

mechanics

Academia Americana de Mecánica

American Academy of Mechanics

Volume 31, Number 3-4

March-April 2002

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mechanics

mechanics provides its readers with news in the field of theoretical and applied mechanics, and serves as a forum for the presentation and discussion of issues related to the development of the science and profession of mechanics. Opinions expressed are those of the authors and do not necessarily reflect official points of views of AAM or the institutions with which the authors are affiliated.

Editor: Horacio D. Espinosa (Northwestern University, U.S.A.)

Associate Editors: Gustavo Buscaglia (Balseiro Institute, Argentina), Eduardo Dvorkin (Univ. Nac. de Buenos Aires, Argentina), Robert Haythornthwaite (Temple University, U.S.A), Dean T. Mook (Virginia Tech, U.S.A), Djenane Pamplona (PUC-Rio, Brazil), G. Ravichandran (Caltech, U.S.A).

The *American Academy of Mechanics* is a non-profit corporation incorporated in 1969 under the laws of the Commonwealth of Pennsylvania. Its objective is to advance the science and profession of mechanics, with particular reference to the countries of North, South, and Central America. It aims to facilitate cooperation among mechanicians, to encourage recognition of achievements in mechanics, and to promote public understanding of the work of the mechanician.

Board of Directors (2001): M. Beatty (University of Nebraska), President and Chairman of the Board; F. Moon (Cornell University), Immediate past President; D.T. Mook (VPI and State University), Secretary; R.A. Heller (VPI and State University), Treasurer; L. Virgin, Director, Region IA (Eastern USA); S. Datta (University of Colorado), Director, Region IB (Central and Western USA); M. Paidoussis, Director, Region II (Canada); P. Kittl (University of Chile, Chile) Director, Region III (Central and South America); R.M. Haythornthwaite (Temple University), Publisher; C.W. Bert (Univ. of Oklahoma), Secretary to the Fellows.

mechanics (ISSN 0076-5783) **POSTMASTER:** Send address changes to *mechanics*, Subscription and Membership, ESM, MC 0219, Virginia Tech, Blacksburg, VA 24061 (Tel. 540-231-6871: Fax 540-231-2290). Editorial and Advertising: Horacio D. Espinosa, Northwestern University, 2145 Sheridan Rd., Evanston, IL 60208-3111 (Tel. 847-467-5989, Fax 847-491-3915). Membership in the American Academy of Mechanics includes the subscription to *mechanics*.

American Academy of Mechanics
Academia Americana de Mecánica

Letter from the Editor

May 12, 2002

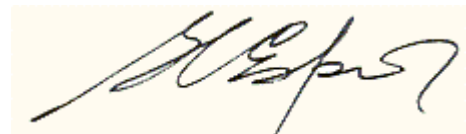
Dear AAM members,

As you may have noticed, a year ago I started a section named "Selections of the Editor." The idea has been to bring to you information on new technologies, historical notes or as in this issue, articles discussing the future of fast evolving technologies from a business perspective, see "The Future of the Microprocessor Business," by M.J. Bass and C.M. Christensen, as well as "The Opportunity and Threat of Disruptive Technologies," by C.M. Christensen. The insight provided by these articles is very valuable in guiding our choice of research endeavors with the potential of having a significant societal impact.

I would like to take this opportunity to invite you to contribute to *Mechanics* with articles written for a general audience addressing important educational developments, historical notes on various aspects of mechanics or other topics you may consider appropriate. The articles should be preferably written in English but this is not a requirement. In fact, this issue contains an article entitled "About the Path to Progress in Buckling Theory," by L.A. Godoy, which is presented in Spanish. I hope that this editorial decision will encourage more contributions from Central and South American AAM members or readers.

Lastly, I would like to bring to your attention a letter I received from Dr. P.P. Nelson, Director of the Civil and Mechanical Systems Division at the National Science Foundation, which is published with her permission. Two issues discussed in her letter deserve special attention from you. The first is the announcement of the retirement of Dr. Jorn Larsen-Basse. I am sure you all will join Dr. Nelson in thanking Jorn for his excellent service and support to our community for the past many years. The second is the announcement of a number of position openings in the CMS Division including the Director position. As you may be aware, some people in the mechanics community have expressed dissatisfaction and frustration with what is perceived as a low funding level in the area of mechanics and as a result a low success rate of proposals submitted to CMS. Although this perception may be argued, I would like to quote a statement by Dr. O. Dillon, which he recently told me in regards to this issue, "... people in mechanics need to get more involved and serve in various capacities at NSF in order to increase recognition of our field's contributions and its societal impact." I am sure you will agree with Oscar's reasoning; hence, I urge you to become involved and submit nominations to Dr. Nelson.

Best wishes to all and a special greeting to all the moms in our community.



Horacio D. Espinosa
Editor

POSITION OPENINGS**National Science Foundation
Civil and Mechanical Systems (CMS) Division**

Greetings:

I would like to inform the mechanics community that Dr. Jorn Larsen-Basse, who served as a program manager in surface engineering and materials engineering for many years, has informed NSF of his intention to retire at the end of this calendar year. I encourage you to express your appreciation to Dr. Larsen-Basse for his years of service to your community - he has done a wonderful job!

At this time, the Civil and Mechanical Systems (CMS) Division has vacancies for Program Managers in the three Program areas, one of which is the 1633 program:

- The Structural Systems and Hazards Mitigation of Structures (SSHM) Program (1637)
- The Dynamic System Modeling, Sensing and Control (DSMSC) Program (1632)
- The Solid Mechanics and Materials Engineering (SMME) Program (1633)

The link to the CMS web page is <http://www.eng.nsf.gov/cms/> and descriptions of these opportunities can be found through that page.

I expect that you know of colleagues who would be good candidates. Please consider and send the names and contact information for those you wish to nominate to me (pnelson@nsf.gov, 703-292-7018) or to the CMS Search Coordinator (Dr. Richard Fragaszy, rfragasz@nsf.gov, 703-292-7011).

These positions are not limited to individuals who currently hold academic positions, so think about anyone you might know in industry or in a government job as well as faculty members. The CMS Division needs highly qualified individuals to manage existing programs and to expand the grant opportunities we provide for Civil, Mechanical and related engineering disciplines. We all benefit from having well qualified, enthusiastic staff in CMS.

In addition, we are advertising for the important position of George E. Brown, Jr. Network for Earthquake Engineering Simulation (NEES) Equipment Project Coordinator. This position has been occupied by Dr. Thomas Anderson, and he has informed NSF that he will be leaving this position during this summer after two years of service. We thank Tom for his contributions, and encourage you to consider nominating persons from academe or industry for this very important position.

The position of Division Director (currently my position) is also being advertised. We need your help in identifying highly qualified candidates for all of these positions.

Please feel free to contact me if you have any questions.

Best regards,

Dr. Priscilla P. Nelson
Director, Division of Civil and Mechanical Systems
Directorate for Engineering
National Science Foundation
4201 Wilson Boulevard, Suite 545
Arlington, VA 22230
Tel: (703) 292-7018 (direct) or 292-8360 (general)
Fax: (703) 292-9053
pnelson@nsf.gov



Civil Engineering
School of Engineering and Applied Science
Department Chair Position

The University of Virginia School of Engineering and Applied Science invites applications for the chairperson of the Department of Civil Engineering. The University of Virginia has approximately 18,000 students pursuing degrees in nine colleges and professional schools, including the School of Engineering and Applied Science (SEAS). The University of Virginia is consistently ranked as one of the top public universities in the country. The School of Engineering and Applied Science has approximately 2,000 undergraduate students, 600 graduate students, 150 tenure and tenure track faculty members in nine departments, and conducts approximately \$40 million per year in externally funded research programs. The Department of Civil Engineering has more than 150 undergraduate students, 75 graduate students, and 15 faculty members. The department offers B.S., M.S., M.E., and Ph.D. degrees in civil engineering. The department has undergone significant growth and programmatic evolution. The department is implementing a new undergraduate curriculum, actively expanding its graduate research program, and is engaged in multi-disciplinary programs with other departments, schools, and universities. Active research programs exist within three main thrust areas: environmental and geo-environmental engineering; structural engineering and applied mechanics; and transportation systems, operations, planning, and safety. We seek a chairperson who can provide leadership for a department committed to excellence in teaching, research, and service, guide the growth of new promising directions, and visibly enhance the department's national stature.

Requirements: Candidates must have an earned doctorate in an appropriate discipline and an outstanding record of scholarship and research achievement. Candidates with exceptional credentials will be considered for an endowed professorship. Applications will be reviewed as they are received and until the position is filled.

Interested Candidates please submit a letter of application to:

Professor James H. Aylor, Search Committee Chair, Ref. #5954, School of Engineering and Applied Science, Dept. of Electrical & Computer Engr., University of Virginia, 351 McCormick Road, P. O. Box 400743, Charlottesville, VA 22904-4743; Email: jha@virginia.edu; Phone: 434-924-6100.

Applicants are encouraged to visit the departmental website for more information at:
<http://www.cs.virginia.edu/~civil/>.

The University of Virginia is an equal opportunity, affirmative action employer.

ANNOUNCEMENTS

AAM AWARDS NOMINATIONS

The Awards Committee of the American Academy of Mechanics requests nominations for the following two awards to be presented at the upcoming ASME International Mechanical Engineering Congress and Exposition in November, 2002

The 2002 American Academy of Mechanics Outstanding Service Award

Preliminary nominations should consist of a one-page letter describing the outstanding service of the nominee to the Academy as well as to the profession, along with a one-page biographical sketch of the nominee, together with the names of at least three people willing to write letters of support in the event that the Awards Committee requests them.

The 2002 American Academy of Mechanics Junior Award

This award, which will be based primarily on research accomplishments, is open to mechanics whose highest degree was conferred after December 31, 1991.

Preliminary nominations will consist of a one-page nominating letter, one-page curriculum vitae, and re- (or pre-) prints of the candidate's best three papers. In the case of co-authorship of any work submitted, the candidate's contribution must be clearly and explicitly indicated in an attached note.

Please Note:

*All preliminary nominations for both awards must be postmarked by August 1, 2002. * Those that do not arrive in a timely fashion will not be considered.*

Once the preliminary materials are reviewed, additional biographical and complete dossiers may be requested of applicants under consideration.

For additional information, please do not hesitate to contact Simon Ostrach at sxo3@po.cwru.edu or Stephen Crandall at crandall@mit.edu, who are current members of the awards committee.

AAM Awards Committee

Simon Ostrach, *Chair*, Stephen Crandall, Tinsley Oden, Wolfgang Knauss, Romesh Batra

*** Please note change in submission date from previous years.**

V
**NOMINATIONS FOR FELLOW OF
THE AMERICAN ACADEMY OF MECHANICS**

NOTES:

- Persons nominated must be members of the Academy (Bylaws, III 1).
- Nominations must be made by two of the Fellows (Bylaws, III 2).
- Fellows shall be elected on the basis of their contributions to mechanics (Bylaws, III 1).

Name of the member nominated (*please print*)

Citation (*please state in no more than **fifteen words** the basis of the nomination*)

Nominated by (*signatures of two fellows required*)

Signature: _____

Printed Name: _____ **Date:** _____

Signature: _____

Printed Name: _____ **Date:** _____

Please mail this nomination form (no faxes please!) to:

Professor C. W. Bert
School of Aerospace & Mechanical Engineering
The University of Oklahoma
Norman, Ok 73019-1052, USA

so as to reach him on or before September 24, 2002. The nomination should include one good copy of a two-page resume of the member nominated.

American Academy of Mechanics Founders Prize and Grant

Up to \$10,000 For the Academic Year 2002-2003
(Deadline: July 1, 2002)

The American Academy of Mechanics is pleased to announce the availability of a Founder's Prize and Grant to be awarded in September 2002 to a doctoral candidate in the field of Mechanics. Funding has been arranged by the Robert M. and Mary Haythornthwaite Foundation through the good offices of Professor Haythornthwaite, founder and first President of the Academy. The award will be made on the recommendation of an AAM committee. The prize consists of a Certificate and \$1,000 that will be presented at the annual meeting of the Academy, usually held in November. The Grant will be made to that same person in two installments, \$6,000 in September 2002 and up to \$3,000 in January 2003, the latter dependent on the size of the approved budget and receipt by the AAM committee of an acceptable progress report. In order to encourage contestants to think constructively about the impact of new and pending developments, they will be asked to compose an original essay of no more than a thousand words under the title "Progress through Mechanics". The winning essay will be published in *mechanics*. The award is open to those who, as of July 1, 2002, are registered as graduate students at a degree granting institution within the Americas, have completed at least one year of full-time graduate study at that institution, have been assigned a thesis advisor at the institution and have had a doctoral thesis topic emphasizing mechanics approved by the institution following candidacy or equivalent procedures. There are no restrictions with regard to citizenship, residency, race, religion, or sex. Letters of support will be required of the thesis advisor and in addition one from either a Member or a Fellow of AAM. Contestants will be judged on the basis of the essay, plans, references and academic history. The intent of the Grant is to support the research of the student through an approved combination of equipment purchases, information access, travel, etc., but not routine living expenses or fees. Detailed rules for the competition will be issued soon: to receive them, express your interest to the committee by FAX to (215) 204-6936, or by writing to:

AAM Founders Prize and Grant Committee
c/o Civil and Environmental Engineering Department
Temple University (084-53)
Philadelphia, PA 19122

PACAM VIII

Eighth Pan American Congress of Applied Mechanics

January 5-9, 2004

Havana, Cuba

<http://www.pacam8.mcgill.ca/>

The Eighth Pan American Congress of Applied Mechanics (PACAM VIII), jointly sponsored by the University of Havana, the Institute of Cybernetics, Mathematics, and Physics of Cuba, and the American Academy of Mechanics, will be held January 5-9, 2004 at the Convention Center, Havana, Cuba. The Honorary Chairman of the Organizing Committee is Prof. Alina Ruiz Jhones of the University of Havana. The Co-Chairmen are Prof. Martin Ostoja-Starzewski of McGill University and Prof. Reinaldo Rodríguez-Ramos of the University of Havana. The Chairman of the Editorial Committee is Prof. Julián Bravo-Castillero of the University of Havana, and the Chairman of the Local Arrangements Committee is Prof. Raúl Guinovart-Díaz of the University of Havana, Cuba.

The aim of sponsors is to promote progress in the broad field of mechanics by (1) exposing engineers and scientists, including graduate students, to new research findings, techniques, and problems, and (2) providing opportunities for personal interactions between mechanics of North and South America, as well as other continents. It is the only conference sponsored by the American Academy of Mechanics (AAM).

The Pan American Congresses of Applied Mechanics are held every two years early in January, always in a Latin American venue, at a time when few other conferences are scheduled. The previous Congresses were held in Rio de Janeiro, Brazil in 1989; Valparaiso, Chile in 1991; São Paulo, Brazil in 1993; Buenos Aires, Argentina in 1995; San Juan, Puerto Rico in 1997, Rio de Janeiro, Brazil in 1999 and Temuco, Chile in 2002. Participants come from the Americas as well as Africa, Asia, Australia, and Europe.

Persons willing to organize special sessions in any area of mechanics are welcome to contact the organizers listed below. All the future announcements will be made at the website listed above, and through the AAM (<http://www.AAMech.org/>). Four-page papers for the conference proceedings will be due June 30, 2003, at either address:

Latin American Co-Chairman
 Prof. Reinaldo Rodríguez-Ramos
 Facultad de Matemática y Computación
 Universidad de la Habana
 San Lazaro y L, Vedado, CP 10400
 La Habana, Cuba
 reinaldo@matcom.uh.cu
<http://www.uh.cu>

North American Co-Chairman
 Prof. Martin Ostoja-Starzewski
 Department of Mechanical Engineering
McGill University
 817 Sherbrooke Street West
 Montréal, PQ, Canada H3A 2K6
 martin.ostoja@mcgill.ca
<http://www.mcgill.ca>

International Symposium on Dynamic Problems of Mechanics (X-DINAME)***Dynamics of the Brazilian Society of Mechanical Sciences***

10-14 March, 2003
Hotel Recanto das Toninhas
Ubatuba, São Paulo

Call for Abstracts**Abstract Criteria:**

Abstracts of scientific works are invited in all dynamic problems of mechanics. The work should be original in scope, of outstanding quality and of permanent interest.

Abstracts are invited in the following topic areas:

- Structural and machine dynamics
- Control and optimization
- Noise and vibration
- Modal analysis
- Any other correlated subject.

Information about the meeting:

The meeting is to take place on the week 10-14th of March, 2003 at Hotel Recanto das Toninhas (<http://www.toninhas.com.br>), Ubatuba, São Paulo. Ubatuba is located somewhere in between Rio and São Paulo, in one of the most attractive and exuberant coastal areas of Brazil, with a consistent touristic structure of hotels and restaurants. It is adjacent to the preservation sanctuary of the Atlantic Ridge Rain Forest and beautiful tropical beaches. The hotel, which is a resort by the sea, provides the right kind of welcoming atmosphere and international standards service which is required for the immersion activities of DINAME. Since the hotel has a limited number of apartments early booking is encouraged.

Key Deadlines:

April 30, 2002: Last day for submission of abstracts for preliminary evaluation of contributions.

May 24, 2002: Notification of selected abstracts and invitation for submission of full texts.

July 31, 2002: Last day for receiving full texts.

August 30, 2002: Notification of accepted papers and/or required modifications of the text.

September 30, 2002: Last day for receiving modified texts.

January 15, 2003: Last day for confirming early booking with the hotel.

A home page for the conference is to become available soon. Any additional information can be obtained by writing to the following address: xdiname@fem.unicamp.br

IX
IUTAM Symposium
On
**Integrated Modeling of Fully Coupled Fluid-Structure
Interactions Using Analysis, Computations, and Experiments**
1 June-6 June 2003
New Brunswick, New Jersey USA

<http://cronos.rutgers.edu/~mechaero/iutam>

Chairs:

- Haym Benaroya and Timothy Wei, Rutgers University, New Jersey
benaroya@rci.rutgers.edu and twei@rci.rutgers.edu

Scientific Committee

- Peter Bearman, Imperial College of Science, Technology and Medicine, UK
- Earl Dowell, Duke University, North Carolina
- Helmut Eckelmann, Georg-August-Universität, Germany
- Peter Monkewitz, IMHEF-EPFL, Switzerland
- Michael Paidoussis, McGill University, Canada
- John Sheridan, Monash University, Australia
- H.K. Moffatt, Isaac Newton Institute for Mathematical Sciences, UK

This Symposium will provide a forum for the latest thinking in analytical, computational and experimental modeling of structures interacting with fluid environments. The specific objective is to provide a structured format in which meaningful and lasting dialogues can be facilitated between leading researchers in the different component disciplines. It is intended that, through these dialogues, multidisciplinary linkages will be established leading to integrated approaches to modeling the complex, nonlinear interactions between fluids and structures. Examples of classes of interactions that may be addressed in this Symposium include ocean structures, fluid conveying structures, and aerospace structures. The energy transfer processes are inherently nonlinear in all aspects of the behavior. The important class of vortex-induced oscillations has regions of lock-in, where the structural natural frequencies rather than the fluid velocity govern the shedding, and there exists hysteretic behavior. The real fluid-structure system is one of complex exchanges of forces and energies, resulting in highly nonlinear behaviors. The ability to model, solve and test fully coupled fluid-structure systems portends a rich and profound understanding. In fact, recent research efforts have indeed started to focus on the development of fully coupled models. This Symposium is therefore a response to these new and exciting developments in the field. By bringing together a critical mass of key researchers in each discipline, and organizing the program to focus on multidisciplinary problem solving, this process of developing fully coupled fluid-structure interaction research programs can be reinforced and enhanced. We look forward to receiving abstracts for review, presentation, and eventual full publication, of topics that fall within the broad framework defined above.

DEADLINES

Submission of Abstracts: 1 February 2003
Notification of Acceptance: 15 March 2003
Hotel & Symposium Registration: 1 May 2003
Symposium: Sunday 1 June - Friday 6 June 2003
Final Manuscripts Due: Friday 13 June 2003 (FIRM)

FOURTH INTERNATIONAL SYMPOSIUM ON VIBRATIONS OF CONTINUOUS SYSTEMS

The Fourth International Symposium on Vibrations of Continuous Systems will take place in Keswick, England, July 7-11, 2003. The primary goal of this Symposium is to bring together outstanding experts in the field of vibrations of continuous systems from all over the world, to discuss technical topics in a very informal atmosphere. As before, participation will be by invitation only, and will be limited to maximum numbers of 50 participants and 40 presentations.

The Symposium is devoted to the vibrations of continuous systems (e.g. strings, rods, straight and curved beams, membranes, plates, shells, and three-dimensional bodies). Examples of topics to be considered include: free and forced vibration, linear and nonlinear vibration, undamped and damped vibration, fluid-structure interaction, and structural elements of composite material.

The Symposium location, Keswick, is in the heart of the Lake District of northwestern England, famous for its beautiful lakes and hills. Typical days at the Symposium will consist of morning hikes or bus excursions, presentation sessions in the afternoons, and social gathering times in the evenings. The outings and social gatherings have proved to be excellent ways of generating relaxed and informal technical discussions and friendships which have been of great value to ongoing research.

Individuals who are interested in taking part in this Symposium should write to:

Professor Arthur W. Leissa
General Chairman, ISVCS IV
Dept. of Mechanical Engineering
206 West 18th Ave.
Ohio State University
Columbus, Ohio 43210
USA

Letters should be accompanied by a one-page summary of the writer's research accomplishments (include a list of published books, papers, reports, etc.) in the theme of this Symposium.

AAM Annual Business Meeting

The **Annual Business Meeting** of the American Academy of Mechanics will be held on **Thursday, June 27, 2002**, in conjunction with the 14th USNCTAM Meeting (June 23-28, 2002) at the Virginia Polytechnic Institute and State University at Blacksburg. The conference banquet will be held in the evening from 7:00 p.m. to 10:00 p.m. Plan now to participate in the AAM business meeting.

The **Board of Directors** will meet 11:30 a.m. to 1:30 p.m. for a working luncheon. The Secretary will issue an agenda within thirty days prior to the Board Meeting.

The **Fellows Meeting** will convene from 1:30 p.m. to 2:30 p.m. At this time, the President-Elect presiding and in accordance with Bylaw Article III.3: "The Fellows shall elect one of their number as Secretary to the Fellows." The term of office shall be from three to five years, to be specified.

The **General Members Meeting** will be held from 2:30 p.m. to 3:30 p.m.

**CALL FOR NOMINATIONS
FOR DIRECTOR OF REGION IB,
CENTRAL AND WESTERN USA**

Nominations are called for Director of Region IB, Central and Western USA, for a three-year term of service commencing in June 2003. The nominee must be an active Member of the Academy registered in that region; and nominations shall be accepted only from active Members resident in that region. Valid nominations must bear the signatures of five (5) Members of the Academy who are residents of the region and are Members in good standing. Nominations shall include a letter from the nominee accepting the nomination. The deadline for submissions is September 30, 2002. Early nominations are strongly encouraged.

Please submit all nominations to

Professor Dean Mook, Secretary
American Academy of Mechanics
Department of Engineering Science and Mechanics
Virginia Polytechnic and State University
Blacksburg, VA 24061-0219

**CALL FOR NOMINATIONS
FOR DIRECTOR OF REGION III,
CENTRAL AND SOUTH AMERICA**

Nominations are called for Director of Region III, Central and South America, for a three-year term of service commencing in June 2003. The nominee must be an active Member of the Academy registered in that region; and nominations shall be accepted only from active Members resident in that region. Valid nominations must bear the signatures of five (5) Members of the Academy who are residents of the region and are Members in good standing. Nominations shall include a letter from the nominee accepting the nomination. The deadline for submissions is September 30, 2002. Early nominations are strongly encouraged.

Please submit all nominations to

Professor Dean Mook, Secretary
American Academy of Mechanics
Department of Engineering Science and Mechanics
Virginia Polytechnic and State University
Blacksburg, VA 24061-0219

SELECTIONS OF THE EDITOR

The Future of the Microprocessor Business

Reprinted with permission from Spectrum

Vol. 39, No.4, April 2002

By Michael J. Bass and Clayton M. Christensen

In a century in which technology left few aspects of life unchanged in some countries, the microprocessor may have been the most transformative of all. In three decades it has worked itself into our lives with a scope and depth that would have been impossible to imagine during its early development.

If you live in a developed country, chances are good that your household can boast of more than a hundred microprocessors scattered throughout its vehicles, appliances, entertainment systems, cameras, wireless devices, personal digital assistants, and toys. Your car alone probably has at least 40 or 50 microprocessors. And it is a good bet that your livelihood, and perhaps your leisure pursuits, require you to frequently use a PC, a product that owes as much to the microprocessor as the automobile owes to the internal combustion engine.

Throughout most of its history, the microprocessor business has followed a consistent pattern. Companies such as Intel, Motorola, Advanced Micro Devices, IBM, Sun Microsystems, and Hewlett-Packard spend billions of dollars each year and compete intensely to produce the most powerful processors, which handle data in 32- or 64-bit increments. The astounding complexity and densities of transistors on these ICs—now surpassing 200 million transistors on a 1-cm² die—confer great technical prestige on these companies. The chips are used in PCs, workstations, and other systems that, for the most part, have been lucrative, high-volume markets.

As with other ICs, microprocessors have for the past few decades been undergoing the exponential rise in performance prophesied by Moore's Law. Named for Intel Corp.'s cofounder, Gordon E. Moore, it describes how engineers every 18 months or so have managed to double the number of transistors in cutting-edge ICs without correspondingly increasing the cost of the chips. For microprocessors, this periodic doubling translates into a roughly 100 percent increase in performance, every year and a half, at no additional cost. The situation has delighted consumers and product designers, and has been the main reason why the microprocessor has been one of the greatest technologies of our time.

In coming years, however, this seemingly unshakable industry paradigm will change fundamentally. What will happen is that the performance of middle- and lower-range microprocessors will increasingly be sufficient for growing—and lucrative—categories of applications. Thus microprocessor makers that concentrate single-mindedly on keeping up with Moore's Law will risk losing market share in these fast-growing segments of their markets. In fact, we believe that some of these companies will be overtaken by firms that have optimized their design and manufacturing processes around other capabilities, notably the quick creation and delivery of customized chips to their customers.

The changes portend serious upheaval for microprocessor design, fabrication, and equipment-manufacturing firms, which have been laser-locked on Moore's Law. Executives lose sleep over whether they can keep on shrinking line widths and transistors and fabricating larger wafers. We don't blame them, given their history. Nor do we see blissfully peaceful slumber in their near future: this is not another article forecasting the imminent demise of Moore's Law.

On the contrary, we believe that the top IC fabricators will have little choice but to invest ever more heavily so as to keep on the Moore trajectory, which we expect to go on for another 15 years, at least. We don't see these investments as sufficient for future success, however.

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Will semiconductors hit a physical limit? They surely will, someday. But this is probably the right answer to the wrong question. The more important question is: as technological progress surpasses what users can use, how do the dynamics of competition begin to change?

Bottom to top

The stakes are high. The microprocessor market, which totals about US \$40 billion a year, has several main tiers. At the top are the most powerful chips, which are used in servers and workstations. Then there's the PC market, dominated by Intel microprocessors. These relatively high-end chips were a major component of a category that rang up US \$23 billion in 2001, after peaking at \$32 billion the year before, according to the Semiconductor Industry Association (San Jose, Calif.), a trade group.

Microcontrollers were another important category, with sales totaling \$10 billion in 2001. They are generally less computationally powerful than high-end microprocessors, and exert real-time control over other systems, such as automobile engines. Finally, digital signal-processing chips, a key component of cell phones, DVD players, and other entertainment products, had sales of \$4 billion last year.

With their exponentially increasing performance, microprocessors might seem unique and unlikely to follow the broad evolutionary pattern that has played out in the past in most other technology-based industries. After all, the Moore's Law phenomenon is unprecedented in industrial history. But strong evidence shows that the same evolutionary pattern that occurred in mainframe computers, personal computers, telecommunications, banking, hospitals, and steel production is indeed occurring in the microprocessor business.

The pattern begins with a stage in which available products do not yet perform up to most customers' needs. So, not surprisingly, companies compete by making better products. In other words, competition during this stage is basically driven by performance. As engineers design each successive generation of product, they strive to fit the pieces together in ever more efficient ways to wring the most performance possible out of the technology available.

Typically, major subsystems need to be interdependently designed—and, as a result, a competitor needs to make all the product's critical subsystems. During this phase, the competitive advantage of vertical integration is substantial, so manufacturers do almost everything themselves. This is the way it was in the earliest days of the PC industry, for example.

The next stage of an industry's development begins when the performance of its products has overshot the needs of customers in the less-demanding tiers of the market. These customers won't pay premium prices for more performance, of course, but will pay extra for a product that is extraordinarily reliable, or one that has been customized to meet their specific needs—especially if they can get that ultra-reliable or customized product quickly and conveniently. Ease of use is another feature that customers typically reward with a premium.

To compete on the dimensions of customization, quick delivery, and convenience, product architectures whose pieces are strongly technologically interdependent tend to give way to modular ones, in which the interfaces among subsystems are standardized. This modularization lets designers and assemblers respond quickly to changing customer requirements by mixing and matching components. It also lets them upgrade certain subsystems without redesigning everything.

But perhaps the most important repercussion of modularization is that it usually spurs the establishment of a cadre of focused, independent companies that thrive by making only one component or subsystem of the product. Think Seagate Technology in hard-disk drives, or Delphi Delco in automotive electrical systems.

One of us (Christensen) has studied how industries that are in transition between the two stages present peril for established firms—and opportunity for upstarts. Large firms can easily become blind to shifts in the types of performance that are valued. For an established company, with its well-defined competencies and business models, the obvious opportunities for innovation are those that focus on unsatisfied customers in the higher market tiers. After all, that is where the innovations that the company has structured itself to deliver are still being rewarded by premium prices.

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Inevitably, though, this high-tier, performance-hungry group of customers shrinks as performance gets better and better. At the same time, in lower market tiers there is a huge increase in customers who are willing to back off from the leading edge of performance in exchange for high reliability, customization, ease of use, or some combination of all three.

Too much of a good thing?

This is precisely the juncture at which the microprocessor market has now arrived. Price and performance, fueled by the industry's collective preoccupation with Moore's Law, are still the metrics valued in essentially all tiers of the market today. Even so, there are signs that a seismic shift is occurring. The initial, performance-dominated phase is giving way to a new era in which other factors, such as customization, matter more.

Perhaps the best evidence that this shift is under way is the fact that leading-edge microprocessors now deliver more performance than most users need. True, emerging applications like three-dimensional games, the editing of digital-video files, and speech-to-text tax the fastest available microprocessors. However, few people who regularly run such applications do so on a single-microprocessor PC. And that fact is unlikely to change. In the future, as now, many of these taxing applications will run on special-purpose or separate processors.



Hundreds of Athlon microprocessors, from Advanced Micro Devices, are fabricated on 200-mm wafers at a new plant in Dresden, Germany [above]. Though designed for PCs, the 1-GHz chips perform beyond the levels most PC users need. MATTHIAS RIETSCH/AP PHOTO

In any case, the users who run these applications regularly are few compared to the masses who use their PCs mainly for word processing, scheduling, e-mail, and Internet access. For this majority, high-end microprocessors—Intel's Itanium and Pentium-4, and [Advanced Micro Device's Athlon](#)—are clearly overkill. Running common benchmark programs, these chips can perform more than one billion floating-point operations per second (flops), and in some cases, more than 2 gigaflops. Yet, Microsoft's Windows XP, the most recent version of the ubiquitous operating system, runs fine on a Pentium III microprocessor, which is roughly half as fast as the Pentium-4.

What will the microprocessor business be like after this shift? Consider how the PC business grew from a cottage industry into a global colossus over the past 25 years.

In the early days, the 1970s, vertically integrated companies such as Apple Computer, Tandy, Texas Instruments, Commodore, and Kaypro built their computers around proprietary architectures and generally wrote their own software. Then, in 1981, IBM shook up the industry with its original PC, which had a modular architecture and subsystems built by such suppliers as Intel, Microsoft, and Seagate.

Early on, Apple's products were by consensus the best-performing and most reliable in the industry. But in time, as microprocessors, software, and other key components improved, garden-variety PCs became good enough for mainstream applications like word processing and spreadsheets. Competitive advantage shifted to the nonintegrated companies whose products made use of IBM's modular architecture. These were the clone makers—Compaq, Packard Bell, Toshiba, AT&T, and countless others. Not long after

that, dependability became the central axis of competition, and a few firms with reputations for reliability—IBM, Compaq, and Hewlett-Packard—managed to command price premiums.

By the early to mid-1990s, functionality and reliability had become more than good enough from just about everyone, and the way that computer makers needed to compete shifted again. This change set the stage for Dell Computer Corp. (Round Rock, Texas) to rocket from the lowest tiers of the market to industry dominance. Dell let consumers in their homes or offices specify a set of features and functions for their computer to meet their particular needs. Dell then delivered that computer to their door in a few days. In effect, by coming up with a business model that emphasized customization, speed of delivery, and convenience, Dell pushed the industry into its next stage of development.

Although our example is PCs, other technology-based industries have evolved similarly, from automobiles to mainframes. In every case, the primary dimension of competition migrated from an initial focus on performance to reliability, convenience, and customization. When performance began exceeding what customers in a tier of the market could use, competition redefined the types of improvement for which those customers would pay extra. Further, the types of features that let suppliers demand premium prices shifted predictably from performance to reliability, convenience, and so on. As this happened, moreover, competitive advantage moved from companies that were highly integrated to ones that were not.

Why don't microprocessor makers simply start producing lower-performance chips? A few years ago, Intel Corp. (Santa Clara, Calif.) began doing just that with its Celeron microprocessor. The problem is that Celeron is a one-size-fits-all proposition. Its architecture isn't more modular than that of the Pentium products it is displacing, and it cannot be customized nearly as much as emerging alternatives.

Interestingly, while the latest microprocessors offer higher processing rates than most users need, semiconductor fabrication facilities now offer circuit design teams more transistors than they need[see [figure](#)]. Put another way, the rate at which engineers are capable of using transistors in new chip designs lags behind the rate at which manufacturing processes are making transistors available for use.

This so-called design gap has been widening for some time. In fact, the National Technology Roadmap for Semiconductors noted it five years ago, observing that while the number of transistors that could be put on a die was increasing at a rate of about 60 percent a year, the number of transistors that circuit designers could design into new interdependent circuits was going up at only 20 percent a year.



Microprocessors hundreds of times as powerful as today's should emerge from chip-making equipment using extreme ultraviolet radiation. Sandia National Laboratories unveiled such equipment a year ago [above]. PAUL SAKUMA/AP PHOTO

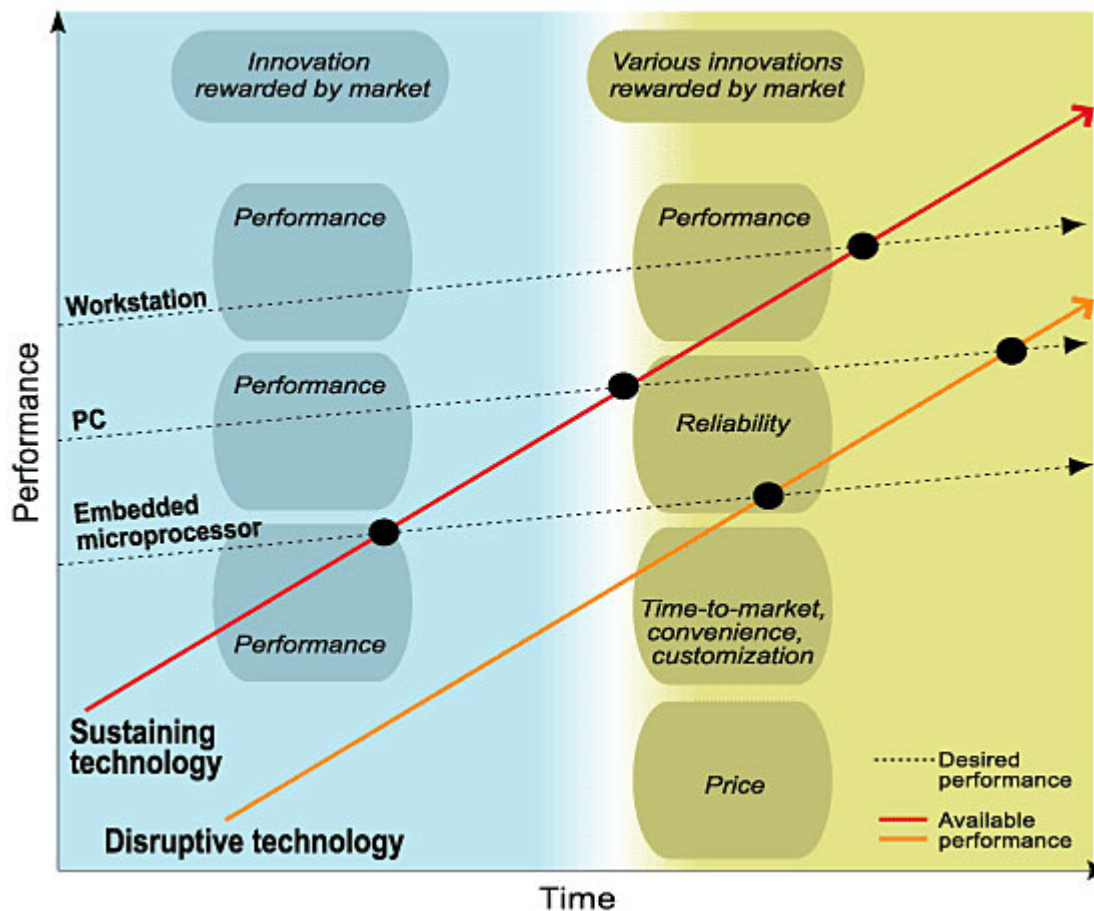
The fact that microprocessor designers are now "wasting" transistors is one indication that the industry is about to re-enact what happened in other technology-based industries, namely, the rise of customization. Keep in mind that in order to develop a modular product architecture with standardized interfaces among subsystems, it is necessary to waste some of the functionality that is theoretically possible. Modular designs by definition force performance compromises and a backing away from the bleeding edge.

Core customization

A form of customization has already taken hold in the lower tiers of the microprocessor industry. System-on-a-chip (SoC) products are modular designs constructed from reusable intellectual property (IP) blocks that perform specific functions [see "Crossroads for Mixed-Signal Chips," IEEE Spectrum, March 2002, pp. 38-43]. IP blocks vary in size and complexity, ranging from simple functions such as an RS-232 serial port interface or a DRAM memory controller, to a complex subsystem, such as an entire 32- or 64-bit microprocessor. These IP blocks can be used within multiple designs within a company, or used in designs at different firms.

In the lower market tiers, several firms are bundling IP blocks into both soft cores (software-like descriptions of the IP blocks that can be synthesized into hardware designs) and hard cores, that is, pre-verified hardware designs. Such cores range from hundreds of thousands to a few million transistors, and their availability in the marketplace enables firms to focus less on new design and more on system integration. They can select and integrate microprocessor and other types of cores into SoC designs that are then manufactured as special-purpose components for a specific product.

Recently, a few companies have been pushing this trend toward component selection and integration even further, into microprocessor cores themselves. Using special design tools, engineers can specify such a microprocessor, and in some cases completely design one, in weeks rather than months.



The Impact of Disruptive Technology

As the performance of conventional microprocessors improves [red line], they first meet and then exceed the requirements of most computing applications [dotted lines]. Initially, performance is key [blue area]. But eventually, other factors—reliability, time-to-market, convenience, and customization—become more important to the customer [green area]. And when a disruptive technology [orange line], such as customizable processor cores, is introduced, it inevitably takes over market share from performance leaders.

Leading companies in this so-called customizable core movement include Tensilica, ARC Cores, Hewlett-Packard, and STMicroelectronics. They have a similar philosophy, but target different markets and application needs. The Tensilica Xtensa processor, for instance, offers customization within the

framework of a simple microprocessor core. Customers can specify their own instruction set extension by accessing a Web site and using a high-level language, such as Verilog.

ARC Cores' ARCTangent family targets the digital signal processor markets. Like Tensilica, it allows users to customize both processor features (bus widths, cache sizes, and so on) and instructions. Hewlett-Packard's HP/ST Lx family is aimed at scalable multimedia acceleration using Very Long Instruction Word (VLIW) techniques. It lets customers choose the amount of instruction-level parallelism—in other words, how many functional units to include, and how many operations can be performed in parallel.

Intel's dilemma

So what will happen in the microprocessor market as a whole? It will be a repeat of what has already happened in other technical industries. The trends of customization and speed-to-market will continue to take hold in the lower tiers—in digital signal processing (DSP) and in processors that are embedded within such products as MP3 players, digital cameras, and set-top boxes. Increasingly, sales of stand-alone digital signal-processing and embedded chips will give way to SoCs that incorporate DSP or other functions.

Gradually, over a period of years, the trend will creep upward into higher tiers of the market, including PCs [see [graph](#)]. In fact, companies such as SuperH, MIPS Technologies, and ARM already produce reduced-instruction-set microprocessor cores that can be combined on a single die with other functional units, and that are easily powerful enough to serve as a PC's central processing unit.

Engineers can specify a microprocessor and in some cases completely design one in weeks, rather than months

For high-end companies such as Intel, the dilemma will be that their best and most profitable customers will continue to need exactly the sort of general-purpose, leading-edge processors that Intel is so good at designing and delivering. Nevertheless, broad trends in electronics suggest that growth will increasingly stem from applications in the lower tiers.

For example, sales of PCs, the largest market for high-end microprocessors, declined in 2001 for the first time since 1985, according to market research firm Gartner Dataquest (Stamford, Conn.). Meanwhile, although the market for DSPs shrank even more than the rest of the microprocessor category last year, on the whole they have been one of the fastest-growing segments of that category. The growth has been fueled by such up-and-coming sectors as wireless communications, handhelds, and game, music, video, and other entertainment systems.



Custom Chips and Future Fabs

As custom-configured chips carve out a larger portion of the microprocessor market, major changes are in store for the way chip fabrication facilities operate. To understand why, consider the underlying trends. One is the ever-increasing abundance of

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transistors on a chip, which portends a day when the number of chips per product will approach one. Also every chip will, to some degree, be custom-tailored to its application, and product life cycles will be short. Put those trends together and the inescapable conclusion is that, in the foreseeable future, chips will have to be made in a few days, not weeks, as they are now.

To achieve that kind of speed, chip fab plants must operate differently, starting with the way wafers flow through them. The dies are fabricated in batches on round semiconductor wafers about the size of a dinner plate [photo].

In today's fabs, those wafers are also produced in batches. Therein lies the problem: as these groups of wafers move from station to station in the fab plant, they have to wait until an entire batch is complete before going on to the next station. At any given time, large amounts of valuable product are simply sitting around the plant.

In the future, wafers will need to move rapidly, one by one, through these plants. The difficulty will be finding a way to implement this single-wafer processing in a variety of fabs, all of which have their own proprietary architecture. To our knowledge, no firm as yet has figured out how to do this, though Taiwan Semiconductor Manufacturing Co. (Hsinchu) and United Microelectronics Corp. (Taipei) have made significant progress in this direction.

Single-wafer processing will also probably compromise the economic viability of investments in 300-mm wafer-processing equipment. The reason is that, in the past, when a factory converted to a low-inventory process flow (such as single-wafer processing), the effective capacity of that plant typically doubled.

What this means for fab plants is that it makes more economic sense to convert a 200-mm-wafer fab plant to single-wafer processing, rather than build a 300-mm-wafer fab plant. The 200-mm, single-wafer-processing fab plant could be expected to process twice the number of wafers per month and therefore as many die as the 300-mm fab—and to do so at much lower levels of cost and complexity. —M.J.B. & C.M.C.

Implications, implications

If the microprocessor market does become dominated by multitudes of targeted chips produced in relatively small numbers, several intriguing ramifications could develop. First, for chip-makers, time to market will matter much more [see "[Custom Chips and Future Fabs](#)"]. Second, as firms target smaller, more specific markets, they will differentiate their products more by discovering specific needs that general-purpose products do not address.

This discovery process works better when products are introduced faster and more frequently, in response to feedback from customers. Tensilica, for one, now boasts that new microprocessor cores used within a system-on-a-chip design can be created and their performance tested through simulation in two weeks. The manufacturing latency of most chip fabrication facilities, on the other hand, remains as high as 10 weeks.

With product life cycles approaching a year or less, some microprocessors currently spend a good part of their lives being manufactured. In the future this situation will be competitively intolerable.

In coming years, success in the microprocessor business will increasingly demand that companies:

- Adeptly use modular designs that reuse and recombine silicon intellectual property.
- Include multiple circuit types and possibly process technologies on a single die, creating custom-tailored systems-on-a-chip.
- Shrink the design cycle for microprocessors and systems-on-a-chip and dramatically reduce manufacturing latency times (time spent in the plant).
- Tolerate, both technically and economically, a manufacturing mix composed of a multitude of low-volume runs of narrowly targeted products with short market windows.

The evolution toward this future will not be driven by or grounded in the choices of managers in today's industry-leading companies. Competition in the relevant tiers of the market will *force* these new trajectories of improvement to become critical. The only question is which companies will have developed the capabilities and organizational structures required to thrive in these markets.

The Opportunity and Threat of Disruptive Technologies

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Vol. 27, No.4, pp. 278-282, April 2002

By Clayton M. Christensen

The following article is an edited transcript of the plenary presentation given by Clayton M. Christensen (Harvard Business School) at the 2001 Materials Research Society Fall Meeting on November 26 in Boston.

Introduction

I am honored to be with you today. I will give context for where the puzzles developed that have led to the research that I summarized in the book *The Innovator's Dilemma*. Companies that at one point were very successful and widely admired, fell from their leadership position within 5 to 10 years. This was very poignant for minicomputer companies, for example, which collapsed after their peak in the world economy during the 1970s and 1980s. While the companies were thriving, the business press attributed their successes to the abilities of their management teams. However, when these companies began to quickly unravel around 1988, the business press attributed their failures to the ineptitude of the very same management teams that had been in place during the years of success. I wondered how good managers could get that bad that fast. I was also suspicious because the minicomputer companies fell in unison. It was not just Digital, but also Data General, Prime, Wang, and Nixdorf. While the companies could have colluded on price, they would never have colluded to collapse together. Something more fundamental had to be happening. Oddly, I found that the reason these companies failed, not just in the computer industry, but over and over again in a variety of industries, is that they were well man-aged.

The Disruptive Technology Model

Certain paradigms of good management that we teach in business schools sow the seeds of a company's eventual failure. Figure 1 presents a model that consists of three elements. The performance of a product or service is plotted over time. The first element of the model, represented by the dotted line, shows a trajectory of improvement that customers can absorb or utilize over time. This line represents the median in every market. At the high end of every market, customers are very demanding and will never be satisfied with the best products available. At the low end are customers who can be satisfied by very little.

The steeply sloping lines of the figure represent a distinctly different trajectory of improvement in the market. They represent the improvement that innovators make available as, generation after generation, they introduce new and improved products. This trajectory of technological progress almost invariably outstrips the ability of customers to utilize those improvements; companies can almost always innovate faster than people's lives can change to fully utilize those innovations. In pursuing more profitable customers who will pay higher prices for better products in more demanding tiers of the market, companies are prone to over-shoot what the original set of customers is able to utilize at a later point in time. For example, in the mid-1980s when we were first learning to do word processing on those early personal computers, it was not uncommon for us to have to stop our fingers and wait for the Intel 8088 processor to catch up with us. But as Intel has pursued more profitable customers, to whom it could sell better products for higher margins, it has way-overshot what customers in the mainstream of the business computing market are able to utilize. What this also means is that technology that is initially not good enough to be embraced by customers in the mainstream of the market can improve at such a rapid rate that it later intersects with the needs of mainstream customers.

The third piece of the model relates to “disruptive technology,” which refers to technology that initially underperforms the incumbent technology, but typically offers more flexibility, convenience, or a lower price. A disruptive technology can serve to create new markets, but it can also upset the equilibrium of companies with good reputations and good products.

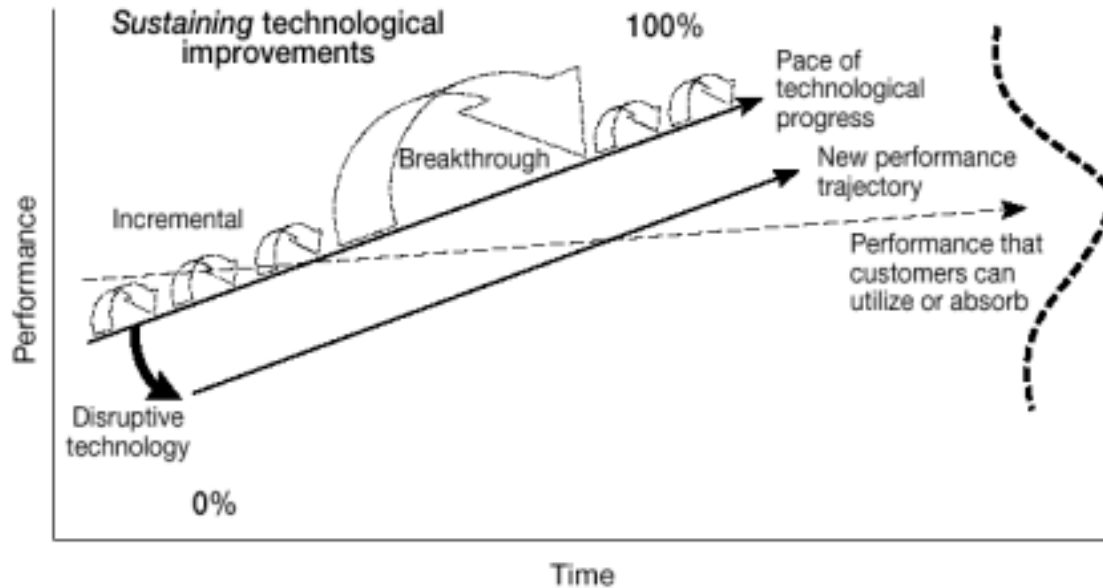


Figure 1. The disruptive technologies model.

Disruption in Action

Computer Industry: From Disk Drives to Intel

I initiated my study of what was driving the failure of some groups of firms with the disk drive industry. In five of six product generations in the industry, a new company would enter and rise to the top only to be toppled by another company that entered and rose to the top. I wanted to develop a model that would explain this phenomenon, which I could then apply to other industries. The disk drive industry served as the “fruit fly” in which it was possible to study many “generations” quickly.

Through this process of the disk drive study, we built a database of every model of every disk drive introduced in the industry’s history. For each of the products, we obtained data on each of the components that was used in the drive. This allowed us to locate the point in the industry’s history at which each new technology was first used. In this process, we identified 116 distinctly different new technologies. Of the 116, 111 sustained the trajectory of performance improvement in the market, as it existed at the time. In other words, the purpose of the technology was to make a good product better. Of these 111 sustaining technologies, a good number were the simple, year-to-year engineering advancements expected of good companies. Additionally, a remarkable number were truly breakthrough innovations that made good products much better, as depicted in Figure 1. What was interesting is that it did not matter technologically how difficult the innovation was. In all 111 of these cases, 100% of the time, the companies that led the industry in developing and adopting the new technology had been the leaders of the old technology. As long as the innovation enabled the company to make a better product that they

could sell for higher margins to their best customers, it was on management's radar screen and it received the necessary resources to get it done. Only five technologies disrupted this trajectory of improvement, which means they brought to the market a product that was not as good as what historically had been available. Oddly, this type of technology always killed the industry leader; in none of these cases did the company that had been the leader in the prior generation survive into the next generation. Even more remarkable, these were relatively simple technologies.

Let us take this phenomenon to the computer world. When we consider the sequence of minicomputers that Digital introduced into its markets through the 1970s and 1980s, it had a perfect record in developing the technologies—whether incremental or radical—that sustained the trajectory of improvement in many computers. The early personal computers that emerged in the late 1970s and early 1980s—such as the Apple II, marketed to children as a toy—were a disruptive technology. Digital received no signal from the market that the personal computer mattered because none of its customers could use personal computers. They were not good enough. Yet, because the trajectory of technological progress outstrips the ability of customers to use it, what was at one point a toy improved at such a rapid rate that it intersected with the customers' needs of many computer users at a later point in time. A whole population of minicomputer companies was knocked out of the market together as a result of listening to their customers.

When I was consulting recently with the senior management at Intel, the chair, Andy Grove, puzzled over my description of “disruptive technology,” and recast it. He said the concept could be more accurately characterized as trivial technology that disrupts the business model of the leaders, which is what makes it difficult to handle. Grove said that Digital did not have a technology problem, but rather a problem with its business model. For example, a minicomputer is fairly expensive and complex, and it has to be sold directly to the customer. Digital had to make gross margins of about 45% just to be acceptably profitable in the market. The senior management faced proposals to invest in two types of products. Some entailed developing better computers than Digital had ever made before that it could sell for \$200,000 and 60% gross margins by penetrating the market where customers had historically only been able to use mainframes. Other proposals entailed investing in personal computers that could be sold for \$2,000 and, in the very best of years, promised gross margins of only 40% that were headed to 20% rapidly. Grove concluded that the choice management had to make was whether to invest in better products that would serve the best customers, which would improve the company's profit margins, or invest in worse products that their customers could not use and that would erode the company's profit margin.

This is the innovator's dilemma. For 111 of the 116 cases in the disk drive study, these paradigms of good management—listening to your best customers and focusing investments on those innovations that promise the highest profit margins—provided good guidance for the innovators. However, in the five cases in which the disruptive technology came in, those same paradigms of good management paralyzed the leading companies and made it impossible for them to respond.

From Steel Mini-Mills to Sony

Let us look now at a very different industry, which is the steel industry. The question is how did the steel mini-mills in North America grow to account for half of steel production? Most of the world's steel is made in large integrated mills that require billions of dollars to build. Mini-mills,

in contrast, melt scrap in electric-arc furnaces and can make steel of any given quality for a 20%-lower cost than integrated mills.

While it seems that every leading steel company worldwide would take on mini-mill technology because the technology is straightforward and the chance to reduce the costs of making a commodity like steel by 20% is a sure path to profit. Right? But not a single integrated steel company in North America or Europe has yet successfully invested in a mini-mill. Only recently, a few companies in Japan have acquired already successful mini-mills. When mini-mills became technologically viable in the mid-1960s, the quality they could produce was very low. Only the concrete reinforcement-bar market accepted their product because rebar had pretty loose specifications. As the mini-mills attacked the competitive rebar tier of the market, the integrated mills happily dropped out of that tier because their gross margins in that market segment were only about 7%. As the mini-mills expanded their capacity, the integrated mills shut the lines down or reconfigured them to make more profitable products. By about 1975, the mini-mills had driven the integrated mills out of the rebar market. The mini-mills, with a 20% cost advantage, made lots of money making rebar in competition with the high-cost integrated mills. But when they finally succeeded in driving the last integrated mill out of the rebar market in about 1975, the price of rebar collapsed by 20%—because you now had low-cost mini-mills slugging it out with low-cost mini-mills in a commodity market. How could they begin making money again?

They had no option but to turn to products that were larger and of higher quality, which means they attacked the next tier up of the market: angle iron and thicker bar and rod. The mini-mills little by little figured out how to make better products. As they hit the next tier of the market, the integrated mills were happy to get out of it, too, because the 12% margins were still relatively low. As the mini-mills expanded capacity, the integrated mills shut the lines down and reconfigured them to make better products, and while the mini-mills were competing against the high-cost integrated players, the mini-mills once again made a lot of money—until 1984, when they succeeded in driving the last high-cost integrated player out of the angle-iron market. The price of angle iron then dropped by 20%.

The mini-mills then moved up into structural beams. The margin that the integrated mills were making in structural beams was about 18%, and the market was three times larger than those the mini-mills had already conquered. In the early 1980s, beams could not be rolled in a mini-mill format to meet specifications. The mini-mills, however, figured out how to roll high-end structural bar—like 12-, 16-, and 24-inch I-beams and H-beams. By 1992, they had driven U.S. Steel out of the structural-beam market. By 1996, Bethlehem Steel had also been driven out of that market.

Integrated steel companies apparently practiced good management. Every time they lopped off the low end of their product line, their reported gross-profit margins improved. Also, every time the mini-mills added the very same products to the high end of their product lines, their reported gross-profit lines improved. As a perfect symmetry of motivation, the high-end players were motivated to leave the very markets that the low-end players were motivated to enter.

Other companies that recently experienced this kind of phenomenon include Sony, which came into the low end of the market with low-quality transistor pocket radios in the mid-1960s. Sony then became the highest-quality electronics maker worldwide. Seiko started with the inferior plastic digital watch and then became the highest-quality watchmaker worldwide. In the photocopier industry, Canon competed against the huge machines by Xerox with a low-end, limited-feature, tabletop photocopier that filled a niche for small companies. Xerox's customers

could not use the little tabletop machines, but in a different application, the inferior machine moved up-market, and so customers consumed infinitely more photo-copying as a result of that industry having been disrupted.

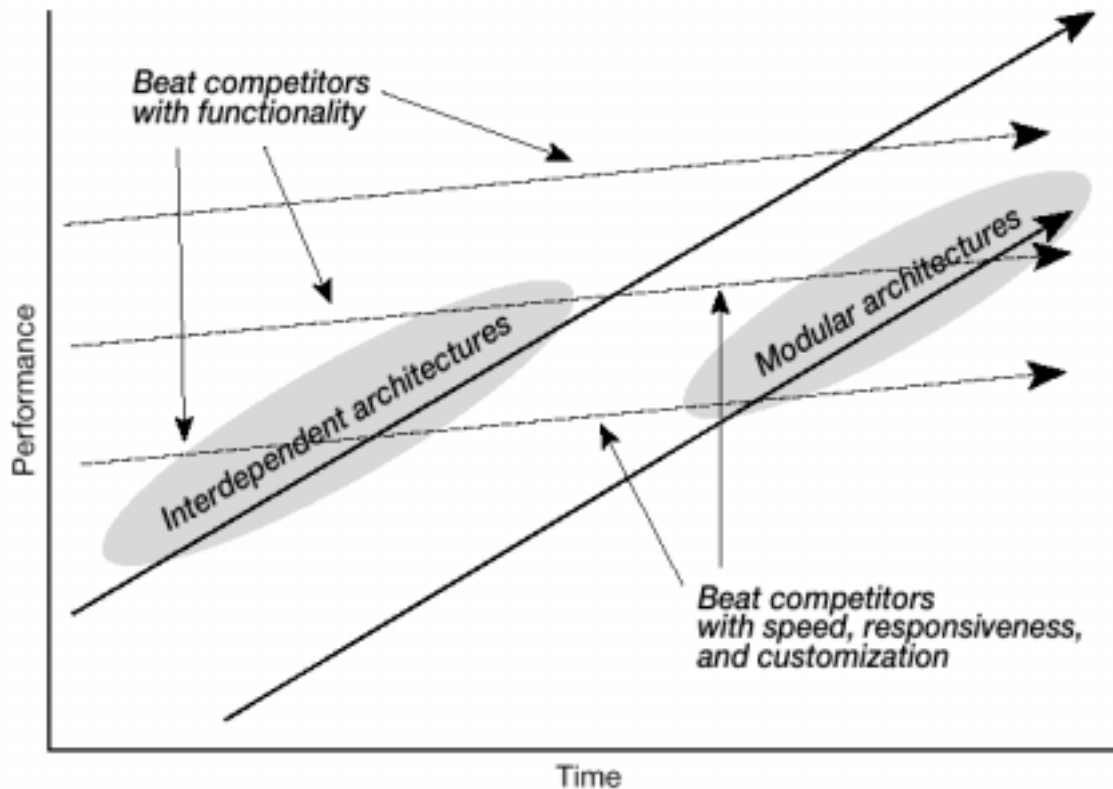


Figure 2. Model of integration and disintegration. Integrated competitors have the advantage when products are not good enough. Focused firms overtake over-served markets.

Interdependent and Modular Architectures

In the early 1980s, IBM had better operating-system technology than did Microsoft and better microprocessor technology than did Intel. Yet, IBM chose to outsource the two technologies to those respective companies and in the process put into business the two companies that subsequently dominated the computer industry. IBM designed and assembled computers, where subsequently no money was made. At the time IBM made those decisions, it was generally regarded as managerially astute. This experience brings up the question as to when to work in-house and when to outsource.

A review of the history of most industries reveals that during the early period when the performance of the product or service is not good enough for what customers in the mainstream need, the architecture of the product tends to be proprietary and interdependent in character. When the product is not good enough, competition is dictated through the development of better products. A proprietary architecture in an interdependent architecture can come closest to the frontier of what is technologically possible.

For example, in the early years of the mainframe computer industry, an independent supplier of operating systems, core memory, or logic circuitry could not have existed because the design of

each of those subsystems depended upon the design of each of the other subsystems. They had to be interdependently architected. An independent-contract manufacturer of mainframe computers could not exist because the way mainframe computers were made depended upon the way that they were designed, and the way that they were designed depended upon the way they were going to be made.

Integrated companies initially tend to dominate their industries, such as IBM and Digital, General Motors and Ford, Alcoa and U.S. Steel, and so on. In the most demanding tiers of these markets, the architectures are still proprietary and interdependent.

Once the technology is more than good enough, how do companies compete for the business of customers in the now-overserved tiers of the market? The answer is that they must compete differently. They must compete in speed-to-market, in the ability to respond quickly to changes in customers' needs, and in the ability to customize the features and functions of products to the needs of smaller and smaller niches of the market. To be fast, flexible, and responsive, the architecture of the product has to evolve from an interdependent to a modular architecture—ultimately one that is built around industry standards. When that happens, the industry tends to disintegrate as specialized companies provide one piece of value added. The population of specialized companies then tends to replace what were the dominant integrated companies in an earlier era. In fact, the dominant integrated companies have to disintegrate in order to compete effectively (see Figure 2).

In an interdependent architecture, the companies that make the money tend to be the ones that do the design and assembly of the product that is not good enough, that is, the end-use product. Companies that were the suppliers tended to live a miserable profit-free existence year after year. So, for example, IBM, with 70% market share, made 95% of the industry's profit. The companies that supplied IBM were wrung out every quarter. General Motors had a 55% market share; it made 80% of the industry's profit. A supplier to General Motors made no money. In a modular architecture, the company that makes the money tends to flip; those that design and assemble the products that the customers use tend to get knocked out and the ones that make money are the ones who supply the subsystems that are not yet good enough. To illustrate, if an engineer at Compaq receives instructions to design a better computer than Dell, the engineer could put in a faster microprocessor, a higher-capacity disk drive, or more dynamic random-access memory. However, anything the engineer does, the competitor can instantly copy. So the ability to make money flips back to Intel, who provides the performance-generating sub-system that the assembler has to use in order to keep it at their leading edge.

I recently published an article in Harvard Business Review called "Skate to Where the Money Will Be." The title refers to what made Wayne Gretzky a great hockey player: He skated not to where the puck was, but to where it would be. It suggests that the place in the value-added chain where the money is made at one point is not likely where it will be made in the future. This is happening now in the car industry. Automobiles have become more than good enough for what most consumers need. The industry used to take six years to design and deliver a new car. Now the design cycle has converged on two years, and customers can order a car custom-assembled to their specifications, deliverable in five days. Car companies accomplish this by shifting the architecture of these cars from an interdependent to a modular architecture. They still procure subsystems from tier 1 suppliers. Inside the subsystems, just like an Intel chip set, the architecture becomes more proprietary and interdependent even while the external interfaces of these sub-systems become more standardized. True to form, the integrated car companies are

having to disintegrate to keep pace with the accelerating cycles. However, car companies like GM sold off the pieces of their business where the money was subsequently likely to be made, as did IBM. In other words, GM and IBM skated to where the money used to be rather than to where the money would be in the future.

Modularity in Action: Advanced Materials

In many ways, the purpose of advanced materials is to be a sustaining technology; that is, to improve the functionality of products at the leading edge. When we started our company, Ceramics Process Systems, in the early 1980s, we introduced components made of ceramics to the automobile-engine world. One of the leading European car companies that was a major investor in our company wanted us to start making silicon nitride piston pins because the calculations revealed that if we could reduce the mass of a reciprocating component in the engine, it would greatly improve its efficiency. With the specifications they gave us, we molded the piston pins. When the piston pins were tested, however, the test engine vibrated uncontrollably. It turned out that over the years, the car company had distributed mass elsewhere in the engine to compensate for the reciprocating mass of the steel. There simply was not a plug-compatible modular opportunity for the advanced materials at the edge of performance. Everything depended upon everything else. We succeeded because we finally found applications where the technology was almost plug-compatible, which is far away from the leading edge.

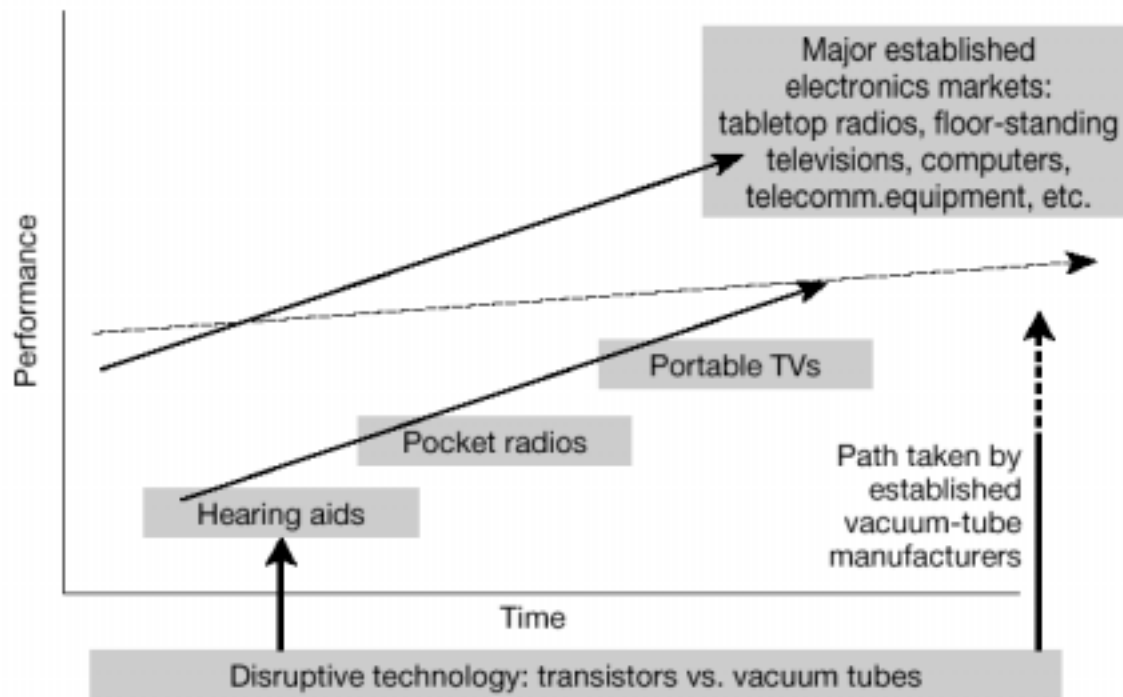


Figure 3. Successful disruptors target smaller, "green space" markets instead of stretching toward existing, larger markets.

How Disruptive Growth Begins

The way disruptive technology makes it to the obvious existing market is by finding simple applications and then gradually improving. Figure 3 shows that the transistor was a disruptive technology relative to the vacuum tube because it could not handle the power in the early 1950s that would be required to be used in the market as it existed at the time. Every one of the

vacuum-tube players saw the transistor coming. They all worked on the technology in their laboratories, but framed it as a technological deficiency instead of an opportunity. In other words, companies were challenged with ways of making transistors good enough to be used in the vacuum-tube market. But transistors took root in a completely new market. The first application was for a small hearing aid in 1953. This application valued the transistor for the very attributes that made it useless in the mainstream market. In 1955, Sony introduced its pocket transistor radio, which was such a low-quality product that it did not compete in the mainstream market. It only appealed to low-end customers, such as teenagers, because it enabled them to do something that had not been possible before—such as listen to Elvis Presley out of earshot of their parents. Sony moved up into portable televisions in about 1959, where a huge new market emerged. Other companies felt no pain because the market served different kinds of customers and different kinds of applications than their mainstream ones. By about the mid-1960s, solid-state electronic components became good enough for the mainstream market to use and they replaced vacuum tubes. This is a typical pattern. A disruptive technology first takes root away from the core of the mainstream market, where users value it despite its limitations.

In the last chapter of *The Innovator's Dilemma*, I discuss whether the electric vehicle and photovoltaics are disruptive technologies. In its concern over air pollution, California passed legislation in the early 1990s mandating that every car company had to sell 2% of its unit volume in California as zero-emission or electric vehicles by 1998, or it would be prohibited from selling gas-powered cars. The car companies worldwide mounted huge efforts to develop battery technology for electric vehicles. By about 1996, it was clear that they would not accomplish this goal. As an example, customers told Chrysler that they wanted a car that went about 250 miles between refueling stops and could accelerate from 0 to 60 miles per hour in about nine seconds. To get its electric minivan to accelerate fast and cruise far, Chrysler loaded 1600 pounds of lead-acid batteries in the back of it. The minivan, however, only cruised 80 miles and accelerated from 0 to 60 mph in 19 seconds. One unfortunate side effect was that it took four times longer to stop this minivan than the gas-powered one because of the balance in the back. It retailed for \$100,000. During consumer tests, Chrysler found no buyer. The government has since pushed the target date to 2003.

In studying this issue, one of my students determined that the industry is coming up with the right answers to the wrong questions. The right question is locating the market in which a customer wants a car that does not cruise far or accelerate quickly, and is cheap. He suggested that parents of teenagers and people in the retirement community would prefer this type of car. He suggested that people in places like Bangkok would find a use for such a car since they cannot travel quickly on the streets and would actually prefer the car to shut down as they waited in traffic.

Another limitation to forcing electric vehicles into existing applications is that they do not fit with existing infrastructures. If plugged into a 100-V outlet, they take an unacceptable eight hours to recharge. Even if every filling station was outfitted with 480 V of service, the car would take 20 minutes to refuel; consumers will not want to wait that long. On the other hand, if the electric vehicle were a neighborhood vehicle, designed not to drive on a freeway, but for teenagers to cruise around town to high school and see their friends, or for retirement communities, it would be convenient for the users to plug in the vehicle overnight. By targeting such a market, the car companies avoid the infrastructural interdependencies that happen when they stretch to use the new technology in the most demanding applications. Over and over again, though, leading companies try to force the technology into demanding, obvious, and large applications.

One of the most personally rewarding things about *The Innovator's Dilemma* has been the number of people who have told me that our theory has helped them frame a problem that they are facing in their own lives. I hope that you too will be able to use our frameworks to better understand some of the issues that your company may be confronting.

Recommended Reading

1. C.M. Christensen, *The Innovator's Dilemma: When New Technologies Cause Great Firms to Fail* (Harvard Business School Press, Boston, 1997).
2. C.M. Christensen, M. Raynor, and M. Verlinden, "Skate to Where the Money Will Be," *Harvard Business Review* (November 2001), p. 72.
3. C.M. Christensen, T. Craig, and S. Hart, "The Great Disruption," *Foreign Affairs* 80 (2) (March/April 2001), p. 80.
4. C.M. Christensen and M. Overdorf, "Meeting the Challenge of Disruptive Change," *Harvard Business Review* 78 (2) (March–April 2000), p. 66.

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Sobre El Sentido Del Progreso En La Teoria De Estabilidad Elastica

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RESUMEN

En este trabajo se propone un acercamiento epistemológico a la teoría de estabilidad elástica, considerando el pasaje de la teoría clásica a la teoría de estabilidad poscrítica inicial. Se caracteriza la teoría clásica mediante el estado del arte presentado por Timoshenko en 1936, y la crisis conceptual que surge alrededor de la incompatibilidad entre resultados teóricos y experimentales para el cilindro con carga axial. Se revisan los estudios de la etapa de transición (1934- 1968) que ofrecieron una variedad de explicaciones incompletas o inadecuadas. Se presenta el surgimiento de la nueva teoría propuesta por Koiter en su tesis doctoral. A continuación se consideran dos posturas epistemológicas para interpretar el cambio de teoría: la primera, debida a Kuhn, considera las teorías como paradigmas y el cambio como revolucionario; mientras que la segunda, debida a Lakatos, considera la estructura de programas de investigación. Finalmente se analizan las limitaciones y aciertos de cada una de esas visiones a la luz del cambio en la teoría de estabilidad.

INTRODUCCION

A partir de la década del 60 varios filósofos de la ciencia han estudiado las teorías científicas como estructuras y se han interesado en cómo se produce el cambio teórico en la ciencia. Entre las posturas más reconocidas se encuentra la de Thomas S. Kuhn (1922-1996), centrada en el concepto de paradigma y de cambio revolucionario (Kuhn, 1962); la de Imre Lakatos (1922-1974) sobre programas de investigación científica (Lakatos, 1978), y la de Larry Laudan sobre tradiciones y utilidad como forma de explicar el cambio (Laudan, 1977). Alrededor de esta problemática se mueve la racionalidad o irracionalidad que tienen los científicos para aceptar un cambio de teoría y cómo es la estructura del cambio que se produce entre una teoría y otra que le sucede.

¿En qué medida esos modelos de cambio son adecuados para todas las disciplinas y todas las épocas? Según Kuhn, se trata de patrones universales, aunque los casos que sirvieron de estudio a Kuhn fueron tomados de la física y la astronomía en el pasaje de la Edad Media al Renacimiento o en los siglos XVII y XVIII. Han habido fuertes cuestionamientos a la interpretación de Kuhn, por ejemplo desde la biología (Mayr 1995). Como Mayr señala, los filósofos de la ciencia ponen demasiado énfasis en descubrimientos en desmedro de cambios en los conceptos.

Sin embargo, la formación de ingenieros durante su etapa de estudios doctorales se basa en el aprendizaje de los aportes mencionados de la filosofía de la ciencia, y se pretende que el aprendiz extraiga sus propias conclusiones sobre su campo de interés. Allí es donde se nota la falta de estudios específicos que se refieran a teorías construidas en las ciencias de la ingeniería. La motivación de este estudio ha sido proveer un estudio de caso sobre el cambio teórico dentro de la mecánica aplicada, en particular referido a la teoría de estabilidad de sólidos elásticos.

La historia de la teoría de estabilidad en el siglo XX representa una situación interesante sobre la contraposición entre buscar una "cosa" (la carga crítica de pandeo) versus la búsqueda de un "proceso" (el pasaje de estados críticos a poscríticos). En este trabajo revisaremos primero la

teoría clásica de estabilidad estructural y como se produce una crisis conceptual. Describiremos el período de transición, en el que se buscaron diversas explicaciones a las discrepancias entre teoría y experimentos, y la nueva teoría basada en la estabilidad poscrítica inicial. Se verá el proceso de cambio desde la óptica de dos posturas de la epistemología: la de paradigmas de Kuhn y la de programas de investigación de Lakatos.

El iniciador de la nueva teoría de estabilidad fue Koiter, quien visitó Brasil en dos oportunidades, en 1983, para dictar un curso de teoría de cáscaras en el LNCC/CNPq, y en 1985, cuando participó en una reunión de IUTAM y dictó un curso de estabilidad.

LA TEORIA CLASICA

Las características de la teoría clásica

Denominaremos teoría clásica de estabilidad estructural a la vigente en la primera mitad del Siglo XX, en la cual se cuantificaba una carga de pandeo (o carga crítica, que es un valor escalar) para evaluar la inestabilidad de una estructura.

A los efectos de llevar a cabo el análisis, la respuesta se computaba a partir de la configuración descargada y hasta llegar a la carga crítica, que constituía un límite de estabilidad. Era necesario imponer un modo de deformación adicional (un autovector) para generar la condición de equilibrio en un estado de equilibrio adyacente. Matemáticamente, la evaluación de la carga crítica generaba un problema de autovalores para autovectores supuestos en base a consideraciones físicas. Un buen estado del arte de la teoría clásica fue escrito por Timoshenko (1936), más adelante actualizado como Timoshenko y Gere (1961).

El éxito de esta teoría estuvo centrado alrededor de la modelación de un caso inicial: la columna bajo carga axial, para la cual se verificaba experimentalmente que el procedimiento funcionaba. Debe recordarse que las estructuras prácticas que se construían en esa época (hasta inicios del siglo XX) se hacían mayormente con estructuras de barras.

Este procedimiento teórico pudo extenderse usando la misma metodología a otras formas estructurales, incluyendo placas. La metodología de análisis podía llevarse a cabo por dos vías: en la formulación diferencial se debe

“... suponer que la placa sufre un ligero pandeo bajo la acción de fuerzas aplicadas en su plano medio, y calcular luego las intensidades que precisan esas fuerzas para mantener la placa en esa ligera forma de pandeo. La ecuación diferencial de la superficie elástica se obtiene en ese caso ... suponiendo que allí no hay carga lateral.” (Timoshenko, 1936, pp. 353).

Otra vía es usando un método energético, en el cual

“... suponemos que la placa, sometida a tensiones por fuerzas que se ejercen en el plano medio, experimenta alguna pequeña flexión lateral compatible con las condiciones de contorno dadas. Esta débil flexión puede originarse sin extensión de ese plano medio y solamente necesitamos considerar la energía de flexión y el trabajo correspondiente efectuado por las fuerzas que actúan en la nombrada superficie media. Si el trabajo realizado por estas fuerzas es menor que el trabajo de deformación por flexión, para cada

forma posible de pandeo lateral, la forma plana de equilibrio de la placa es estable. Si el mismo trabajo llega a ser mayor que la energía de flexión para cualquier forma de deformación lateral, la placa queda en situación inestable y acontece el pandeo” (Timoshenko, 1936, pp. 354).

Adicionalmente, la metodología se podía también llevar a cáscaras delgadas, para las cuales no existía aún un campo de aplicación fértil, pero que era de interés mayormente teórico. Por ejemplo, R. Zoelly estudió en su tesis doctoral de 1915 el problema de esferas de pared delgada dentro del marco de la teoría clásica, llegando a expresiones sencillas que podían calcularse a mano para modos simétricos. Timoshenko no encontró objeciones a esa formulación en 1936:

“Hasta ahora hemos considerado únicamente el pandeo simétrico de la lamina pero un estudio mas general prueba que debido a la simétrica de la lamina esférica comprimida uniformemente, respecto de cualquier diámetro, la ecuación ... deducida de la hipótesis de simetría ... puede utilizarse siempre en el cálculo de la tensión crítica.” (Timoshenko, 1936, pp. 535).

También se lograron expresiones para el cilindro con carga axial y para el cilindro con presión lateral. La gran ventaja de la formulación clásica estaba asociada a su simplicidad, compatible con los escasos recursos de cálculo disponibles en esa época. Podría pensarse que el progreso de los recursos de cálculo se tradujeron en el progreso teórico en este campo. Sin embargo, notaremos que no fue el ingreso de la computadora sino el cambio conceptual el que posibilitó la gestación de un nuevo enfoque a la problemática.

La evidencia experimental adversa a la teoría clásica

¿Porqué el enfoque clásico comenzó a mirarse críticamente? El viraje de apreciación estuvo claramente asociado a la evidencia contraria que se encontró para un caso considerado importante en su época: el cilindro bajo carga axial.

Si se calculaba la carga critica siguiendo el procedimiento clasico, para un modo de pandeo con m semiondas en direccion axial y n ondas en direccion circunferencial (representativo de borde simplemente apoyado) se obtenia una formula muy simple para la tension critica σ_{cr} , dada por

$$\sigma_{cr} = E h / \{ \sqrt{3(1-\nu)} \}$$

donde a es el radio del cilindro y h el espesor, E el modulo de elasticidad y ν el coeficiente de Poison. El conflicto surgio porque estos σ_{cr} son valores mas altos que los que se median en experimentos de laboratorio sobre modelos en escala reducida, para cilindros de duraluminio, de acero y de bronce:

“En todos los casos, la falla ocurrió a una tensión mucho menor que la que predice la teoría. En ningún caso la tensión última fue más del 60 % de la teórica” (Timoshenko, 1936, pp. 497-498).

La Figura 243 de Timoshenko (1932) muestra en abscisas la relación entre la carga teórica clásica calculada y la obtenida en experimentos. En realidad, en muchos casos se obtenían valores del orden del 15 al 30% del valor que predecía la teoría clásica.

“Se observa claramente que la razón de la tensión de ruptura a la teórica decrece cuando la razón a/h aumenta, es decir que la discrepancia entre el experimento y la teoría es mayor en las láminas más delgadas” (Timoshenko, 1936, pp. 498).

Este estado de conocimientos generó una gran desconfianza hacia la teoría clásica como forma de evaluar una carga crítica.

A pesar de que el caso disparador de la crisis fue el cilindro con carga axial, sin embargo la esfera bajo presión uniforme ofrecía aun mayores dificultades. Siguiendo la teoría clásica, la carga crítica de la esfera era aun mas inadecuada que la del cilindro, pero esto no resulto evidente sino hasta tiempo después. Las razones asociadas a que la esfera no fuera tomada como un caso critico pueden ser dos: (i) no habían experimentos confiables sobre esferas, mucho mas complicados que los del cilindro; y (ii) no había urgencias planteadas desde la industria aeronáutica para evaluar inestabilidad de esferas.

Otra de las deficiencias de la teoría clásica fue la falta de información sobre la trayectoria poscrítica:

“La profesión de ingeniería estuvo satisfecha por mucho tiempo con el conocimiento del límite de estabilidad... Se tomó la posición que, por seguridad, la carga en una estructura debe siempre mantenerse por debajo de ese limite, haciendo que fuera superfluo investigar que puede ocurrir por encima de ese límite” (Koiter, 1945, pp. 3).

Esta situación cambió con la necesidad de un mejor aprovechamiento estructural del material:

“En la tecnología moderna, más específicamente en el diseño de aviones en los que la economía de peso es de gran importancia, esta excedencia de carga límite está ciertamente permitida!” (Koiter, 1945, pp. 4).

De modo que existía la necesidad de evaluar el comportamiento poscrítico para aprovechar mejor la capacidad portante de placas, y sin embargo no se contaban con maneras confiables de computarla.

Nótese que de estos tres problemas señalados (cilindro, esfera y placa) sólo el primero produjo una crisis en la teoría clásica como para ponerla en jaque.

El rechazo a la teoría y el retorno a la experimentación

Como respuesta operativa ante la falta de acuerdo en cilindro con carga axial, se llevaron a cabo gran cantidad de experimentos de laboratorio con diferentes niveles de cuidado. Los pioneros fueron Robertson en 1928, Lundqvist en 1933 y Donnell en 1934, entre otros. Se observó gran dispersión de los resultados de las cargas máximas que podía alcanzar una cáscara cilíndrica, pero se observaba una clara tendencia a mayores diferencias entre teoría y cálculo para valores grandes de la relación a/h (radio/espesor de la cáscara). Los estudios

“... mostraron también que las investigaciones teóricas desarrolladas para una cáscara cilíndrica ideal pueden ser usadas sólo como una guía para desarrollar formulas empíricas basadas en numerosos experimentos” (Timoshenko y Gere, 1961, pp. 473).

Esa desconfianza se extendió a todo modelo teórico en este campo, y la forma de cálculo pasó a basarse en formulas empíricas obtenidas a partir de los experimentos disponibles. En lugar de comprender más el fenómeno, los ingenieros se dedicaron a describirlo en más detalle dentro de los rangos de interés industrial.

LA TRANSICION: CUESTIONAMIENTOS Y EXPLICACIONES EN EL MARCO DE LA TEORIA CLASICA

Aproximadamente entre 1934 y 1968 hubo intentos de explicar las razones de la discrepancia entre la teoría clásica y los experimentos. Timoshenko, en la primera edición de su libro, reporta la siguiente explicación, que le parece satisfactoria:

“Para explicar este desacuerdo, L. H. Donnell anticipó una teoría que tiene en cuenta los desplazamientos iniciales de la superficie cilíndrica ideal y analizó la flexión de la lamina debida a esta imperfección inicial, suponiendo que las flechas no son pequeñas. Admitió también que falla cuando comienza la fluencia del material. Tomando los desplazamientos iniciales en forma de ondas e igual longitud en las direcciones axial y circular, combinadas con ondas de pandeo simétrico con respecto del eje, Donnell encontró que la carga de falla ... se puede expresar en función de la razón radio espesor a/h ... Se advierte que existe un acuerdo satisfactorio entre estas curvas y los resultados de los ensayos” (Timoshenko, 1936, pp. 498-499).

En la segunda edición de su libro, Timoshenko ya no es tan optimista con respecto a la explicación anterior e incluye más explicaciones alternativas:

“Una investigación adicional para las discrepancias entre teoría y experimentos fue realizada [por von Karman y Tsien, 1941] estudiando el comportamiento postcrítico de cáscaras cilíndricas ideales en compresión... Suponiendo pandeo y tomando una expresión para [el desplazamiento] w con varios parámetros... Vemos que para originar pandeo en un caso ideal debemos tener una tensión de compresión dada por la ecuación [clásica], pero con un aumento de la deflexión la carga requerida para mantener la cáscara cilíndrica en la condición pandeada rápidamente disminuye y se acerca a cerca de un tercio de la carga de pandeo requerida teóricamente” ... “Por eso la flexión comienza cuando la carga es pequeña y las deflexiones alcanzan los valores a los cuales la continuación del pandeo requiere de una carga mucho menor que la teórica para el caso ideal” (Timoshenko y Gere, 1961, pp. 472-473).

Hay también otra explicación disponible de 1950:

“En un siguiente estudio [Donnell y Wang, 1950] de la influencia de imperfecciones en el proceso de pandeo se supuso que la cáscara cilíndrica tiene inicialmente ondas ... Se ve que la imperfección más pequeña reduce considerablemente la carga máxima que puede tomar la cáscara cilíndrica en compresión” (Timoshenko y Gere, 1961, pp. 473).

Esos intentos trataban de corregir o mejorar la formulación analítica clásica. En resumen, podemos señalar las siguientes explicaciones sobre la discrepancia, la mayoría de las cuales fueron consideradas por Donnell (1976) y Esslinger y Geier (1975):

(a) La teoría clásica de laminas empleada en los modelos no era lo suficientemente exacta. Sin embargo, se puede demostrar que la influencia de los términos despreciados en la formulación es pequeña comparada con las grandes discrepancias presentes entre experimentos y teoría, de manera que esa explicación debió desecharse.

(b) No se había considerado la trayectoria poscrítica del cilindro, que cae después del estado crítico. Esta explicación de B. Almroth (1963) llevó a evaluar la trayectoria poscrítica e intentar identificar su punto más bajo como valor de carga de pandeo. En principio se consideraba el modo crítico y se investigaba la trayectoria poscrítica con deformaciones en ese modo.

“La sensación de satisfacción en el progreso hacia un límite inferior de cargas poscríticas llegó a un final abrupto cuando Hoff [Hoff, Madsen, y Mayers, 1966] ... realizó la observación que parece que esos cálculos basados en muchos términos de series darán en definitiva una carga poscrítica nula” (Esslinger y Geier, 1975, pp. 110).

Eso contradecía la evidencia que la carga crítica no era nula, de modo que esto hizo abandonar esa explicación.

(c) Se habían tratado los números de ondas del modo de pandeo supuesto como variables continuas en lugar de considerárselas como variables enteras. Las investigaciones basadas en números enteros de ondas (Hoff, Madsen, y Mayers, 1966) mostraban que sobre la trayectoria poscrítica las cargas no llegaban a cero, y los valores mínimos tenían una cierta concordancia con los experimentos;

“... sin embargo, la teoría producía un número de curvas y no había criterio para seleccionar la curva que aparecería en el cilindro real para un largo determinado” (Esslinger y Geier, 1975, pp. 112).

(d) Las condiciones de contorno analíticas no se satisfacían en los experimentos y viceversa. Esta explicación fue sostenida por J. Singer durante algún tiempo, pero fue abandonada cuando surgió la teoría de Koiter con fuerza en la década del 70.

“Despreciar la influencia de los bordes... está bien justificada por los propios ensayos, en los cuales no ocurre una dependencia del fenómeno de pandeo con el algo del cilindro o con las condiciones de borde. Sin embargo, esto también es aceptable en forma teórica” (Koiter, 1945, pp. 185).

(e) No se había considerado la posibilidad de que haya un salto de un estado de equilibrio a otro entre una posición precrítica y una poscrítica. Esta explicación se debe a T. von Karman y H. S. Tsien (1941).

“Debido a que los desplazamientos supuestos por von Karman y Tsien no pueden reproducir la solución general en la carga crítica, sus resultados para cargas que no se desvíen excesivamente de la carga de pandeo presumiblemente no son tan buenos” (Koiter, 1945, pp. 177).

(f) No se habían considerado imperfecciones en la respuesta, que pueden ser la causa de una caída en la carga teórica de pandeo. El principal estudioso de esta explicación fue Donnell, como vimos antes.

La teoría clásica coexistió con estas explicaciones durante algún tiempo, y se generaron expectativas de solucionar las dificultades de la teoría clásica mediante extensiones. Las evidencias en contra de las explicaciones (a-e) ofrecidas por diversos autores demostraron que no se podían sostener; sin embargo continuaron vigentes por algún tiempo hasta que finalmente se desvanecieron en la literatura.

¿Porqué se aceptaron esas explicaciones inadecuadas en su momento? Donnell afirma que se aceptaban por su conveniencia:

“Esas explicaciones traían beneficios porque les daban a los diseñadores valores aceptables con el respaldo de una explicación teórica, especialmente (d-e), a pesar de que: (i) los valores encontrados mediante el uso de esas explicaciones no se correspondían con los factores reales; (ii) no tenían una justificación real; y (iii) producían valores demasiado conservadores en algunos casos” ... “Sin ese respaldo los ingenieros hubieran tenido que enfrentar la horrorosa verdad que sus construcciones tenían diversos defectos que afectaban significativamente su capacidad resistente... Pero los ingenieros deben vivir y trabajar en un mundo real...” (Donnell, 1976, pp. 394).

Sin conocer la tesis de Koiter, Donnell postuló que (f) era la causa de las discrepancias entre teoría clásica y experimentos. Esta explicación

“... fue cuestionada por Cox (1940) y por von Karman y Tsien (1941) quienes notaron que esas desviaciones iniciales deberían ser varias veces mayores que el espesor de la pared para explicar los valores experimentales bajos; pero esas desviaciones no podrían haber escapado a que las notaran otros investigadores ... Además, Cox y von Karman y Tsien señalaron que la explicación de Flugge-Donnell requeriría la evolución gradual del pandeo al aumentar la carga, mientras que los ensayos mostraban un pandeo abrupto y casi explosivo; tampoco se podían explicar de manera satisfactoria la gran dispersión de los puntos de los ensayos” (Koiter, 1945, pp. 4).

Había además una explicación extendida cualitativa que servía para tomar en cuenta los casos que fallaban en la teoría clásica y también los que lo confirmaban (la columna) (Donnell, 1976, pp. 395).

Ninguno de los trabajos elaborados durante el periodo de transición producían explicaciones satisfactorias por si mismos o combinados, y por ello no lograron establecer una nueva base explicativa con un desarrollo teórico aceptable por la comunidad científica en general. Habían intentos de corregir errores individuales, pero sin reconstruir la teoría clásica. En realidad, no se trataba de aferrarse a una teoría antigua (como postula Kuhn), sino que nadie sabía como desarrollar una nueva.

El papel que cumplió la transición fue fundamental: (i) sin ella no habría sido posible recombinar ideas viejas y sumarles nuevas; (ii) prepararon el campo para generar nuevos términos; (iii) descartaron posibilidades, que de otra manera hubieran permanecido latentes como explicaciones alternativas; y (iv) se estableció claramente que había un vacío en el conocimiento, a pesar del trabajo intenso de los expertos de la época.

LA TEORIA DE ESTABILIDAD DE LA REGION POSCRITICA INICIAL

Las características de la nueva teoría

Warner T. Koiter (1914-1997) escribió su tesis doctoral en Holanda en 1945, pero recién en la década del 60 se difundieron sus trabajos en Estados Unidos y Europa. Koiter señala:

“De los ejemplos anteriores es claro que las teorías de estabilidad formuladas en el pasado son insuficientes. Deben ser suplementadas de tal manera que aun el comportamiento de estructuras diferentes, que difieren en la vecindad de la carga de pandeo teórica, pueda ser descripta” (Koiter, 1945, pp. 4).

Las contribuciones de Koiter constituyen una nueva teoría, desarrollada desde el inicio de manera sistemática y consistente, con bases matemáticas sólidas y aproximaciones ingenieriles que se hacen explícitas.

Algunos aspectos salientes de esa teoría son los siguientes:

- (a) La formulación está basada en la energía potencial total del sistema estructural, en lugar de usar consideraciones de equilibrio directo. Por esa vía se rescataban la trayectoria de equilibrio precrítica y la carga crítica clásica.
- (b) Koiter requería de evaluar la trayectoria poscrítica en sus etapas iniciales (a diferencia de otros estudios de la etapa de transición que precisaban la totalidad de la trayectoria para trabajar).
- (c) Matemáticamente, la nueva teoría requiere de un desarrollo en serie de Taylor (o de un análisis de perturbaciones). También o de un análisis de autovalores y autovectores.
- (d) Permitió confirmar las sospechas sobre la importancia de las imperfecciones en la estabilidad del sistema.

“No sólo que el valor de la carga critica esta influenciado significativamente por las desviaciones de la estructura con respecto al modelo, sino que la naturaleza del fenómeno de pandeo es completamente diferente en la estructura y en el modelo” (Koiter, 1945, pp. 92).
- (e) Se generó el término “sensibilidad ante imperfecciones”.
- (f) Demostró cómo se produce la interacción de modos acoplados en presencia de imperfecciones pequeñas en la estructura.

Los éxitos de esta teoría están asociados a dos aspectos centrales: Primero, las predicciones de la nueva teoría se verificaban experimentalmente frente a experimentos realizados antes o con experimentos nuevos. J. Roorda llevó a cabo experimentos en 1964 para probar los resultados teóricos de Koiter (Roorda, 1964). En segundo lugar, la nueva teoría lograba explicar y modelar matemáticamente el comportamiento de casos difíciles (como el cilindro y la esfera) y también de casos sencillos (la columna). A pesar de que la nueva formulación era complicada, sin embargo podía usarse sin necesidad de contar con una capacidad de cómputo importante. Koiter mismo nunca usó computadoras en sus trabajos, y las extensiones para computar su teoría con

elementos finitos ha sido mucho más recientes. Tercero, es posible generalizar la teoría de manera de llevarla a diversos casos estructurales.

Las ventajas de la teoría incluye el proveer curvas correctas poscríticas en las etapas iniciales, para desplazamientos menores al espesor de la cáscara, para lo cual se requiere de poco esfuerzo computacional. Además la teoría provee criterios de estabilidad de la estructura en cualquier configuración de carga, incluyendo la carga crítica. Esto posibilita clasificar los posibles comportamientos de acuerdo a las trayectorias poscríticas que se generan, o de acuerdo a indicadores basados en la energía del sistema.

La teoría proveyó una clasificación de los posibles comportamientos esperables en cualquier estructura. El análisis proveyó los casos de bifurcación estable, inestable, asimétrica y punto de inflexión, los cuales eran todo lo que le podía ocurrir a un sistema estructural al llegar al estado crítico.

Conceptos previos mantenidos en la teoría de Koiter

Consideremos primero los siguientes interrogantes:

- ❑ ¿Qué quedó de la teoría clásica? En la teoría de Koiter quedaron la carga crítica (como referente y caso “ideal”, al que no se puede llegar en la práctica) y el modo crítico, que eran parte de la teoría clásica.
- ❑ ¿Qué cosas nuevas hay que evaluar? En la teoría clásica no era necesario evaluar la trayectoria poscrítica (inicial), su estabilidad y la sensibilidad ante imperfecciones. Todos estos elementos son cruciales en la nueva teoría.
- ❑ ¿Qué cosas relaciona la nueva teoría? Relaciona la estabilidad de la trayectoria inicial con la caída en la carga de pandeo con respecto a la clásica.

Veamos que elementos conocía Koiter durante el desarrollo de su teoría. En primer lugar, Koiter utilizó el criterio energético para evaluar la estabilidad de un sistema. El criterio mismo no era nuevo y Koiter cita como caso antecedente a Bryan (1888):

“... Su trabajo parece haber sido el primero en intentar el desarrollo de una teoría general de estabilidad. Su trabajo se basaba en el criterio energético, que postula que un estado de equilibrio es estable o inestable dependiendo de si la energía potencial en ese estado tiene o no un mínimo. Sin embargo sus cálculos de la energía elástica toma en cuenta solo términos cuadráticos en los desplazamientos. La segunda variación de esta energía entonces es de la misma forma que la propia energía y es positiva como ella... la segunda variación de la energía potencial total es también siempre positiva de modo que la inestabilidad en esos casos queda excluida” (Koiter, 1945, pp. 1).

La principal fuente de la teoría fue, como Koiter mismo señala, los trabajos de Trefftz (1933) quien uso el criterio energético para evaluar la estabilidad. Pero las limitaciones de estos enfoques consisten en que al restringir la energía a contribuciones cuadráticas (en lugar de cuárticas) en función de los desplazamientos sólo era posible considerar el estado neutro de estabilidad:

“Las consideraciones generales de estabilidad desarrolladas anteriormente y discutidas aquí estaban restringidas a la investigación de la estabilidad neutra y específicamente

estaban orientadas hacia la determinación del límite de estabilidad. Los fenómenos que aparecen al alcanzar o aun exceder el límite de estabilidad no eran considerados” (Koiter, 1945, pp. 3).

Un segundo ingrediente tomado por Koiter fue la influencia de imperfecciones pequeñas. Pero mientras que Donnell consideraba que las imperfecciones se amplificarían al aumentar la carga hasta que el material entrara en fluencia, Koiter considero la influencia sobre un proceso de bifurcación, sin introducir plasticidad en la explicación.

Un tercer elemento es el modo de pandeo, que estaba presente en la teoría clásica. Ahora ese concepto juega un papel central, y la nueva teoría admite modos aislados y también modos acoplados. En la nueva teoría el modo se puede determinar mediante un análisis de autovectores.

Cuestionamientos y limitaciones de la teoría de Koiter

Cuando la nueva teoría se extendió tuvo que enfrentarse a críticas de otros investigadores. Los principales cuestionamientos surgidos en la teoría fueron los siguientes:

(a) El comportamiento de una cáscara en la región poscrítica inicial no es un criterio confiable para predecir su sensibilidad ante imperfecciones (Kempner, 1968). Se encontró un contraejemplo en este sentido: un cilindro elíptico bajo carga axial tiene un comportamiento que sigue la teoría de Koiter en su primer carga crítica, pero luego recupera su capacidad portante y puede ser sometido a cargas mucho mas altas.

(b) No es posible predecir con exactitud la respuesta poscrítica avanzada basándose en la respuesta poscrítica inicial. Esta limitación surgió como consecuencia del empleo de tecnicas de perturbación a partir del entorno del estado crítico.

El hecho de evidencia en contra (especialmente el caso de no predecir correctamente la carga de pandeo del cilindro elíptico) no hizo que la teoría fuera rechazada: “La teoría de Koiter soportó el ataque y es usada en numerosas publicaciones” (Esslinger y Geier, 1975, pp. 113).

Si bien Koiter mismo sólo trabajó con sistemas continuos, su teoría fue extendida a sistemas discretos, como se reseña en (Godoy, 2000). Proporcionó un campo fértil sobre el cual se pudo seguir trabajando. Fue confirmada teóricamente por la teoría de catástrofes en los 70 y 80.

ESTABILIDAD COMPUTACIONAL

La teoría de Koiter ha coexistido con un desarrollo paralelo, que ha usado los términos y nivel de comprensión logrados por Koiter. El avance en capacidades de computación y el desarrollo de algoritmos para seguir problemas no lineales hizo que se desarrollara una nueva forma de trabajo, con menos énfasis en la teoría y mas énfasis en el cómputo.

La estabilidad computacional (Bushnell, 1985) pone énfasis en el cómputo de las trayectorias de equilibrio usando técnicas numéricas; no usa técnicas de perturbación sino técnicas incrementales de avance, que son más eficientes. Debemos señalar que la teoría de Koiter también se puede computar mediante el método de elementos finitos, pero la precisión para el cálculo de trayectorias poscríticas es menor que en la estrategia de computar trayectorias paso a paso.

La motivación detrás de este enfoque ha sido la necesidad de contar con programas de elementos finitos para usuarios masivos. Desde el punto de vista del usuario, hubo un cambio de énfasis en la comprensión por uno de descripción. Pero el enfoque computacional al problema de pandeo también tiene sus limitaciones: por ejemplo, los algoritmos pasan por alto los puntos críticos, les cuesta cambiar de trayectorias, no proveen información sobre la estabilidad misma de los estados de equilibrio. En todas las soluciones intentadas se vuelve a la teoría de Koiter (por ejemplo, para cambios de trayectoria se usan perturbaciones hasta encontrar un estado de equilibrio).

LA VISIÓN DE PARADIGMAS Y REVOLUCIONES DE KUHN

En esta sección no intentaremos describir la teoría de Kuhn sobre el cambio científico, dado que esa tarea escapa a las limitaciones de un trabajo y a las posibilidades del autor. La versión original de este enfoque se encuentra en Kuhn (1962), mientras que una versión modificada fue presentada en Kuhn (1981). En lugar de eso supondremos cierta familiaridad del lector con la teoría de Kuhn y pasaremos a ilustrar de que manera esa teoría permite interpretar la situación de la estabilidad elástica al cambiar del enfoque clásico al nuevo.

La identificación de paradigmas en la teoría de estabilidad

Un aspecto central en el análisis de Kuhn es el de paradigma. El concepto mismo de paradigma no es fácilmente definible, como han notado muchos filósofos de la ciencia, dado se incluyen allí los supuestos teóricos, las leyes y las técnicas para su aplicación que los miembros de una comunidad científica adoptan en un momento determinado. Kuhn explicita el papel desempeñado por un paradigma como vehículo para la teoría científica:

“...su función es la de decir a los científicos que entidades contiene y no contiene la naturaleza y como se comportan esas entidades. Esta información proporciona un mapa cuyos detalles son elucidados por medio de las investigaciones científicas avanzadas... este mapa es tan esencial como la observación y la experimentación para el desarrollo continuo de la ciencia... Al aprender un paradigma, el científico adquiere al mismo tiempo teoría, métodos y normas, casi siempre en una mezcla inseparable” (Kuhn, 1962, pp. 173-174).

Lo primero que nos preguntamos es si las teorías clásica y poscrítica inicial llegan a constituir paradigmas en la concepción de Kuhn. Esto también permitirá establecer si ha habido realmente un cambio de paradigma.

Podemos afirmar que la teoría clásica fue el paradigma fundacional en la disciplina. Timoshenko explica el estado de situación hacia 1936:

“Necesidades prácticas de carácter urgente originaron en los últimos años amplias investigaciones teóricas y experimentales acerca de las condiciones que rigen la estabilidad de los elementos comprimidos... Parece haber llegado el momento en que esos trabajos, conservados en diferentes lugares y en distintos idiomas, y que son con frecuencia de difícil acceso para los ingenieros que los precisan como normas de sus proyectos, se compilen y lleguen a formar un libro” (Timoshenko, 1936, Prologo).

El nombre dado por Timoshenko a su libro de 1936, “Teoría de estabilidad elástica”, pasó a identificar el área de trabajo y se constituyó en el referente para todos aquellos que practicaban la especialidad o eran usuarios de ella. Trabajar en esta área (o paradigma) era contribuir a llenar los vacíos dejados por el libro, evaluando nuevas cargas críticas de pandeo con la metodología establecida, tanto experimental como analítica. Esto no significa que no hubiera antes de 1936 un cuerpo de conocimientos sobre estabilidad, sino que ese era un cuerpo disperso y se constituyó en paradigma asociado a la contribución de Timoshenko.

Entendemos que uno de los aspectos que pone en evidencia la identidad de un paradigma es el cambio de nombre de la teoría que lo designa. En el caso que estudiamos, la teoría clásica se llamaba “Teoría de estabilidad elástica”, mientras que la nueva teoría se identifica como “Teoría general de estabilidad elástica”. La distinción radica en una palabra (“general”); sin embargo, tiene las implicancias que la teoría clásica carecía de generalidad, se constituía por medio de una serie de casos resueltos sin una unidad de principios y sin unidad metodológica. Mas aún, ambas teorías hacen cosas distintas y buscan entidades distintas, de naturaleza diferente: una cosa (carga crítica) en la teoría clásica, y un proceso (el pasaje del estado crítico al poscrítico) en la nueva teoría.

Un segundo aspecto constituye la aparición de libros de texto en el proceso revolucionario de la ciencia:

“... puesto que los libros de texto son vehículos pedagógicos para la perpetuación de la ciencia normal, siempre que cambien el lenguaje, la estructura de problemas o las normas de la ciencia normal, tiene, íntegramente o en parte, que volver a escribirse ... Así pues, los libros de texto comienzan truncando el sentido de los científicos sobre la historia de su propia disciplina y, a continuación, proporcionan un sustituto para lo que han eliminado” (Kuhn, 1962, pp. 214).

Un tercer aspecto es la redefinición de la historia que condujo al paradigma y de la galería de héroes:

“Por razones que son obvias y muy funcionales, los libros de texto científicos... se refieren sólo a las partes del trabajo de científicos del pasado que pueden verse fácilmente como contribuciones al enunciado y a la solución de problemas paradigmáticos de los libros de texto. En parte por selección y en parte por distorsión, los científicos de épocas anteriores son representados implícitamente como si hubieran trabajado sobre el mismo conjunto de problemas fijos y de acuerdo con el mismo conjunto de cánones fijos que la revolución más reciente en teoría y metodología científica haya hecho presentar como científicas” (Kuhn, 1962, pp. 215).

Es interesante observar que el nombre de Henry Poincaré, quien trabajó hacia finales del siglo XIX, no aparece citado en ninguna de las ediciones de Timoshenko; sin embargo, es parte de la historia recuperada en los libros del nuevo paradigma (Thompson y Hunt, 1973), porque a él se remontan las técnicas de perturbación, necesarias en la teoría poscrítica inicial pero no en la clásica. Algo similar ocurre con Lyapunov, quien sentó las bases para comprender el concepto de estabilidad; pero ese concepto aparece como marginal en la teoría vieja y como central en la nueva. Y de Trefftz, quien desarrolló el método energético. Por lo tanto hay una redefinición de quienes son los precursores en el área de trabajo.

Un cuarto aspecto es que la nueva teoría no está formada por correcciones a la teoría clásica; esto se había intentado en el periodo que denominamos de transición, pero sin éxitos significativos porque no se avanzó en la comprensión de los fenómenos no explicados o anomalías. La nueva teoría debe comenzar definiendo axiomas y construye conocimientos de una manera más sistemática que en la teoría vieja.

Postulamos aquí que hay dos paradigmas bien establecidos, y que hubo un verdadero cambio de paradigma entre la teoría clásica y la poscrítica inicial; sin embargo, veremos que las consecuencias de ese cambio no son tal como las describe Kuhn sino que presentan otros matices. Además, el nuevo paradigma nunca adquirió el monopolio, de manera que han subsistido ambos paradigmas aunque a veces entren en conflicto.

Anomalías

Kuhn menciona que los descubrimientos de los que surgen nuevos tipos de fenómenos presentan algunas características:

“...la percepción previa de la anomalía, la aparición gradual y simultánea del reconocimiento tanto conceptual como de observación y el cambio consiguiente de las categorías y los procedimientos del paradigma, acompañados a menudo por resistencias” (Kuhn, 1962, pp. 107).

Consideremos dos anomalías vistas en las secciones anteriores. Entendemos que la teoría de Kuhn permite acomodar adecuadamente ambos casos, cuyo resultado fue diferente. Por el contrario, el concepto de anomalía no aparece explícitamente en Lakatos.

(a) La anomalía del cilindro con carga axial hizo tambalear a la teoría clásica. Esta puede interpretarse en el marco siguiente:

“...una anomalía sin aparente importancia fundamental puede provocar una crisis si las aplicaciones que inhibe tiene una importancia práctica particular...” (Kuhn, 1962, pp. 135-136).

Vale decir, la anomalía es significativa por no permitir predecir adecuadamente la inestabilidad de un problema urgente para la industria aeronáutica. Pero hay otra característica más de esta anomalía, que pertenece al grupo de

“...las anomalías reconocidas cuyo rasgo característico es su negativa tenaz a ser asimiladas en los paradigmas existentes” (Kuhn, 1962, pp. 157).

En este sentido vimos como fue difícil para los investigadores de la transición poder explicar las discrepancias anómalas como parte de la teoría clásica.

(b) La anomalía del cilindro elíptico bajo carga axial, por el contrario, no hizo temblar a la teoría de Koiter. ¿Que diferencia hay con la anterior anomalía?

“Aparentemente, tampoco había parecido lo suficientemente importante como para provocar el malestar que acompaña las crisis; pudiendo reconocerse como ejemplos en contrario y, no obstante, ser relegados para un trabajo posterior” (Kuhn, 1962, pp. 135).

De manera que la anomalía del cilindro elíptico cayó en un lugar latente: se comprendía porque la teoría no lo podía tomar en cuenta, pero no se contaba con un remedio que lo permitiera incluir adecuadamente. Esa paso a ser una de las limitaciones de la nueva teoría, que se concentraba en la vecindad del estado crítico y no llegaba mas lejos, como lo hace la estabilidad computacional.

El cambio de términos y conceptos entre paradigmas

Kuhn acierta al decir que los términos no se aprenden separados o aislados, sino que vienen en grupo y es allí donde toman sentido. Los conceptos que existían antes de la nueva teoría incluían la carga crítica, el modo de pandeo, y en el periodo de transición habían llegado a incorporar la trayectoria poscrítica, pero eso no bastó para acomodar los cambios revolucionarios. Lo nuevo (Koiter) no se puede describir completamente en términos de lo viejo (teoría clásica). Antes no había conceptos construidos para estabilidad poscrítica inicial, ni para sensibilidad ante imperfecciones. Por ejemplo, Donnell había introducido en esta área de la ciencia el término de imperfección, pero no con el enfoque de Koiter, que le dio un viraje nuevo. Por lo tanto, hay un vocabulario nuevo, términos nuevos y conceptos nuevos. Eso no excluye que esos nuevos términos no hayan sido aprendidos por los investigadores que continuaron trabajando en la teoría clásica.

El concepto de modo en la teoría clásica era una forma única e interesaba sólo como paso intermedio para llegar a la carga crítica. En la versión de Donnell se hablaba del modo postcrítico, pero también como forma inmutable. Mucho después se introduce la idea que el modo postcrítico puede evolucionar a medida que avanza el proceso de deformación. Koiter recupera tanto el modo único como el acoplado, no como forma de evaluar la trayectoria poscrítica avanzada sino sólo como manera de tomar una dirección de deformación hacia la cual la estructura tendería al inicio de los estados poscríticos.

Si bien previamente ya había conceptos de estabilidad debidos a Lyapunov, Koiter incluyó la estabilidad en el sentido de Trefftz, como término predictivo que forma parte del análisis. Entonces la estabilidad deja de ser una cualidad teórica secundaria y pasa a ser un elemento útil para la clasificación. La clasificación del comportamiento no era posible (ni se vislumbraba) antes de Koiter. La nueva teoría toma un carácter holista al clasificar los tipos de comportamiento posibles. Como tal, da una estructura a la teoría.

En la nueva teoría, algunos términos, como modo de pandeo y estabilidad, se conectan con la física del problema de una manera distinta. Y esto altera el conjunto de situaciones con las que se relacionan estos conceptos. Los problemas de traducción se ven facilitados enormemente por la transición, que incorpora términos necesarios para el nuevo enfoque, como los de trayectoria poscrítica e imperfecciones. Quizás términos como carga crítica ya no son idénticos, pierden el carácter predictivo que tenían en la teoría clásica y ahora adquieren un carácter de referencia, pasan a ser un valor en el cual apoyarse y son útiles para buscar un nuevo término que si tenga carácter predictivo. Ese nuevo término es la carga máxima (evaluada en la trayectoria imperfecta no lineal) que sólo adquiere sentido en conjunto con imperfecciones, respuesta inicial y estabilidad del estado crítico.

¿Hay revolución en el sentido de Kuhn?

El concepto de revolución de Kuhn ha sido duramente criticado desde la óptica de la biología. Según Mayr (1995), en biología no hay revoluciones en el sentido de Kuhn. Por ejemplo, en la clasificación sistemática no hubo una sustitución revolucionaria porque los dos paradigmas se siguieron usando. Mayr señala:

“No hubo una revolución en la que se rechazara la ciencia anterior. No hubo paradigmas inconmensurables” (Mayr, 1995, pp. 115).

En la teoría de estabilidad aún hoy los programas de computadora para propósitos generales que usan elementos finitos, pueden calcular tanto las cargas críticas (teoría clásica) como las trayectorias no lineales (teoría poscrítica). Es del conjunto de enfoques que el ingeniero interpreta la realidad o toma decisiones, y asigna a cada elemento de juicio un peso para interpretar su objeto de estudio.

En la teoría de estabilidad es claro que hubo una revolución, pero no un cambio repentino como dice Kuhn, ni tampoco irracional y excluyente. El conflicto entre paradigmas ciertamente existe en la teoría de estabilidad

“Así pues, es evidente que debe haber un conflicto entre el paradigma que descubre una anomalía y el que, más tarde, hace que la anomalía resulte normal dentro de las nuevas reglas” (Kuhn, 1962, pp. 156).

Pero eso no hace que ambos paradigmas resulten mutuamente excluyentes, como pretende Kuhn por ejemplo en este párrafo:

“Por consiguiente, demos ahora por sentado que las diferencias entre paradigmas sucesivos son necesarias e irreconciliables” (Kuhn, 1962, pp. 165).

Tampoco es cierta la imagen de revoluciones cortas y bien definidas con largos periodos intermedios de ciencia normal. La teoría clásica de estabilidad se consolidó como paradigma alrededor de 1936, pero casi coincidentemente se inició el periodo de transición en la búsqueda de solución de la anomalía del cilindro, que duró alrededor de 30 años. Nunca hubo cambio irracional del punto de vista de los científicos, basados en cuestiones de valores, sino que los científicos lograron ver el campo formado por dos paradigmas que coexisten aún. Siempre fue posible comparar los paradigmas entre ellos, y no han sido imposibles la traducción y la interpretación.

Nos queda siempre, por cierto, la alternativa de sólo llamar revoluciones en la terminología de Kuhn, a aquellas que cumplan con los requisitos que él señala, incluyendo (i) que “los proponentes de paradigmas que están en competencia estarán a menudo en desacuerdo con respecto a la lista de problemas que cualquier candidato a paradigma deba resolver”; (ii) que “en el nuevo paradigma, los términos, los conceptos y los experimentos antiguos entran en relaciones diferentes unos con otros”; y (iii) que “quienes proponen los paradigmas en competencia practican sus profesiones en mundos diferentes... ven cosas diferentes y las ven en relaciones distintas unas con otras” (Kuhn, 1962, pp. 230-233). Pero esta alternativa no parece estar en acuerdo con la ciencia real a la que se trata de comprender. Notamos que los puntos (i) y (iii) son

los más conflictivos y alejados de la realidad de la teoría de estabilidad. Consideremos estos aspectos en más detalle.

Por ejemplo, tomemos un científico cuya práctica de investigación está centrada en la teoría clásica, y le interesa principalmente evaluar cargas críticas clásicas en sistemas no clásicos, como elementos estructurales construidos con materiales compuestos (composites). Un artículo que deba ser revisado por otro científico inmerso en la teoría poscrítica encontrará que sólo le están contando parte de la historia, y que el estudio es preliminar y está incompleto. Pero no desechará la información provista por errónea, sino por incompleta y que puede llevar a conclusiones equivocadas. Tampoco ambos científicos viven en mundos conceptuales diferentes, porque muy probablemente ambos conozcan acerca de los dos paradigmas, sino que su práctica transita por caminos que terminan en lugares diferentes. Lo que para el primer científico es “trabajo a ser realizado en un futuro”, para el segundo es “trabajo aún no realizado”.

Sobre los científicos que generan el nuevo paradigma

Este es un tema marginal en Kuhn, pero el caso de Koiter parece corresponderse bien con el siguiente párrafo de Kuhn:

“Casi siempre, los hombres que realizan esos inventos fundamentales de un nuevo paradigma han sido muy jóvenes o muy noveles en el campo cuyo paradigma cambian... se trata de hombres que, al no estar comprometidos con las reglas tradicionales de la ciencia normal debido a que tienen poca práctica anterior, tiene muchas probabilidades de ver que esas reglas no definen ya un juego que pueda continuar adelante y de concebir otro conjunto que pueda reemplazarlas” (Kuhn, 1962, pp. 146-147).

La situación de Koiter al buscar su tema de tesis fue descrita así por él mismo:

“Llegué a la conclusión que valía la pena escribir una tesis doctoral en las noches... Al año siguiente imaginé que había encontrado una base adecuada, el teorema que el dominio estable en el espacio de carga de una estructura elástica con un estado precrítico lineal es siempre convexo. Fue desalentador cuando descubrí algo más tarde que Papkovich había publicado este teorema en 1934...” (Koiter, 1979, pp. 243).

Esto ilustra que Koiter era un recién llegado al campo y no tenía el tipo de compromiso con las reglas tradicionales de la teoría clásica. Aunque no se trata de algo central en la visión de Kuhn, el papel que cumplen los forjadores de un paradigma aparece acertadamente descrito en nuestro caso.

LA VISIÓN DE LOS PROGRAMAS DE INVESTIGACIÓN DE LAKATOS

Podemos analizar el cambio teórico de la teoría clásica a la de Koiter desde el punto de vista de los programas de investigación. Siguiendo a Lakatos (1978), podemos decir que existió un programa de investigación regresivo, basado en el concepto de carga crítica clásica, el cual fue progresivo en el momento en que se aplicó solamente a columnas y placas, pero que se convirtió en regresivo al abordar problemas de algunas cáscaras. Hay un cambio racional, y está basado en el poder explicativo del nuevo programa.

El enfoque de Lakatos permite una mejor caracterización de la estructura de los programas con respecto a la caracterización indefinida de la estructura de los paradigmas en Kuhn.

El núcleo firme

Según Lakatos, en el núcleo firme se define el conocimiento fundamental no problemático, las leyes básicas del programa. En el núcleo firme del programa de Koiter, los postulados básicos son

(a) Los axiomas de equilibrio y estabilidad de sistemas elásticos, basados en la energía potencial total. Estos axiomas no pueden someterse a cuestionamiento porque están tomados como el núcleo que se va a defender. Tampoco pueden demostrarse de manera independiente. Como sustento requieren de usar trabajos virtuales para sistemas continuos, y no tienen ni ese apoyo para sistemas discretos.

(b) La estabilidad del sistema en el estado crítico (que se calcula en base a derivadas superiores de la energía) determina si la trayectoria poscrítica en su parte inicial (desplazamientos muy pequeños) existe para valores crecientes de la carga o sólo para valores decrecientes. Koiter señala esto así:

“Esto lleva a la sospecha que la diferencia en el comportamiento elástico de diferentes estructuras con cargas cercanas a la carga teórica de pandeo esta conectado con el carácter diferente de los posibles estados de equilibrio vecinos correspondientes a esas cargas” (Koiter, 1945, pp. 5).

(c) Para pequeñas irregularidades en el sistema el estado precrítico se perturba y da una carga máxima (menor que la crítica cuando el estado crítico mismo es inestable) que se puede evaluar mediante un análisis de perturbación del comportamiento perfecto.

El cinturón protector

Las hipótesis auxiliares que defienden el núcleo se construyen de manera ecléctica. En la versión de Koiter, la teoría de estabilidad elástica incluye un cinturón protector constituido por varios elementos:

(a) Se considera que las componentes estructurales más eficientes son de una dimensión o de dos dimensiones, con tensiones aproximadamente constantes en la sección transversal o en el espesor (estado membranar), para los que el pandeo ocurre de manera de bifurcación.

(b) Se considera que se puede usar el tensor de Cauchy de tensiones conjuntamente con el tensor de deformaciones de Green-Lagrange, lo cual es estrictamente inconsistente. Pero según Koiter,

“En muchos problemas de pandeo podemos ignorar la diferencia entre las coordenadas cartesianas en la estructura indeformada y en el estado fundamental como variables independientes” (Koiter, 1982).

(c) Koiter usa una teoría simplificada de cáscaras, para cáscaras rebajadas:

“Para cáscaras el comportamiento más característico de pandeo ocurre en modos de pandeo rebajados, cuya longitud de onda mínima L es pequeña comparada con el radio de curvatura más pequeño de la superficie media” (Koiter, 1982).

Estas hipótesis auxiliares que hace Koiter podrían ser removidas si hiciera falta, pero a él le parecen razonables. Lejos de ser parte del núcleo central, estas condiciones pueden ser atacadas por otros autores y podrían estar sujetas a ajustes sin perjuicio del núcleo central.

La heurística

La heurística es la maquinaria para la solución de problemas en la teoría. Se ayuda de técnicas matemáticas y consiste en reglas metodológicas. La heurística positiva avanza sin importarle lo que dice la evidencia en contra, como los resultados adversos del cilindro elíptico o del cilindro con agujeros.

Como heurística importante la nueva teoría usa la técnica de perturbaciones. Thompson y otros investigadores enriquecen la heurística al asumir sistemas con múltiples grados de libertad (Thompson y Hunt, 1973), de modo que incorporan al método de elementos finitos como técnica de solución.

El cambio de programas

En el enfoque de Lakatos el cambio de programa de investigación comienza cuando el viejo programa se vuelve regresivo, o sea cuando la teoría se comienza a fabricar para acomodarse a los hechos. Se manufacturan hipótesis auxiliares después de los acontecimientos para proteger a la teoría de los hechos. Se atrasa con relación a los hechos y el programa regresivo corre detrás de los hechos para alcanzarlos. Esta situación regresiva se encuentra fácilmente en la teoría clásica de estabilidad, lo cual pone en evidencia una gran desconfianza en sus postulados y predicciones.

La teoría de Koiter por el contrario, es progresiva. Explica los fenómenos no explicados en la teoría anterior, y conduce a descubrir hechos o fenómenos nuevos. Por ejemplo, conduce a descubrir el acoplamiento de modos, ignorado hasta ese momento. El nuevo programa logra predecir hechos nuevos muy rápidamente una vez que fue aceptado por un grupo significativo (aunque no completo ni mayoritario) de científicos.

La historia externa

La historia en Lakatos contiene una historia interna y una externa. Lo que hemos narrado anteriormente es parte de la historia interna, formada por la explicación racional del crecimiento del conocimiento objetivo.

La historia externa es la historia sociológica y social de la ciencia. Aquí interesa fundamentalmente considerar la participación de Koiter, que produjo un cambio en la perspectiva desde la cual se podía ver el problema de estabilidad. Afortunadamente la situación de Koiter se encuentra documentada en sus escritos (Koiter, 1979) y en las conferencias que dictó en Río de Janeiro en 1983¹.

¹ Tuvimos ocasión de participar en dos cursos dictados por Koiter en LNCC en 1983 y 1985.

“En la primavera de 1940 tuve la mala fortuna de caer con neumonía y pleuresía y casi no pude salir de cama cuando ocurrió la invasión Nazi el 10 de mayo. Sin embargo, en cama leí un artículo estimulante por Leslie Cox... y su tratamiento simple del comportamiento de una columna con un apoyo no lineal elástico me sugirió la idea básica que la inestabilidad en la carga crítica podría ser la explicación del desastroso efecto de imperfecciones. Esta conjetura me llevó al desarrollo de la teoría general de estabilidad elástica no lineal en mi tesis... El trabajo básico, incluyendo la aplicación del sorprendente problema de pandeo de cáscaras cilíndricas bajo compresión axial, fue completado en febrero de 1942” (Koiter, 1979, pp. 243).

El idioma en el cual fue escrita la tesis sin duda influyó en sus posibilidades de impacto:

“Debido a que el alemán era la única lengua extranjera permitida por los Nazis para publicaciones, mi manuscrito obviamente sólo podía ser escrito en mi lengua nativa, que no era fácilmente accesible a la mayoría de mis colegas en el extranjero” (Koiter, 1979, pp. 243).

La defensa de la tesis fue postergada por Koiter hasta la finalización de la guerra en 1945. La tesis fue publicada por un editor a quien Koiter estuvo agradecido hasta su retiro, porque le facilitó demorar la publicación hasta que se produjo el fin de la guerra.

La primera presentación que realizó Koiter sobre estabilidad fue en una conferencia en París en 1946. El tema del trabajo era el pandeo de placas en estados poscríticos avanzados, pero sin recapitular la teoría que ya había desarrollado. Al parecer, Koiter entendía que ya había publicado su teoría en la tesis, y que no correspondía reiterar lo dicho otra vez. Las dificultades de entender la teoría misma, sumado a que estaba presentando un tópico que avanzaba sobre la teoría, hizo que el trabajo no fuera bien recibido:

“La sesión del congreso en la que presenté un trabajo con algunos resultados numéricos inesperados fue coordinada por von Mises, quien ... cerró la discusión diciendo que el no creía una palabra del trabajo” (Koiter, 1979, pp. 244).

Con esta mala recepción, Koiter se desanimó y se retiró de este campo, al que sólo volvería para dictar un curso en Estados Unidos en 1961. El primer trabajo sobre estabilidad publicado por Koiter en inglés es de 1956 (Koiter, 1956). La primera traducción de su tesis doctoral al inglés fue publicada por NASA en 1967, pero para esa época había otros científicos trabajando en los pasos de Koiter. Dos escuelas siguieron a Koiter: la de Harvard, con Budianski y Hutchinson, quienes continuaron con la formulación continua, y la de Inglaterra (centrada en University College London) donde se desarrolló la formulación discreta con Thompson y Chilver.

Koiter mismo no desarrolló una escuela en su entorno:

“Una de las serias limitaciones de mi carrera fue que he acompañado sólo unos pocos estudiantes a su grado doctoral... Una razón ... es que en mi trabajo no tenía mucha necesidad de asistencia por jóvenes asociados” (Koiter, 1979, pp. 240).

La escuela de Harvard fue también pequeña, pero en University College London se llevó a cabo un programa extenso de investigación, extendiendo la teoría entre 1965 y 1985. Si bien la formulación discreta permite la incorporación del método de elementos finitos a la teoría, esto sólo se realizó en la década de los 80.

La tesis doctoral de Thompson en Cambridge data de 1963. En el prólogo de su primer libro, Thompson argumenta que su formulación fue desarrollada independientemente de la de Koiter:

“Por varios años el primer autor no tuvo conocimiento de la disertación clásica del Prof. W. T. Koiter, la cual había permanecido desconocida desde 1945 y sólo recientemente fue traducida al inglés por la NASA. Esto fue muy afortunado, porque el peso de la contribución del Prof. Koiter podría haberlo desalentado a seguir con su propio desarrollo en el tema” (Thompson y Hunt, Prólogo, 1973)

Koiter menciona acerca de las influencias que operó su trabajo en otros investigadores:

“El interés en este campo también floreció en ese tiempo en particular en la Universidad de Harvard, donde creo que mi trabajo ha tenido alguna influencia, y en University College en Londres, donde un enfoque similar para sistemas elásticos discretos fue desarrollado de manera más o menos independiente, como se describe tan elocuentemente en el prefacio de la monografía de Thompson y Hunt...” (Koiter, 1979, pp. 244).

La historia externa explica porqué el trabajo de Koiter permaneció sin conocerse entre 1945 y 1961, y sólo con muchas restricciones a partir de ese momento. Las dificultades teóricas asociadas a la teoría misma fueron un escollo a su divulgación generalizada, que siempre permaneció limitada a un grupo de estudiosos que podían dedicar el tiempo necesario para su comprensión.

EL CAMBIO EPISTEMOLOGICO EN LA TEORÍA DE ESTABILIDAD

Una de las utilidades del enfoque epistemológico es que nos ayuda a seguir aspectos de las teorías que de otra forma podrían pasarnos desapercibidos. En este caso hemos prestado atención a dos enfoques de la filosofía de la ciencia, asociados a la interpretación de paradigmas científicos y a la de programas de investigación. Algunas de las conclusiones importantes que surgen de esta mirada pueden resumirse de la siguiente manera.

(a) Resulta útil visualizar de una manera más general las teorías dentro del marco de paradigmas o programas. La riqueza descriptiva de los programas de investigación parece más adecuada para las teorías de las ciencias de la ingeniería, al incluir una estructura explícita. Esa estructura explícita (núcleo, cinturón, heurística) es además identificable en el caso de las teorías de estabilidad. El concepto de paradigma es más elusivo y no nos ayuda tanto a estructurar la teoría analizada.

(b) Entendemos que las crisis de las teorías en ciencias de la ingeniería se generan cuando se identifican sus límites y se encuentran problemas que no pueden predecir correctamente un comportamiento importante. Pero no porque el problema sea conceptualmente crucial (como pretendía Bacon y los empiristas) o porque se haya identificado una forma de falsación, como pretendía Popper (1969), sino porque la limitación encontrada es significativa en el contexto de

aplicación. Por eso la teoría clásica de estabilidad entró en crisis, al encontrar serias limitaciones en problemas significativos (cilindro con carga axial, significativo en la industria aeronáutica).

(c) El papel de las anomalías en el cambio de teoría aparece ilustrado en nuestro estudio de estabilidad. El problema del cilindro bajo carga axial, ¿fue planteado como un caso de falsación, en el sentido de Popper? Claramente no. Nadie diseñó ese caso como un experimento crucial con la intención de falsar la teoría. Como menciona Timoshenko (1936), se debió al uso de cáscaras delgadas en estructuras aeronáuticas, que necesitaban de un nivel de seguridad alto y por ello indujeron a los ingenieros a realizar experimentos para tener confirmaciones independientes de los valores teóricos. La sorpresa fue encontrar diferencias tan grandes entre experimentos y cálculos. El motor de curiosidad fue la demanda de una industria, ampliando las fronteras de las necesidades, las que llevaron a pasar de columnas a cáscaras como tema de interés.

Sin embargo, cuando Kempner encontró falencias en la teoría de Koiter (el problema del cilindro elíptico en compresión), esta no entra en crisis porque el problema no es significativo como necesidad industrial, sino sólo es un contraejemplo y puede ser tratado como una anomalía. En definitiva, se genera una hipótesis ad-hoc (la teoría no puede aplicarse en ese tipo de casos).

En las ingenierías el conocimiento está atado a su significación en cuanto a las necesidades que logra satisfacer. Eso hace que necesariamente requiera de un marco histórico y uno de aplicación. El conocimiento no puede separarse de la aplicación, porque sino deja de tener significación.

(d) El periodo de transición juega un papel fundamental en el cambio teórico, no tanto porque los investigadores involucrados acumulen conocimientos hasta encontrar una nueva formulación, sino porque permite incorporar términos y conceptos nuevos útiles en la nueva teoría, aunque de una manera diferente. En nuestro ejemplo la transición aparece similar a lo descrito por Lakatos.

(e) El lugar de las revoluciones ha sido siempre conflictivo en la nueva filosofía de la ciencia. En nuestro caso hubo un cambio revolucionario; sin embargo ese cambio no tuvo la forma de una conversión en masa al nuevo paradigma, con la desaparición del viejo paradigma. Por el contrario, han seguido coexistiendo ambos paradigmas, aunque dentro de un marco más general en el que se reconocen las limitaciones del paradigma viejo. Esto es evidente en los programas para propósitos generales de elementos finitos, en los que coexisten los algoritmos de cálculo de carga crítica con los de evaluación de trayectorias no lineales que tiene sentido en la interpretación de Koiter.

AGRADECIMIENTOS

Este trabajo no habría sido posible sin el aporte de los colegas y profesores en University College London durante el periodo de 1975-1979. También agradecemos las enseñanzas de Koiter durante 1983 y 1985 en Río de Janeiro, y los comentarios que realizó en reuniones más informales.

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