單維彰師鐸獎評選佐證資料 1

與學生互動照片及剪報示例

照片

1. 98 學年開學前的暑期，我開設「微積分」先修課程，那是中央大學開始嘗試暑期先修的初期。當時有 121 名學生，來自全校理工資地各院。這一班的部份學生一直在學校裡保持聯繫。四年之後，這些同學畢業前，邀我回到當初「微積分先修」的教室，與他們合影留念。

2. 108 學年，也就是 108 課綱實施的第一年，我為了親自了解高中階段數學課綱的實施狀況，獲得桃園市大園國際高中校長及數學同仁的同意，全學年每週一次到大園高中，每次整個上午：

   (1) 參加數學教師共備，

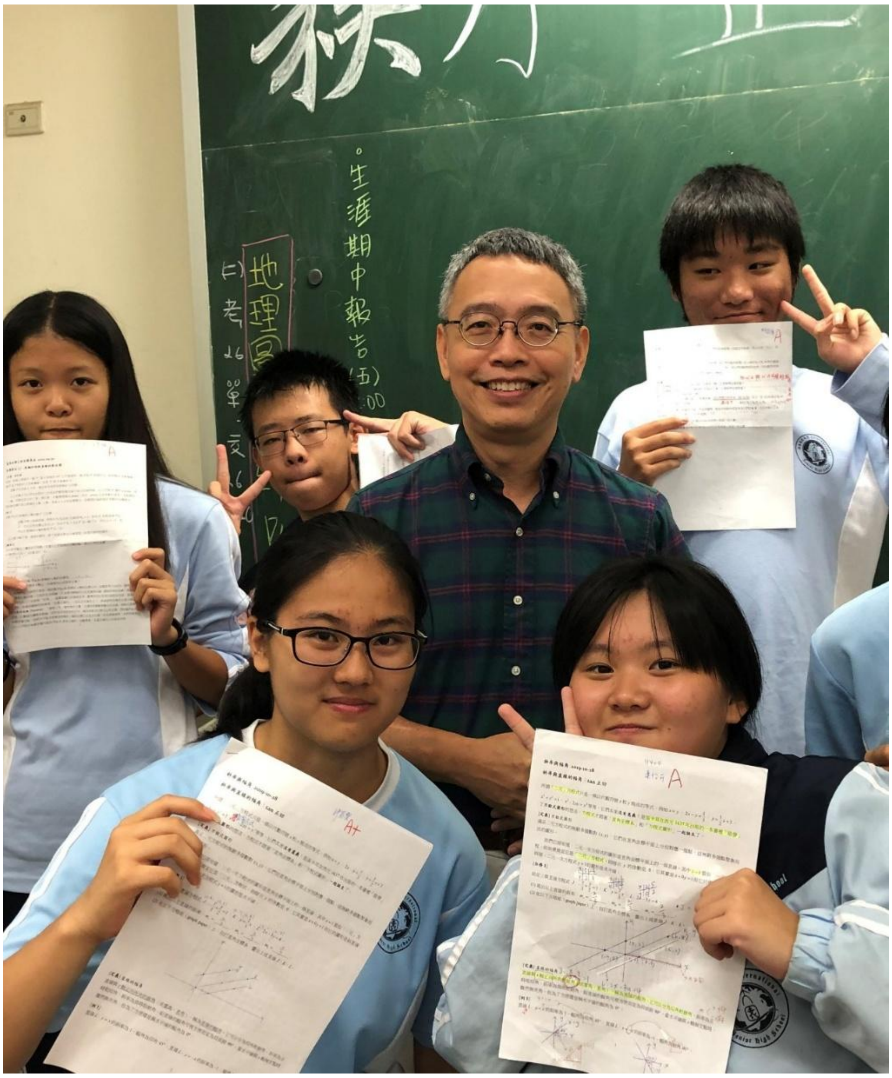
   (2) 到 114 班觀課，

   (3) 為 114 班學生提供 1 節實驗性質的授課。

   在 108 年 10 月 28 日的課堂中，我補充了廣義角正切（tan）與直線之斜率、幅角的關係，搭配計算機來理解 tan 的三角比意涵，以及如何直線斜

率跟幅角的對應關係。這堂課引起相當高的興趣。當天留下一份回家做的功課。隔週收作業，批閱與檢討之後，跟部份同學合影留念。

那一年的共同備課、觀課與實驗授課，並不是我個人的研究計畫，純粹是身為 108 課綱高中數學綱要的召集人，想要負起的一份責任。這一年的現場經驗，後來成為我製作許多教師研習教材的實徵依據。



剪報

我想要留在大學裡一輩子的生涯規畫，是著眼於作為大學教師的職業。當我還在美國留學時，就開始準備作為一名教師，廣泛蒐集通識材料，期許自己在專業以外提供給學生的通識與人文素養。雖然後來的網路資訊蓋過了我的收藏，但那段經歷是無法取代的。當我初任教職，帶的都是數學專業科目，但我已經開始提供學生通識類型的材料。三十多年前，我用剪刀膠水製作節選的文本，再影印給學生，內容從數學到社經文化皆有。下頁起，掃描幾份當年的剪報作為示例。

專題

我們當做什麼？

董世平

六月四日當中共政府竟然以血腥鎮壓的方式，「收復」天安門的消息傳來時，相信每個中國人都是悲憤莫名，不禁問道：「這是一個什麼政府？」從電視上看到北平市民冒自己生命的危險，來搶救傷者及死者時，怎能不感動，但想到開槍的人竟是自己的同胞時，又怎能不憤慨呢？我中華民族竟又遭逢了一次浩刼。

中華民族的近代史是一頁充滿血淚及屈辱的歷史，六月四日也令人再次想起了「六三」及「五四」，林則徐禁煙，引起鴉片戰爭，中國的門戶被「船堅炮利」打開了，自此「師夷之技以制夷」，「中體西用」，「變法」，「革命」，至五四的「德先生、賽先生」，中華民族始終未能走出一條路來，直到現在當大陸人民要求「自由」、「民主」時，所得的回答竟是坦克車、機關槍，「車堅槍利」竟拿來對付自己手無寸鐵的人民，我們怎能不悲痛？

我們在此以簽名、集會的方式，齊聲聲討中共的罪行，以捐款濟助大陸示威的同胞們，我們要問這有用嗎？我認爲這些是有用的，因這表示了我們身爲中國人對這件事的不滿與抗議，也支持了直接面對共產政權的同胞們，讓他們知道他們不孤單，我們是同一陣線。但我覺得還不夠，報紙上訪問學者、專家的意見，常看到的一個心態就是「無力感」，眞是如此嗎？我不認爲如此，我們的確能做的更多。

當年戊戌變法失敗，清兵搜捕志士，梁啓超勸譚嗣同一同東渡日本，譚嗣同回答梁啓超：「不有行者，無以圖將來；不有死者，無以酬聞主。今南海（指康有爲）之生死未可卜，程嬰杵臼，月照西鄉，吾與足下分任之。」程嬰與公孫杵臼，也就是「趙氏孤兒」的故事，原記載於史記，春秋時晉權臣屠岸賈殺趙盾全家，並搜捕孤兒趙武，程嬰與公孫杵臼定計救孤兒，程嬰將其子交與公孫杵臼，並假裝密報，致公孫杵臼及程嬰之子被殺，後程嬰撫養趙武成人，報仇雪恨。譚嗣同以公孫杵臼自任，自份必死，而勸梁啓超東渡日本，再圖將來，由於譚嗣同的鮮血，梁啓超的文章，致革命風潮蔚起，經　孫中山先生奔走號召，終致建立中華民國。故此我認爲「不有死者，無以喚國人；不有生者，無以圖將來。」現在，因環境的因素，我們皆無可選擇的成爲生者，我們就當負起生者的責任。死者並未已矣，因他們的血喚起了我中華民族，而我們願否說：「民主的鬪士，吾與足下分任之。」程嬰必須忍辱負重，教養托付給他的嬰孩，直至趙武長大成人，雪恥報仇。這不是一天、兩天，一年、兩年的事，甚至是數十年的事。如今托付給我們的，不再是一個嬰孩，乃是我們受辱的國家，我們苦難的民族，這是終我們一生也未必能達成的事，的確，「士不可不弘毅，任重而道遠。」

任何一個暴力、鎭壓自己百姓的政府是不可能長久的，但現今的共產政府的崩潰，並不足以擔保中華民族就能脫離苦難，我們必須重建中華文化，走出我民族的一條路來。現今中華文化在中共破壞下受到相當大的摧殘，放眼看去，我們必須說：「中國的希望在臺灣」，這個責任是無可推卸的，臺灣要爭氣，中國才有希望。個人認爲，我們可以全力以赴去做的有以下數端：

第一、心懷中國。盼望天安門廣場上所流的鮮血，再次提醒我們，我們乃是血脈相連的。

第二、立志。不只是一時的情緒激昂，過後就煙消雲散，我們必須要問我們能爲我們的民族、國家做什麼？找出個人一生的方向。

第三、認眞。從自己、現在開始，努力做好我們所當做的，若我們現在虛渡光陰，又怎能期望將來爲國所用呢？

第四、貫徹始終。這是最後也最重要的，不因環境的影響而改變，堅持理想。

當我們記念大陸上爲民主、自由而奮鬪的學生和烈士時，讓我們無愧而勇敢的對他們說：「民主的鬪士，吾與足下分任之。」

（註：本文作者係數學系教授）

中原大學

二、知識與智慧

——在中興大學的演講

陳之藩

英國劍橋大學開溫第士實驗室，在雷塞福時代，曾經有過一位研究X射線的科學家，司諾（C. P. Snow）。他又是小說家作了很多小說，後來又變成專管科學文化的政府官吏。他有過一陣很煩躁，說出「兩種文化」這麼一個名詞。因此近三四十年來，常有人提起及引用這個名詞「兩種文化」。

兩種文化這個名詞的誕生，可以說相當偶然。因為大物理學家雷塞福、大數學家哈代，在二三十年代當時，是名聞遐邇的人物。但他們在劍橋大學裏常常覺得文學、哲學、藝術等系的教授羣跟他數學物理教授羣不太一樣。好像這兩羣教授彼此互瞧不起。雷塞福曾自豪的說：現在是原子時代的潮流，而這個潮流就是我。他倒是可以這樣睥睨當世，作此豪語的。因為他的每一發現，每一論文都在改寫歷史；立時影響歷史。他的實驗室中原子分裂的聲音在世界各個角落發生如雷的廻盪，但他卻不能使劍橋大學的文學、藝術、哲學教授羣對他投出青睞。司諾當時是開溫第士實驗室的一名助手，後來是基督學院的院士，看到這種現象，因而有感；他的感觸是，也許文、藝、哲等等是一種文化，而科學是另一種文化罷！而這兩種文化有着基本上的不同，有着不能相通的地方。

我們乍看起來，文、哲、藝術的知識與科學的知識，在中學裏是不同名稱的功課，在大學裏是不同名稱的院系，在社會上是不同名稱的部門，但其為學術，其為知識，好像是相同的。

可是仔細一想，你就會產生一種異樣的感覺。為什麼柏拉圖我們拿起來就可以看，就一直在引用，為什麼阿基米德我們只說他那個原理，再沒有什麼別的傳下來了。為什麼希臘的人文、制度、史詩至今為人所引證，而希臘的化學、物理則僅是一些名詞，其餘均不堪入目了。為什麼莎士比亞的幾乎每句話被世人反覆誦讀，而中世紀的鍊金術幾乎完全不為人所知了。為什麼三千年前的莊子寓言還為人所樂道，而三十年前的化學元素只九十二種已落伍得一塌糊塗了。就是在一個普通家庭中，解釋一首杜甫的詩，子女要請益於父母；而算一題新數學，父母要請教於子女？一言以蔽之，有些知識，好像是變的很少，是越老越值錢；有些知識卻是變得很快，變的很多，是越新越可貴。我們於是慢慢感覺出來，真的，文、哲、藝術的性質，與科學的性質有所不同。如同站在涇渭交滙的地方，凝視這一半是清的涇水；凝視另一半卻是濁的渭水，兩水雖然流入一個河床中，卻區分得如此鮮明，如此顯著。

現代的文化學者，比如，哈佛大學的布瑞頓（C. Brinton）也愛把知識界一分為二，一種是累積性的知識；一種是非累積性的知識。

非累積性的知識，可以用文學的例子來說明。二千多年前，一位希臘文學家把一些觀念提出來從而闡明是與非，善與惡，美與醜等。二千多年後，我們所處理的差不多還是這些。在知識的領域裏並沒有增益多少。

累積性的知識則不同了，希臘科學家所談的星座，所談的物質的要素，所談的原子，現在不會有人重視了。換句話說，如果亞利斯朶芬，如柏拉圖，忽然降臨二十世紀，給我們一個演講。我們聽來還是頭頭是道。可是希臘科學家，比如阿基米德，如果忽然降臨二十世紀，他要補習許多物理上的功課，學習許多數學上的術語，才能與波耳或愛因斯坦交談。

或者我們用另一個方式說，一位中興大學的學生，並不比希臘的一位科學家聰明，但他比希臘科學家有更多的物理知識。他自然在文、藝、哲學上比二千五百年前的希臘人知道更多的事實，但在科學上，他不僅是知道更多的事實，而且他明瞭更多事實間的關係，也就是他知道更多的定律，更多的原理。

這種累積性的知識與非累積性的知識的區分是非常重要的。這種區分絕對不是說科學好而有用，文、藝、哲學壞而無用，這只是說就累積而言，這兩種知識是顯然不同的。凡是有人說藝術劣於科學，或另一種人說科學劣於藝術，都是沒有注意這種累積性的區分，而隨便以自己習見的尺度來衡量別人的長短。

一個公平的文化學者，必須冷靜的正視，辨認，累積性與非累積性的知識，去追蹤它們相互間的關係，去研討它們對人類行為的影響。這兩種知識各有其重要性是自不待言的。

我們回顧過去三百年，可以說累積性的知識累積的很快，而過去三千年，非累積性知識累積的很慢，而快與慢，並不能以之為判斷優劣的標準。

我們有此衡度在心，現在就可以檢討這兩類知識對人類行為的影響了。對此一問題，已不止是個人的好與惡，團體的取與捨的問題；而是關聯到我們未來的生與死，存與亡。而這裏所說的未來，即是眼前的未來，不久的未來。

我們談到這兩種知識對人類行為所產生的影響，我想大家一定與我同感：那即是談虎色變。

現在大家公認，如火如荼的宗教改革運動對中世紀的破壞，遠不如科學所破壞的既劇且烈。牛頓的理論本來是物理界中的事，但他的影響卻是左右了三百年來的宇宙觀與人生觀。把來世上天國的人生觀，一變而為今世在地上建天國的人生觀。這個變化是空前鉅大的。達爾文的進化論本來是生物界中的事，但他的影響卻把進步的人生觀牢固的

An interview with 9

Richard Feynman

about superstrings

Richard Feynman was Professor in the Department of Physics at the California Institute of Technology. He is credited with laying the theoretical foundations for much of modern particle physics and quantum field theory, and was awarded a Nobel prize for his work on quantum electrodynamics. As one of the 'elder statesmen' of modern fundamental physics, his scepticism towards superstrings is especially pertinent. He died in early 1988.

A few years ago, Stephen Hawking said that he thought the end of theoretical physics might be in sight. I think he was referring to the recent successes in attempting to unify all of physics into a single descriptive scheme. This seems a very provocative statement. How do you feel about this, having spent a lifetime in attempting to unify certain aspects of physics?

I've had a lifetime of that, and I've had a lifetime of people who believe that the answer is just around the corner. But again and again it's been a failure. Eddington, who thought that with the theory of electrons and quantum mechanics everything was going to be simple and then guessed everything, because it was going to be simple, but guessed wrong. Einstein, who thought that he had a unified theory just around the corner, but didn't know anything about nuclei and was unable of course to guess it. And today, there are a large number of things that are not understood. That isn't fully appreciated, and people think they're very close to the answer, but I don't think so.

Do you think we've got any right to suppose that nature at its deepest level is unified — that there are simple mathematical statements that can encapsulate all of reality?

In our field we have the right to do anything we want. It's just a guess. If you guess that everything can be encapsulated in a very small number of laws, you have the right to try. We don't have anything to fear, because if something's wrong we check it against experiment, and experiment may tell us that it's not true. So we can try anything we want. There's no danger in making such a guess. There may be a psychological danger if you bend too much work in the wrong direction, but usually it's not a matter of right and wrong. Whether or not nature has an ultimate, simple, unified, beautiful form is an open question, and I don't want to say either way. I'll find out — although maybe I won't live long enough. I want to find out as much as I can about nature, and not to presume ahead of time. Whether it's a simple formula, or it's complicated, doesn't make any difference. Everyone's guesses can turn out the way they want.

One of the problems about testing these recent ideas experimentally is that the features which suggest there might be unification occur only at exceedingly high energies. I think we're beginning to reach the end of the road for high energy particle physics as far as accelerators are concerned. It's hard to see beyond the next generation of experiments, simply because of financial constraints. Do you think for these reasons that maybe theoretical physics is degenerating into philosophy?

Maybe theoretical physics is degenerating but I don't know into what. Let me just say something first. I have noticed when I was younger, that lots of old men in the field couldn't understand new ideas very well, and resisted them with one method or another, and that they were very foolish in saying these ideas were wrong — such as Einstein not being able to take quantum mechanics. I'm an old man now, and these are new ideas, and they look crazy to me, and they look like they're on the wrong track. Now I know that other old men have been very foolish in saying things like this, and, therefore, I would be very foolish to say this is nonsense. I am going to be very foolish, because I do feel strongly that this is nonsense! I can't help it, even though I know the danger in such a point of view. So perhaps I could entertain future historians by saying I think all this superstring stuff is crazy and is in the wrong direction.

What is it you don't like about it?

I don't like that they're not calculating anything. I don't like that they don't check their ideas. I don't like that for anything that disagrees with an experiment, they cook up an explanation — a fix-up to say 'Well, it still might be true'. For example, the theory requires ten dimensions. Well, maybe there's a way of wrapping up six of the dimensions. Yes, that's possible mathematically, but why not seven? When they write their equation, the *equation* should decide how many of these things get wrapped up, *not* the desire to agree with experiment. In other words, there's no reason whatsoever in superstring theory that it isn't eight of the ten dimensions that get wrapped up and that the result is only two dimensions, which would be completely in disagreement with experience. So the fact that it might disagree with experience is very tenuous, it doesn't produce anything; it has to be excused most of the time. It doesn't look right.

Is this a problem of style of research, or is it a problem of the sort of things that these people are trying to do?

No, I don't know whether you'd call it style of research, it's a question of verifying your ideas against experiment and whether the theory is precise enough. It is precise mathematically, but the mathematics is far too difficult for the individuals who are doing it, and they don't draw their conclusions with any rigour. So they just guess.

You seem to be saying that they're careless in the way they're going about it.

No, there're not careless, but it's very difficult. So they're unable to make a precise prediction — not through carelessness, but through inability. But they then continue to say it looks like a promising theory, in spite of the fact that they have to add all these guesses. Now it could be that it wraps up six of the ten dimensions, and it could be that it does this and it does that. For instance, there's a large number of particles in this theory, many more than we see. So we say right, the ones that we don't see maybe acquire an enormous mass — the so-called Planck mass — which has prevented us from seeing them. And the ones that we do see don't acquire such an enormous mass. But why is it those and not the others? The answer should be a consequence of the very theory they're writing down. But they are unable to demonstrate this. In other words, there's no real comparison to experiment. On top of that, the particles that we see do acquire a mass, but it's much smaller than the Planck mass — it's at the range of present experiment. And how that comes about, this other scale of mass, is not known.

Finally, although people say that there are no experiments to lead us, it's not true. We have some twenty-four or more — I don't know the exact number — mysterious numbers associated with masses. Why is it that the mass of the muon compared with the electron is exactly 206 or whatever it is, why are the masses of the various particles such as quarks what they are? All these numbers, and others analogous to that — which amount to some two dozen — have *no* explanations in these string theories — absolutely none! There's not an idea at the present time, in any of the theoretical structures that I have heard of, which will give a clue as to why those masses are what they are.

Does Industry Want Mathematicians? (Does Academia Care?)

By Robert Styer

Does business want or need mathematicians? Experts in the field—John Hamilton (Computational Science Laboratory, Eastman Kodak Company), Aaron Owens (Modeling and Simulation Group, Dupont Central Research and Development Group), Linda Banecker and Thomas Pratt (Technical Operations, Lockheed–Martin Management and Data Systems), and Robert Borrelli (Harvey Mudd College)—presented their perspectives on this question at the second annual Mathematics and its Applications Throughout the Curriculum Workshop at Villanova University in June 1997. In spite of the different backgrounds of the individual speakers, one common thread wound through the discussions: When using the term "industrial mathematician," we must put the emphasis on "industrial," not on "mathematician." This perspective appeared in many guises, hinted at many consequences, and provided some thought-provoking suggestions for academia.

The Value of a Model

The industrial mathematician works toward the goals of industry, not the goals of mathematical elegance or completeness. Nothing illustrates this more clearly than a story that Robert Borrelli shared with workshop participants.

Many years ago, when he was working at Philco Ford, Borrelli explained, the company was working on satellite stabilization problems for the government. The company was using massive computer runs to optimize the stabilization design parameters. On his own initiative, Borrelli found an exact formula for the optimum parameters; his solution would have even saved massive amounts of computer time. However, his boss was not happy with this achievement. Why? Because Philco Ford was receiving large sums of government money for those computer runs; Borrelli had optimized the wrong variable!

Today, Borrelli guides the Mathematics Clinic at Harvey Mudd College—a program that he helped found more than 20 years ago. The clinic supervises teams of senior mathematics majors who consult on industry problems and learn firsthand the nature of industrial mathematics.

Businesses are also reinterpreting traditional academic categories to reflect these industrial goals, reported Dupont's Aaron Owens. When he was hired by Dupont, he explained, the company had four people who held the job title of mathematician. That title no longer exists. Even the names of the groups in which mathematicians work have changed: The former Applied Mathematics Group is now the Modeling and Simulation Group, the Applied Statistics Group has become the Quality Management Group, and the Operations Research Group has acquired the goal-oriented title of Supply Chain Optimization Group. "I don't do problems that interest me," said Owens. "I work on problems important to Dupont."

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Industrial goals affect the way mathematicians work in several ways. These goals often dominate the mathematical modeling process. "Often, the question is which way to adjust a process," said Owens. "You have a 50–50 chance of getting the answer right! Accurate and elegant models are fine, but what really matters is how rapidly you can answer yes/no, up/down."

"The model does not have to be perfect to be useful," said Kodak's John Hamilton. "The value of a model is measured by the importance of the question it answers." He argued that simple models are more tractable and easier to verify. And while industrial models are not detailed enough to replace experimentation, they should be able to predict efficiently fruitful ranges for experimental parameters.

Industrial goals also affect where mathematics is most useful in industry. For example, Hamilton outlined two distinct goals of business: (1) to reduce product development time and (2) to achieve robust manufacturing processes. Industrial mathematicians can reduce product development times significantly when their models specify more fruitful experimental ranges, he said.

The closer one gets to production, however, the less mathematical models contribute. Manufacturing managers rightly abhor tinkering with tried and true processes, added Hamilton. "An ill-advised change in the process can literally cost millions of dollars in lost production," he explained, "so they are cautious and slow to change."

"Job #1 in production is to maintain production," asserted Owens. "All that counts is throughput." Consequently, mathematical research focuses on rapid product development. In today's business climate, a product needs to be developed quickly; research then switches to a new product line.

Hamilton discussed several areas in which he has worked: medical x-ray intensifying screens, blood serum analysis (fluid flow in a porous medium), stockroom analysis, quantum chemistry and dye design, color interpolation for digital cameras, and phase noise blur filter design. Thomas Pratt of Lockheed–Martin identified mathematical models of the turbine blade loss that results from collisions with birds, blade design, fan blade ordering, and single crystal growth as areas in which he and other mathematicians are interested. As these diverse lists suggest, the industrial mathematician must be flexible and willing to digest plenty of nonmathematical material. According to Owens, mathematical flexibility is critical: Owens earned his degree in theoretical astrophysics, yet he uses tools from numerical modeling, parameter estimation, systems of stiff ODEs, neural networks, partial least squares, and exploratory data analysis. In fact, he claims to have gone through several career changes—all within Dupont.

Part of a Larger Team and A Bigger Picture

Industrial mathematicians do not chart the course of industrial research. Rather, they function as a small but integral part of a larger team, and their mathematical training is but one element in a bigger picture. Owens described the mathematical practitioner as an internal consultant, providing small parts to larger projects. As would any consultant, the industrial mathematician has a vested interest in finding promising project ideas, selling these ideas to the team, and ensuring that the results are used. Thus, communication skills are essential; internal memos and progress reports serve as industry's "publications." Hamilton concurred with Owens, adding, "You need to keep checking to see if you are still working on the right problem (talk, talk, talk!). The problem-solving process is iterative. . . The learning never ends."

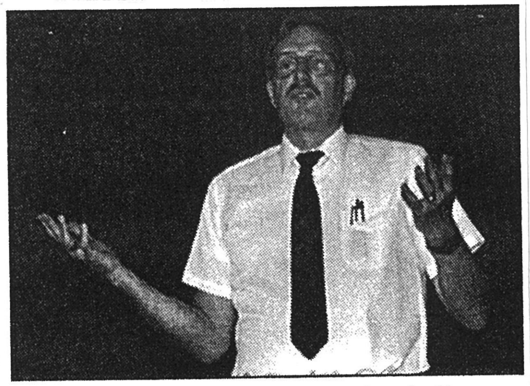
In order to be an effective member of this team, the speakers observed, the industrial mathematician must have good computing skills. According to Owens, spreadsheets, the universal form of communication, are ubiquitous. Almost all statistical functions are computer-based; almost all modeling projects end with a computer model; and almost all mathematics in industry is done on computers. All results come out of a computer even when much of the work is analysis, said Hamilton.

Hamilton reported on his informal survey of 50 recent Kodak hires who had listed a degree in mathematics on their resumes; more than half were hired into jobs that included the word "software" in the job title, and many were hired because of specific software experience.

Linda Banecker corroborated these anecdotal observations. As a hiring manager for Lockheed–Martin, she is acutely aware of the lack of people trained in information technology; this year, her company has hired less than two-thirds of its quota for technical personnel. And, according to Banecker, a conservative estimate of unfilled information technology positions nationwide is 190,000; 68% of IT companies cite the lack of skilled workers as a barrier to future growth. To attract talented recent college graduates, Lockheed–Martin offers an impressive Engineering Leadership Development Program and a Technical Development Curriculum. The training curriculum begins with a hefty dose of mathematics and then quickly moves to a variety of computing and business and engineering topics.

Owens also stressed the dominant role of statistical tools for industrial mathematicians. Observation errors, systematic and random, are always factors in the industrial setting, he explained. The data quality from research is often uneven, and the quality of production process data is even worse. At Dupont, he reported, a large portion of the work is carried out by bachelor's degree technicians, who have a great need for numerical computing and visualization and spreadsheet skills.

Owens pointed to the large amounts of



The mathematics he uses in his work at the Eastman Kodak Company might be mostly at the undergraduate level, says John Hamilton, but it's "applied with a graduate perspective." Elaborating on his comment from the MATC workshop, Hamilton takes another look at problem solving in industry: "By the time the computations are being done, it just looks like linear (ho-hum) algebra, but by then the problem has already been defined, the level of approximation has already been set, and the alternative methods have already been evaluated. Misjudge the problem there and all the linear algebra in the world won't fix it."

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time wasted by naive researchers analyzing two-point experiments with large error bars. "They run an experiment with the parameter first moved up then down, and make a decision based on these two data points, only to find that a bad product results," he explained. "The simplest observation that there is noise in observations is not getting through to graduates. The statistical education of engineers and scientists is sorely lacking, at least at the institutions from which Dupont hires!"

Reading between the lines, one might conclude that business goals do not correlate to academic mathematical goals, that computer skills and statistical skills are much more important than classical mathematical academic training, and that mathematicians are only a minuscule part of the industrial scene. To add insult to injury, each speaker cited other problem areas: Hamilton said that in most of his work at Kodak, he uses mathematical tools taught at the undergraduate level; Owens reported that three of the four mathematicians working at Dupont on his arrival are now doing computing or experimental work; Borrelli pointed to the difficulty of finding suitable industrial problems for the Mathematics Clinic student teams; and Banecker acknowledged that the mathematics component of the training curriculum acts like a filter to weed out those who would not survive the later portions of the training.

Proof Is in the Hiring

Does mathematical training per se have any value for industry? Each speaker provided some insight into this question: After completing a master's degree in mathematics, Linda Banecker began her industrial career as a programmer, typi-