck of Euler, I must now say that he thought, and very many of us thought, that the buckling problem had been solved. Not so! In fact, this solution only applies to an idealized perfectly elastic and perfectly symmetrical bar. But you know that in every faith there are people who are ready to believe, but want to be shown. So, just as St. Thomas was not convinced until he touched the body of Christ, some of you engineers, and even we theoreticians, who are scared stiff of you, said: "$\Pi^2 EI/L^2$? Well, let's see." And you started performing experiments. And the moment you began performing experiments, it wasn't true at all that the buckling load was $\Pi^2 EI/L^2$. Well, we engineers get over that easily because we always use coefficients of ignorance, which of course we sell as coefficients of safety to the outsiders, and with that, we said, "All right, if that is the load, we'll take a factor of safety of two and a half or three, while usually in structures the factors of safety are much smaller and we will still use $\Pi^2 EI/L^2$."

But there came a point where the problems to be solved became a little more complicated: there was buckling of frames, of plates, of shells. Of course, it was the glory of our good friend, Timoshenko, to invent the energy method, and the energy method allowed good engineering solutions with any approximations you could think of, for any practical problem you could think of without too much work. I remember that when I published my first paper on the application of finite differences to buckling problems, I gave a talk to the alumni at Columbia entitled "How to Get Something for Almost Nothing", because you could compute with it almost any buckling load and it cost you almost nothing at a time when we weren't using computers yet. Now with computers, finite difference, and finite elements, you can say that all of the practical problems of elastic instability can be solved. Well, the statement has to be qualified: not all, because if you remember, the first edition of Timoshenko's Theory of Elastic Stability ends with the formula for the buckling of a perfect thin sphere acted upon by normal pressure. That formula is also very sexy because it is simple, it has a coefficient $1/\sqrt{3(1-\nu^2)}$, easy to remember and that it has in it constants which define an elastic material (the elastic modulus, and

Poisson's ratio), and geometrically, two parameters: the thickness of the shell, h, which is supposed to be very small, and the radius of a shell, a. Well, again, this is where the Timoshenko treatise ended, and I remember that when I first taught the course on elastic stability to graduate students, that was the end of the world of buckling. Once you had reached that formula, that was all that you could teach at the time. But again, you experimentalists gave us trouble because you kept experimenting and you experimented on curved surfaces and particularly on thin shells -- and, boy, was there trouble! To begin with, the coefficient in front of the Eh/a, that is $1/\sqrt{3(1-\nu^2)}$, which is about .577, and the experiments did not give that at all. Now if they had given a higher load, engineers would not have cared at all, because they like to play it safe, so they would have said: "Fine, we'll use a smaller coefficient of safety and we are in business."

The trouble is that the results obtained in the experiments were below those of the formula. And here I must tell you of two stories. First, is that, as you well know, Engesser said, "Look, the mistake is that we are considering elastic stability and most bars buckle after the elastic limit is reached." He introduced the tangent modulus and everybody told him: "You're absolutely wet. This is not the way it works." Even the great von Karman agreed and came up with his reduced modulus. But then, a few years later, Engesser was proved to be right, and this produced among engineers a very uneasy feeling: the Euler formula was so simple! You just compute the buckling load, you put in a coefficient of safety, and that is it. But the moment you introduce the tangent modulus, in order to know at what point of the stress-strain curve you are, that is to determine the slope representing the tangent modulus, you've got to know what the load is. So if you don't know the load, you cannot determine $E_t$ and if you don't know $E_t$ you cannot determine the load. So, you have to use one of those methods of successive approximations, or cyclical computations that engineers don't like very much. Engineers like to open a book, particularly a handbook, that says: "This is the formula: apply it." They don't know what it means, but they turn the crank, get the answer, and that's it. I'm not talking of engineers like you, of course. This learned group is far above such a low level of engineering performance!

The second story I want to tell you is that I was then particularly interested in thin shells because I designed thin shells. I was annoyed by the fact that the experimental results were much lower than those of Timoshenko's formula, and was not very elated either when, first, Theodore von Karman alone, reduced that coefficient to about .3, and then with the help of his student, Tsien, brought it down to about .27. I am a very bad engineer, but by shear luck, I have studied ten languages, and I speak five, so I can read the literature from all over the place. And one day, I read in the literature that my good friend, Eduardo Torroja, the Spanish structural engineer, was designing a dome, a shallow spherical dome, for a church in Switzerland and since he had doubts about the value of the buckling capacity of that shell, he made a model of it, about 50 ft. across and hung pails from it. He filled them with water, and there came a point when all of a sudden, the shell snapped through, and he found that the coefficient in front of the basic formula was not .577, it was .06, which is about 1/10 of the theoretical result. I had also read an article by Professor Csonka, the famous Hungarian structural engineer and professor, who had done a very interesting, unwilling experiment. Csonka had designed a dome, not in the shape of a sphere, but in the shape of a square elliptic paraboloid, that looks very much like a sector of a sphere, and this concrete dome had stood up for 3 years under the regular snow they get in Budapest, but the fourth year there was a tremendous snow fall and, when the snow melted, Csonka found that the surface of the paraboloid was wavy. In other words, it had buckled locally, all over. I was in touch with him (in writing), and I was fascinated by the remark of Csonka, who instead of saying: "Oh, my God, it buckled!", said, "Under the dead load and a regular snow load, it did not buckle. Under the dead load and one more foot of snow, it buckled, so the buckling load is in between the two." So, I took a piece of paper and a pencil, and figured it out, and sure enough the coefficient came out to be .06. At that time, at Weidlinger Associates, which I had joined in 1954, and in a sense it was a big mistake because I lost all my vacations and my enjoyment of going to Italy, but in another it was fascinating. I was in charge of designing a containment sphere in steel, for a nuclear reactor. And I must confess to you that, knowing that in theory the coefficient was at least .27 and the experiments said .06, and since I knew that experiments are never wrong, but theory can be wrong quite often , I didn't know what criteria to apply to the design of this nuclear reactor. Having been an involuntary member of the "Manhattan Project", I happened to know a bit more about bombs than most people, so I was scared. Until I invited Torroja to lecture at Princeton, where meanwhile for some mysterious reason, I had been appointed Professor of Architecture although I'm not an architect, and Torroja addressed my students, and he mentioned his experiments on thin shells. I said to him: "Well, of course, you know, your experiments prove that the coefficient in front of the buckling formula for spheres is .06 instead of .27." He said, "No, I didn't know that." "And you probably know that Csonka has done this involuntary experiment and he also got .06" -- and he didn't know that either. So finally, by putting the three of us together, we became convinced that the coefficient should be .06. The moral of this story is simple: to find the truth, it takes an Italian, teaching in America, working with a Spaniard and a Hungarian.

If all these difficulties were of a purely theoretical nature, I don't think we should worry so much, and in fact, I don't think your association would exist. But they are involved in practice too, and there is the rub. Over the last many years, I had a number of cases where I acted as an expert in court: in 1949, in my first case, when I proved that a gentleman did not fall, but had jumped from the 17th story of a building, and I won the case. I have been in court a number of times since and I find that of all catastrophic failures (this figure must be taken with a grain of salt), I think that probably about 40% are due to a lack of knowledge or understanding of the soil. I had one case in Florida where a very reputable soil mechanics engineering firm had taken borings and decided to suggest pile foundations 90 ft. deep. Then a year went by and when they began putting the piles in, the first pile disappeared! It so happens that in Florida, as most of you know, the soil near the shore, a damnable soil, consists of a formation of plants and some kind of conglomerate that keeps them together. This layer is about 10 ft. deep, so if you don't go through that, you think you have a fairly good soil. But if you go through it, then the rock is about 180 ft. deep. So to their dismay, these people had to build bearing piles 180 ft. long. The other cause of disasters, of course, is materials, but I would say that a good 30 - 40% of the cases I have been involved with were due to buckling. I would just mention in passing, the roof of the skating rink in Hartford, Connecticut which was a space frame about 250 by 300 ft. on four pylons, which stood up for a number of years and, luckily, one night at four in the morning, its 1,500 tons of steel crashed to the floor in 5 seconds. Somebody who was awake and looking out of the window at a nearby hotel saw it happen. I went there the day after and I, who thought I knew what Dante's Inferno looked like, believed that was Dante's Inferno. When I looked at the drawings, it became obvious to me that the interior bars of the space frame's upper level had been braced, all of them, but the bars on the boundary were only braced on one side; they were not braced out of boundary. So, those must have buckled, and sure enough, the research proved that one bar, which was the most compressed bar, buckled, the load was transferred to the next, which couldn't take it, and it buckled, and to the next, and in 5 seconds this wave of compression buckling went through the entire frame, and the frame collapsed. This was a lucky case, because no-one got hurt.

The second case I want to mention was a case in which there were 92 people hurt, and went to court together. It's a Ruth Goldberg story, an incredible story. There was a building on 46th Street and Second Ave. in Manhattan occupied by light industry on the lower floors, and offices on the higher floors, and one morning at about 8:30, there was an explosion, and the two facades on 46th St. and on Second Ave. were completely blown out. They hit people on the street, and even people across the street in other buildings. The explanation of this problem was absolutely incredible. It so happens that on the first floor of the building, where they had some mechanical systems, there was a little cylindrical steel tank of compressed air. The tank was covered with a thin shell at the top, and the bottom was also a thin shell. The company that made this tank, and made them by the million (you probably have one of them in the basement of your house) had not tested them for the last 28 years because "they were perfectly safe". Well, we were able to prove that the gadget, that was supposed to stop the increase in pressure of the air at a certain value, malfunctioned. The pressure went above the value of the buckling pressure for the lower little dome (a dome about one and a half ft. in diameter), and the tank exploded. In exploding, the top dome was shot up, and hit a pipe right above it. The pipe was not very well put together, and broke, and it contained gas. All this happened at 5:00 in the morning. The elevators began operating at about 7:00, and they sucked the gas up to the floors above, and spread it throughout the building: by about 8:30 the entire building was permeated with gas. One of the employees came in at 8:30, lit a cigarette -- and BOOM! -- the whole building exploded. And why did that happen? Only because that little one and a half ft. dome at the bottom of the tank could not take the pressure and buckled.

If I may, I would like to tell you what happened when I went to court. In court you are confronted with judges who, if you are lucky, do not know anything about engineering, have never heard of the word instability or buckling, and don't interfere. In my trial, the jury's most cultured person had a high school degree and was foreign born, so that he didn't quite understand English. Then I had my attorney, who introduced me as a great man: "This man has two Ph.D's, etc., etc.". Finally we had a cross-examining attorney, who did his best to make me look like a monkey. So, the cross-examining attorney looks at me and says: "You have not one, TWO Ph.Ds, you are an engineer's engineer, aren't you!" And I said, "Yes, I am" because I'm very modest. "Would you be kind enough, Mr. Salvadori, to tell the members of the jury how many of these degrees have been obtained from a good, honest American University? Because, Your Honor, I object to this man being called 'Dr.' How do we know that his so-called Ph.D's are equivalent to an American doctorate?"

I had already been in court a number of times and I am very fast on the draw, so I turned to the Judge, and said: "Your Honor, and members of the jury, I apologize to you. In introducing my credentials, due to my professorial absentmindedness, I forgot that I have a Doctorate in Science from Columbia University." The face of the lawyer looked like an idiot's: he had not read my curriculum vitae carefully and didn't know that, being an honorary degree, it didn't count! But the jury was very impressed, and from then on, I was an American Doctor, and the attorney had to call me Dr.!

But he tried to confuse the jury by saying: "How do you know that this thing buckled?" I explained to him why the tank bottom buckled. We had seen the bottom and it was obvious that it had buckled. Then he said: "Could I ask you what kind of formula you used to prove that this thing buckled?" I came up with my ".06". So he felt very happy and said: "Dr. Salvadori, you know better than I... "(whenever somebody says to me that I am supposed to know better than he, I know he is lying). "You know better than I, that there is a formula by the famous Dr. von Karman, of international reknown, in which the coefficient is not your .06, but is .27". I answered: "Mr. Attorney, first of all, Theodore von Karman was a very dear friend of mine" (so von Karman and I in the eyes of the jury were equals), "and secondly, Dr. von Karman knew very well that, unfortunately, his formula did not check the tests, and mine does." Well, he couldn't answer that, so he turned away from me, and said : "Well, Dr. Salvadori, you must agree though, there are always two ways of skinning a cat" -- "Yes, I agree to that" I said -- "but there is always one right way and one wrong way, and in this case mine is the right way!". So, that was the end of that and I won the case.

The point I'm trying to make, while I try to keep you awake, is that the studies you are performing and that have advanced the field of instability to a wonderful degree in the last 20 years (the last Japanese code has .18 in front of the formula, so we are getting there slowly) have not only a tremendous appeal for those of you who, perhaps like me, enjoy working on the theory of structures, but have also a tremendous influence on the codes, on the judgements of the engineers who pass on the design of buildings, in all the building departments of every single city in the United States, and that therefore, your job is not only an important job theoretically, but it is a job of deep professional responsibility.

I have been aware of this, and I was delighted when at least one of my formulas made it into a code. I gave up the field, as I told you, over 25 years ago, so I certainly am not qualified to talk about the present state of discrepancies between theory and tests, but it has been reduced greatly and I try to keep up with the literature.

I would like to press upon you that small as your group actually is, and it cannot be very large for two reasons: firstly, that to work in this field, one has to be very strong on theory, and secondly, that one must be either a very strong experimentalist or capable of putting the two fields together. The only person in my whole life, whom I have seen work equally well in both the experimental and theoretical fields was my dear friend, Enrico Fermi, but he was a genious, so he was in a different category.

Now, I would like to end with one episode that has very little to do with buckling, but has to do with Timoshenko. Timoshenko and I and others were at a meeting in Italy of the international group that at that time ran the Congresses of Applied Mechanics, and it was my great joy to invite Timo to my villa in Tuscany. He, an old Russian gentleman came, spent two or three days with us, and when he left, I suddenly realized that for three days he had been either with me alone or with members of my family who did not speak English, but he had never mentioned elastic stability or any other technical subject. He was a gentleman, a guest of an Italian family, and he knew that it would not have been proper for him to talk shop among laymen. And in coming here, it just occurred to me that I'm an anti-Timoshenko, because I'm in front of a group of people who really know what elastic stability is today, am extremely ignorant in the field, and yet for a plate of good food, I sold my soul to you. I apologize.

I don't believe that my few remarks demand any explanations, but if by any chance anybody had in mind any questions about any of the nonsense I spoke about (between you and me some of it was not nonsense at all) I would be very glad to answer.

**Bruce Johnston:**

I would like to have you tell about your teaching experiences in the New York City school system.

**Dr. Salvadori**

My good friend Bruce would like me to tell you a few words about what I'm doing in the New York City schools. Eleven years ago, the New York Academy of Sciences, of which I have been a vice president, sent a letter to all the members living around New York, asking them whether they would, in one way or another, help with the teaching of math and science in the New York City schools, which is a big problem. About 140 of us answered. A chemist said: "Bring the kids to my lab one day and I will show them how lovely it is to make chemicals"; an astronomer said: "All right, I'll take a group of 20 to the planetarium and show them how glorius it is to look at the sky", and then there was a nut who proposed a course of 14 two-hour lectures on "Why Buildings Stand Up". And to his amazement (of course, it was me) the course was accepted. Well, I went into East Harlem, which is Spanish Harlem, with the excuse that I speak Spanish much better than I speak English, and I was scared stiff. My students have varied in age, I would say, from about 18 to about 60. When I taught the first year at Columbia in 1940 there were in the front row of my class, two gentlemen, about 50 years old, obviously Austrian, and as I went on with my applied mathemetics formulas, they would go..."Ah, hah... Ah, hah....Yes, Ah, hah." So after the third "Ah, hah", I stopped and said: "Look, you seem to know all this...", and they said: "Yes, we are both teachers; we have taught this material..." So I asked: "Then why are you here?" And they said: "Because this is the best show in town."

So I have had students of all ages, but I had never taught children. But I have a basic principle: that one cannot throw water down the throat of people who do not want to drink, and this is true of education. Education has to be wanted before it can be imparted. So, both at Columbia, and in the high schools, I first explained why the students or the children may want to know about this or that. For example, when I first taught differential equations, my first lecture started: "Welcome, gentlemen", (there were no women at the time). "Let's go together to a restaurant, you order your meal, and the waiter will ask: 'Do you want your coffee now, or do you want it at the end of the meal?', and you say: 'I would like it at the end because I want it hot'. So of course, he brings it with the first course and puts the cream next to the coffee. Now the question is, to get the hottest possible coffee at the end of the meal when you are going to drink it, do you pour the cream now, or do you wait until the end of the meal?" I would have a show of hands, and usually 25% said they would wait, 25% would put the cream in right away, and 50%, the cowards, did not express an opinion, because they were scared. Then I began setting up the problem, and without ever mentioning the word "differential equation" I ended up on a first-order differential equation, which can be integrated by elementary calculus and we concluded that you have to pour the cream right away. This is now known at Columbia as the Salvadori Coffee problem. Another time when I had to explain the use of anti-trigonemetric functions, I thought: "Who wants to know about anti-trigonometric functions? It's ridiculous. If I have trigonometric funtions, I look at the tables the other way around, and I get anti-trigonometric functions." So, instead, I said: "Let me assume that today you want to go a movie and, exceptionally, you just go because you want to see the movie and you are not going with your girlfriend. Now if you sit right next to the screen, you are going to see the screen under a very small angle, if you sit at the very back of the room, you are also going to see the screen under a very small angle, so in-between these two extreme positions there must be a row of seats from which you see the screen at the best possible angle. Would you be interested to find out where to sit when you go to the movies to see the film?" "Oh, yes." So we solve the problem, and we run into anti-trigonometric functions. Now, with the kids, I did exactly the same thing. When people ask me: "What do you teach them?", I answer, the same course I teach my graduate students at Columbia, but from the very beginning I have them build models of cardboard, paper, tongue depressors, or string, not models that are just a reduced-scale representation of a bridge or a building, but models which we then test and which have a structural behavior identical with that of the building they represent.

The kids get absolutely excited: they love it, and I didn't quite understand why we had been so successful until I had dinner with Anna Freud, the daughter of Sigmund Freud (we were getting an honorary degree together), and I explained to her what I was doing, and how amazed I was at the reaction and the intelligence of these children. In architecture at Columbia, I say to them: "Look, this piece of paper can't stand up because it's too thin, it's no good, but all that you have to do is to give it a little curvature, and without adding any material to it, it stands up! It cantilevers, and if I put a pencil on top, it even carries a load!" When I worked with my 12 year olds, I said: "You see this paper, it doesn't stand up..." and suddenly it occurred to me to ask: "Would anyone tell me whether there is a way of making it stand up?" And a little tot, this tall, came to me and said, "Give it to me." I gave it to him, and he curved it and said: "Here." I was furious, because he had stolen the show! So I said: "Well, that's very clever of you (damn it!) but can you do better than that?" He says: "Sure, give it to me." And believe it or not, the child who had never left Harlem (he had never seen the middle of Manhattan, because this is the situation among the poor blacks of Harlem) folds the paper, and says: "There you are." He had created a folded plate. Now my students at Columbia (and they are the cream of the crop) would never have guessed that. I've now been teaching kids for eleven years. I have taught adults in Europe all over the place, in Africa, in Asia, in South America, but I don't believe I have ever enjoyed teaching as much as with these kids, who are extremely bright and who, as Anna Freud explained to me, I get at the right age because at that age the kids love to use their hand skills, and they do not like too much to deal with abstract concepts (grammar and mathematics present difficulties), but when you talk to them about beams and columns and arches, they just look around and this is what they see in New York. And she added: "You probably don't know this, but our intelligence grows up to the age of puberty, and then it's all the way down to the end." I felt terrible, because I was seventy at the time, but then I realized she was 82. So I didn't mind.

**John Springfield:**

I'd now like to call on Gerry Fox to thank our speaker.

**Gerry Fox:**

I thought I was going to do this by letter. Well, it was a pleasure to me to convince Mario to come here. I'm glad he accepted and I'm sure that you all enjoyed listening to Mario. I also teach at Columbia, I'm an adjunct professor and he is a legend on campus, and rightly so. I think we were very fortunate in having such a grand speaker and I thank you, Mario, very much for coming here today.

I'd like to give you one statement that a colleague of his at Columbia once told me about him: "Mario is a delightful spirit".

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