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IMPAKT

TÉNYEK A TUDOMÁNYOS ALAPKUTATÁSRÓL

Szilárd: Csak a tényeket írom le – nem azért, hogy bárki is elolvassa, csakis a Jóisten számára.
Bethe: Nem gondolod, hogy a Jóisten ismeri a tényeket?
Szilárd: Lehet, hogy ismeri, de a tényeknek nem *ezt* a változatát.

[*Leo Szilard, His version of the Facts. S.R. Weart & Gertrud Weiss Szilard* (Eds), MIT Press, Cambridge, MA, 1978, p.149.]

A tartalomból:

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Az alapkutatások értéke

Miért kell mégis folytatni az alapkutatásokat annak ellenére is, hogy ma még nem kecsegtetnek alkalmazási lehetőségekkel? Választ kell adnunk erre a kérdésre, hogy elejét vegyük a gazdasági, politikai és morális támogatás hirtelen és ügyetlen megvonásának.

Az alapkutatás testesíti meg a vizsgálódás szellemét és a felfedezőket magáért a felfedezésért hívja harcba. Ez a természet miértjeinek és hogyanjainak önzetlen, kíváncsi vizsgálata; felelet keresése a megválaszolatlan kérdésekre. Ezt a szellemet már csak azért is őrizni kell, mert az alkalmazott tudományok számára is ösztönző hatású. Szeretnék idézni egy tanulságos gondolatot *Polányi* Mihály egyik művéből (Personal Knowledge — U. Chicago Press, 1958, 182. old.): "*A természettudomány (alaptudományi) módszerét pontosan azért dolgozták ki, hogy a dolgok természetét kiderítse gondosan ellenőrzött körülmények között és sokkal szigorúbb kritériumok alapján, mint ahogy a jelenségek a gyakorlati problémákkal járó helyzetekben megjelennek. E körülményeket és kritériumokat csak a téma teljesen tudományos értékű és szellemű vizsgálata során lehet feltárni, s erre csak azok a szellemek képesek, akiket a tudományos értékek megbecsülésére neveltek. Ezt az érzékenységet nem lehet csak úgy átkapcsolni és olyan célok szolgálatába állítani, amelyek a kutatók belső szenvedélyével ellentétesek.*"

Az alapkutatás és az alkalmazott kutatás összefonódik. Olyanok, mint egy fa, amelynek gyökerei az alapkutatást, ágai pedig az alkamazott kutatást jelképezik, s ha elvágjuk a gyökereket, az ágak is elsatnyulnak. A másik intellektuális érték abban a szerepben rejlik, amit az alaptudomány a fiatal kutatók nevelésében játszik. Olyan hozzáállás kialakítását segíti elő mely a legtermékenyebb, leghatásosabb tevékenységre sarkallja a fiatalt, bármivel foglalkozzék is pályafutása során. A tapasztalatok szerint az alaptudományi kutatásban eltöltött évek gyakran a legjobb felkészülést jelentik az alkalmazott kutatásban folytatott munkához.

Az alapkutatásnak etikai értékei is vannak. Elősegíti a kritikai szellem kifejlődését, a tévedés belátásának képességét. Antidogmatikus szemléletű, mert annak megértésére nevel, hogy minden tudományos eredmény csak szerény próbálkozás a természet megismerésére, s eleve ki van téve annak, hogy egy jövőbeni kísérlet majd megcáfolja. Az alapkutatás barátságba hoz a természettel, elősegíti a természetben elfoglalt helyünk és szerepünk mélyebb megértését.

Az alaptudományoknak politikai értékük is van. Nemzetek fölött álló kollektív vállakozásoknak kell lenniük (reméljük, hamarosan valóban azzá is lesznek), melyek minden nemzeti, faji, ideológiai határon átlépve mozgósítják az embereket. A verseny célja pedig az, hogy a munka minősége legyen a döntő, ne pedig a nemzeti felsőbbrendűség.

Sajnos a közelmúltban néhány tudományos szemfényvesztő csaló a nagyközönség előtt kétségeket ébresztett a tudomány magasztos erkölcsi mércéi iránt. Pedig már az is elég meglepő, hogy más emberi tevékenységekhez képest milyen kevés csalás fordul elő a

(Folytatás a következő oldalon)

MAGYAR TUDOMÁNYOS AKADÉMIA KÖNYVTÁRA

természettudományokban. Nyilván azért, mert roppant veszélyes hamis állításokkal kockáztatni egy kutató tudományos rangját, elfogadottságát. Persze előfordul, hogy a jó szándék ellenére is napvilágot látnak helytelen eredmények, de az ilyen esetek hamar tisztázódnak.

Az elmúlt évtizedekben az alaptudományok értéke elvesztette vonzerejét. Támogatottsága a hetvenes évek óta csökkent, ma már nem lehet olyan hatékonyan végezni a munkát, mint korábban. Az Egyesült Államokban például az NSF (National Science Foundation), amit kimondottan az alapkutatások támogatására hoztak létre, áttért az alkalmazott kutatások támogatására. Hasonló jelenségek figyelhetők meg Európában is.

A "hagyományos" alaptudományok, mint például a részecskefizika, a magfizika, a csillagászat ezt jobban megszenvedik, mint a biológia vagy néhány új alapkutatási irányzat (pl. a káosz, a komplexitás, a neurológia), mert az utóbbiak közelebb állnak az alkalmazhatósághoz. Különös módon a csillagászat és a kozmológia kevésbé sínyli meg ezt a folyamatot, mint a részecske- és a magfizika. Ennek valószínűleg az az oka, hogy a nagyközönség szemében ezeket a tudományágakat velünk született érdeklődés kíséri, s bizonyos, a vallási kérdésekhez is közel álló problémákat vizsgálnak (teremtett-e a világ vagy sem stb.). A részecskefizika szerencséje úgy hozta, hogy a kozmológia kabátszélébe kapaszkodva próbálhat nagyobb támogatásra szert tenni, mert eredményeire szüksége van a kozmológiának, hogy feltárhassa, mi történt az Univerzum létének első három percében.

Sok jó indok volna arra, hogy a jelenleg még nem alkalmazható alaptudományi kutatás továbbra is jelentős anyagi támogatást kapjon. De mekkora legyen ez a támogatás? Akkora, mint a tudomány fénykorában, 1946 és 1970 között? Tény, hogy az akkori támogatás gazdag terméshez vezetett mind az alkalmazott, mind az alapkutatásban. De vajon kell-e nekünk ilyen gazdag termés ilyen rövid idő alatt? Mi a helyes mérték, mi az, ami már túlságosan kevés? Ezekre nagyon nehéz válaszolni. Kérdéses, hogy ma is fenn kell-e tartani a háború utáni fénykor bőséges támogatását. Ugyanakkor világos, hogy nem lehet a támogatást olyan kicsire szabni, hogy elnyomorítsa az ígéretes alaptudományi ágazatokat és megakadályozza, hogy tehetséges fiatalok ilyen pályákon elinduljanak.

Tipikus példa erre a részecskefizika mai helyzete az Egyesült Államokban. Leállították ugyanis a texasi Szupravezető Szuperütköztető építését, ami egy 12 milliárd dolláros költségvetésű óriási tudományos program volt. Európának is szembe kell néznie hasonló helyzettel egy szerényebb protonütköztető tervezésekor. Amikor ilyen terv törléséről vagy lelassításáról döntenek, veszélybe kerül a részecskefizika, mert elveszítheti a területen dolgozó szakemberek kritikus számát. Ugyanakkor kevés erőfeszítés történt annak érdekében, hogy nemzetközi szinten minden érdekelt fél erőforrásait egyesítve hozzanak létre egy ilyen óriási berendezést.

A nacionalizmus és a regionalizmus kritikus helyzetbe hozta a részecskefizikát; az amerikai és az európai csoportok egymástól függetlenül kezdtek egy óriásgép építésébe. A tudománypolitika bizony nehéz kenyér, amikor az anyagi erőforrások szűkösek.

A tudományos közösséget is bírálhatjuk az alaptudomány szellemének cserbenhagyásáért. Nacionalista nézetek tünetei jelentek meg, melyek szerint az amerikaiaknak meg kell őrizniük vezető szerepüket egy-egy szakterületen.

A túlhajtott specializáció is hátrányosan hat a tudomány szellemére. Isaac Isidor *Rabi* szavaival (Science: The Center of Culture, New York, 1971, 92. old.): "*A tudományoknak nagy szüksége van integrációra és egységesítésre. A napjainkban megfigyelhető irányzat azonban éppen ezzel ellentétes... Csak a fokozatot szerzett fiatal kutató, ez a szegény igásállat a megmondhatója, hogy már tényleg csak pislákolnak e magasztos elvek lángjai. Ahogyan nő a fizikusok száma, úgy válik minden szakterület egyre önfenntartóbb és egyre jobban önmagába zárkózó közösséggé. Az ilyen balkanizálódás messze viszi a fizikát a természetfilozófiától, ami pedig a természettudomány intellektuális célja.*"

Kevesen fordítják erejüket arra, hogy közérthetően és mély benyomást keltve fejtsék ki az alapvető tudományok szépségét, mélységét, jelentőségét, s nemcsak az új eredményeket, hanem a múlt vívmányait is. Ezt nemcsak az iskolai órákon, hanem könyvekben, folyóiratokban, tévéelőadásokban is meg lehetne tenni. Küzdeni kell azzal a balhiedelemmel szemben, hogy a tudomány anyagias, vallásellenes és lerombolja az erkölcsi értékrendet. Éppen ellenkezőleg! Csak meg kell mutatni a tudomány értékeit! Segítene, ha rámutatnánk az alkalmazott tudományok pozitív eredményeire is, melyek elősegítették a társadalom magasabb életszínvonalának megteremtését, s ha indokolnánk, hogy több tudományos munkára van szükség a környezeti problémák megoldásához.

Zárszó helyett: fohász

Úgy hírlik, egy pragmatikus korszak küszöbén állunk, mely az alkalmazott kutatásokra teszi a hangsúlyt. Lehet, hogy a végéhez közeledik az alkalmazott tudományok évszázada, mely alapvető felismerések sorozatát hozta a kvantummechanika és a relativitáselmélet forradalmával. Ha így lenne, akkor is szükségünk lesz alapkutatásokra, hogy jobban értsük a természetet és önmagunkat, hiszen a tudomány minden része, minden aspektusa összetartozik. A tudomány nem művelhető igazán, hacsak nem magáért a tudásért. Csak akkor maradhat fenn, ha eredményeit az emberiség boldogulására használjuk fel, nem pedig arra, hogy egyik csoport uralkodjék a másik felett. Az emberiség fennmaradása egymás megértésén és a tudáson alapszik. A tudás egymás megértése nélkül embertelen, egymás megértése a tudás nélkül hatástalan.

Victor F. Weisskopf,
Természet Világa (1995. február) 72

Kutató publikálj, csak aztán kapjanak rajta!

Az idézetelemzés határai című cikkében (*Impakt*, 5. évf. 3. szám, 6. old.) *Berényi Dénes* akadémikus olyan alapvető problémákra hívja fel a figyelmet, amelyekkel a tudományos teljesítmények értékelésével foglalkozó testületek és bizottságok egyre gyakrabban kénytelenek szembenézni. E rövid hozzászólásban Berényi professzor észrevételeit kívánom egyrészt alátámasztani, másrészt pontosítani, nyilvánvalóan azonos végső konklúzióra jutva.

A tudománymetria numerikus adatokkal és matematikai statisztikai módszerekkel dolgozik, tehát véleményem szerint feltétlenül kvantitatív tudományágnak minősül. Lényeges észrevenni azonban, hogy a tudománymetria módszerei nem alkalmazhatók automatikusan minden problémára, különösen akkor nem, ha a vizsgálandó probléma nem egyértelműen kvantifikálható. Ennek a paradox helyzetnek legjobb példája az egyéni tudományos kutatási tevékenység értékelése, amely már eddig is heves vitákat váltott ki — sokszor a fogalmak nem kellően tisztázott volta miatt.

A tudománymetria fentebb említett alkalmazása szükségszerűen a tudományos publikációs tevékenység vizsgálatára összpontosít, amely a kutatási tevékenység egyetlen kvantifikálható része. Berényi professzor ezzel kapcsolatban a szerzőség definíciójának nem formális, hanem inkább érdemi többértelműségére hívja fel a figyelmet, és megállapítja, hogy a felmerülő problémák túlmutatnak a tudománymetria alkalmazhatóságának határain.

Többszerzős publikációk esetén valóban felmerülhet az egyes szerzők hozzájárulásának külön-külön értékelése. Véleményem szerint azonban ez már nem a tudománymetria hatáskörébe tartozik, hanem általában a szerzők személyes ügye, más esetekben pedig tudományos etikai kérdéssé válik. Mindenképpen igen veszélyes azonban megkísérelni annak méricskélését, hogy egy közös publikáció kinek-kinek milyen mértékben sajátja!

A tudományos szakirodalom nem ismeri a szerzői hányad fogalmát, és erre való utalások sem szokásosak a szakirodalomban. A jelenlegi gyakorlat szerint egy-egy többszerzős publikáció a szerzők közös szellemi terméke, az arányok közti kismértékű különbözőséget nem szokás (sőt nem illik) hangsúlyozni. Ha egy kiemelkedő tudós úgy érzi, hogy egy munkában őt illeti az alapvető elismerés, akkor etikátlan dolog a csupán nélkülözhető közreműködőt társszerzőnek bevenni. Hasonlóan etikátlan a közreműködőtől is ilyet elfogadni. Egy Nobel-díjas tudós társszerzőjének lenni adott esetben egyenesen csapásnak tekinthető, hiszen a szakmai közvélemény Berényi professzor észrevétele szerint minden érdemet a kiemelkedő tudósnak tulajdonít — sok ismert eset alapján teljesen igazságtalanul!

Az érem másik oldala — az ú.n. "tiszteletbeli szerzőség" — is érdemel néhány szót. Az egyes tudományágakban még elvétve uralkodó feudális szokások szerint a legfelső vezető nevét minden publikáción fel kell tüntetni, függetlenül attól, hogy az érdemi tevékenységet fed-e vagy sem. Bár ilyen esetben a "főtudós" tudománymetriai mutatószámai magasra szállnak, a szűkebb szakmai környezet pontosan tisztában van a valós érdemekkel. Az igen elítélendő etikátlan magatartás csupán a szakmai kollektíva közérzetét befolyásolja hátrányosan.

A tudománymetria alapvető feltételezése — az azonos mértékű szellemi hozzájárulás — az egyetlen, a tudományos etika követelményeivel összeegyeztethető megoldás. Ha valaki saját hozzájárulását tartja mindenek felett valónak, ne írjon közös publikációt!

Külön kell említeni a modern kísérleti részecskefizika publikációs gyakorlatát, amelynél nem ritka a sok ország intézményét képviselő többszáz szerzőt felsoroló publikáció sem. Ilyen esetekben maga az a körülmény, hogy egy-egy ország kutatócsoportja részéről hány szerző szerepelhet, az erőviszonyoktól függő bonyolult alkudozások tárgya, és a szerzőség végső fokon nincs feltétlenül összefüggésben a szakmai érdemekkel. Ez a probléma az egyéni tudományos teljesítmény értékelésénél még mindig sok gondot okoz, és ezideig nem ismeretes megnyugtató megoldása.

Berényi professzor felveti még azt a kérdést is, hogyan lehet a "tartalmilag" előrevivő eredményeket kvantitatív módon megkülönböztetni a széles körben használt technikai trükköktől, vagy táblázatoktól, szintén buktatókat rejt magában. Ha ugyanis egy technikai trükköt vagy táblázatot sokan találnak hasznosnak, és így az eredeti publikációt sokan idézik, ez egyértelműen a tudományos közösségre gyakorolt magas "impaktról" tanúskodik. Nem árt emlékezni arra, hogy a maga korában a Gregory Breit és munkatársai által készített első Coulomb-függvény táblázat milyen hatalmas mértékben segítette az atommagreakciók kvantitatív vizsgálatát.

Feltétlenül egyet kell azonban érteni Berényi professzor megfogalmazásával: "a tudományos teljesítmények értékelése mindig az eredmények hatásának vizsgálatát jelenti". Nyilvánvaló módon a különböző követelményrendszereket is ehhez az alapelvhez kell igazítani. Ez a tény azonban véleményem szerint nem a tudománymetria határait ostromolja, hanem arra hívja fel a figyelmet, hogy az eddigieknél jóval precízebben kell megfogalmazni a tudományos tevékenység ismérveit, amelyek optimális esetben egyértelmű, de mindenképpen nem triviális szakmai tartalommal bíró kvantifikálást tesznek lehetővé.

Bencze Gyula, KFKI, RMKI

A celebration intellect

Humans too rarely celebrate intellectual excellence with pomp and ceremony and reverence. The Nobel Prizes are a dazzling exception.

The Nobel Prizes, with exception of the peace prize, are awarded in Stockholm in December on of the anniversary of Alfred Nobel's death. The Nobel Foundation invited this reporter to participate in the 1994 Nobel festivities.

The Nobels in Chemistry, Physics, and Medicine or Physiology are no longer the richest prizes awarded for achievement in the sciences. But they remain the most prestigious. The history and tradition of the Nobel Prizes, and the still substantial amount of money associated with them, ensure that they will retain that status.

But more than tradition or monetary value maintains the preeminence of the Nobels. The weeklong Nobel Prize festivities are an unparalleled national celebration of intellect that honors, in addition to the Nobel Prize winners themselves, the spirit that underlines all human intellectual achievement.

Sweden, as Swedes will almost compulsively tell you, is a small nation. About the size of California, it has 8 million residents. Though relatively small, Sweden is, of course, an advanced, industrialized nation.

Despite its sophistication, Sweden increasingly realizes that its future prosperity depends on interactions with other developed nations. Last year, Swedes voted to join the European Union. And Swedish scientists consider their regular contacts with other Western scientists, particularly those in the U.S., as vital to their research. But the Nobel Prizes set the Swedes apart, and they know it.

People grumble about the selection of the Nobel Prizes. The Nobel Committees are too secretive. Prizes are awarded too long after the research that led to them was carried out. Prizes sometimes are awarded to the wrong scientists.

There is some validity to the complaints, but not much. The Nobel Committees do operate in strict secrecy, but considering the importance of the prizes and the intense interest of scientists in them, it is hard to imagine how else the committees could operate. The selection process may be secretive, but the nominating process (yes, Nobel Prize winners must be nominated) draws on the wisdom of some 1,000 chemists worldwide for the chemistry prize alone.

Sometimes it does seem to take the Nobel Committees a very long time to recognize particular research contributions. Perhaps this year's prize in physics awarded for work on neutron diffraction techniques begun just after World War II, was a bit late.

On the other hand, what would the critics have the Nobel Committees do? Ignore Barbara McClintock's seminal research in genetics because almost no one understood its significance for some 30 years? And the flip side of this argument isn't pretty either — honoring work instantaneously runs the risk of bestowing the Nobel imprimatur on research that later turned out to be, at best, insignificant and, at worst, just plain wrong.

The presentation of the Nobel Prizes — first physics, then chemistry, medicine or physiology, literature, and finally economics — in Stockholm's Concert Hall is a string event. Before each presentation, a member of the Swedish academy that selects that Nobel Prize winner discusses the research that led to the award. The winners are invited to receive their prizes from Sweden's king.

And the Nobel banquet that follows is a fantastic affair. Fifteen hundred guests in formal evening attire assemble in the Blue Hall of Stockholm's City Hall. The prizewinners are led arm-in-arm with Sweden's king, queen, and members of the royal family into the hall while choir sings. The four-course meal is served with remarkable precision.

Perhaps the involvement of Sweden's king and queen in the Nobel festivities should not be so significant, but it is. The royal family provides the ceremonies a solemnity no elected official, in this cynical age, can.

U.S. President, in their invitations to the White House for example, seem to make no distinction between the players who win the NCAA basketball tournament and the scientists who win Nobel Prizes. All receive the same 15 minutes in the presidential spotlight. The U.S. National Medals of Science, although prestigious, are not awarded annually and have never captured the national imagination the way the Nobels captivate Sweden. The king and queen of Sweden, and Sweden as a nation, seem to draw a sharper distinction between athletic and intellectual achievement than we do.

The outcome of an athletic endeavor has no importance to humanity. The difference between a sporting event, or any other entertaining diversion, and the work of George A. Olah, this year's Nobel Laureate in Chemistry, or the work of Clifford G. Shull and Bertram N. Brockhouse, this year's Nobel Laureates in Physics, is vast. The work of scientists is the bedrock upon which the future will be built.

It seems obvious, but in so many instances, it's not. In Stockholm, at least for a week in December, it is obvious. There, scientists, economists, and a man or woman who creates literature take center stage, honored as no entertainers or athletes, no matter what their talents, ever are.

Rudy Baum,
C&EN, (January 30, 1995) 34

Science Advice At The White House

"The President's Scientists: Reminiscence of a White House Science Advisor," *by D. Allan Bromley, Yale University Press, Box 209040, 302 Temple St., New Haven, Conn. 06520-9040, 1994, 273 pages, $30.*

The popular image of a presidential science adviser is usually that of a pipe-smoking theoretician sitting in a cluttered office at a university somewhere, unconcerned with the messy world of Washington politics. In fact, the advisers who have served Presidents since the 1950s have not been remote, passive, or apolitical. Since formal establishment of the office, the President's chief scientist has been expected to understand, accept, and play by the same rules as the rest of the White House staff: Science may recommend a decision, but prudent partisan politics will dictate the choice.

By centering on this elementary yet controversial truism, D. Allan Bromley enlivens and deepens an otherwise unremarkable autobiographical account of science in the Bush Administration. In "The President's Scientists: Reminiscences of a White House Science Advisor," Bromley deftly intersperses statesmanlike discussions of policy with surgical dissections of operational politics. The technique is both jarring and effective.

Its mix of aloofness and subtle revelation echoes Charles Wilson Peale's famous self-portrait, "The Artist in His Museum" (Pennsylvania Academy of Fine Arts, 1822), in which an imperious Peale lifts a curtain to reveal a gallery filled with biological and paleontological specimens. Like Peale, who believed museum visitors would draw natural history lessons from uninterpreted "cabinets of curiosities," Bromley intends that fellow scientists and other readers draw broader conclusions from his anecdotes. Time and again, he emphasizes that the science adviser must always be the President's scientist, not the scientists' man (or, someday, woman) at the top. The adviser must provide the best objective technical advice, knowing full well that the Administration may ignore it.

Many observers argue that development of nuclear weapons demanded establishment of a science adviser. As Bromley implies, however, the tradition of the President having such an adviser is much longer. Abraham Lincoln frequently consulted Joseph Henry, first secretary of the Smithsonian Institution, on a wide range of technical issues. During the Civil War, Lincoln also relied on a Navy ordnance specialist for advice.

By mid-20th century, science and technology in general had demonstrated sufficient national importance to warrant permanent status at court. Leaders of the scientific community, tired of struggling to affect policy from the outside, had also realized that achieving political influence would require their active involvement in partisan national politics.

So, from Oliver Buckley, the first official appointee in 1951, and James Killian, who followed him in the Eisenhower Administration, through John H. Gibbons, who hold the post today, the President's adviser has publicly shared the ideology of the party in power. When Yale physicist Bromley accepted nomination as assistant to the President for science and technology and director of the Office of Science & Technology Policy (OSTP) in 1989, he entered this world and temporarily relinquished his academic independence.

Bromley clearly believes that presidential science advisers have been of dubious benefit to the research community. Access to the Oval Office, he argues, has not necessarily translated into more money for science. Science's loss, however, has been the republic's gain. Proximity ensures that a President can obtain reliable, tailored, informal, and confidential advice at a moment's notice. To provide that service in an era of leaks, snoopy journalists, fast-changing foreign relations, and political second-guessing is important and difficult. To provide it well may mean contradicting the consensus, advice, and even the best interests of the scientific community. "[If] there is the slightest suggestion that the Advisor is functioning as a lobbyist for [the science and technology] communities," Bromley observes, "then the Advisor's effectiveness and acceptance in the President's inner circle will vanish." He characterizes President Nixon's parting comment to his science adviser, Edward David — "You represented the scientists well" — as "definitely the kiss of death for a Presidential Advisor."

Bromley sees many signs of increasingly politicized science, such as the changing, composition, at least, of the President's Council of Advisers on Science & Technology (PCAST). In the Bush Administration, which changed the name of this group to PCAST to distinguish it from its predecessor PSAC (President's Science Advisory Council), failed to utilize the council effectively. Like all advisor groups without assigned responsibilities or power, its members — despite their considerable prestige — were helpless to reverse the situation. President Clinton's PCAST appointments signal significant change in the voices around the table — there are now many powerful and accomplished women and minorities, and representatives of science-based industries on the council. But there's no indication yet that this PCAST will move beyond continued ceremonial validation on Administration choices.

Bromley's second indicator of change is improved communication and cooperation between OSTP and two important power centers in Washington: the federal agencies and Congress. The White House office was never intended to manage research or dispense federal monies for it, and Bromley's opposition to placing the Critical Technologies Institute within the White House reaffirmed that position. In his view, OSTP's role is to facilitate communication to and from industry and the research community (through the scientific and engineering societies), not to initiate projects.

Improving government-industry linkage — a Bromley priority — conformed well to the Bush political agenda, and, despite criticism from some industry leaders, Bromley obviously regards establishment of various OSTP committees and reports on these links as one of his prime accomplishments.

Like many another political observer today, Bromley would like to remodel Congress. Its reorganization, he writes, is "one of the most urgent and important challenges that the nation faces." He also condemns the practice of earmarking projects for funding without normal peer review. If present trends continue, he asserts, the practice "will destroy the quality on which [U.S.] leadership in world science and technology depends." To his credit, however, he worked vigorously to repair relations between OSTP and the Hill, and was well received by congressional science and technology doyens, who considered him to be a scientist's scientist and respected his broad understanding of all research fields. President Clinton's appointment of the former head of the congressional Office of Technology Assessment as his science adviser signals more legislative cooperation in furtherance of the Administration's goals.

Bromley writes that he deliberately deemphasized national security issues in OSTP, leaving the field to Bush's national security adviser, Brent Scowcroft. Bromley defends the decision as partly due to insufficient OSTP staff and budget, but he seems uneasy with the resulting fragmentation of advice on national security. Targeting of expertise and staff can improve performance quality in some settings, but national security issues today are inherently broad based. They extend beyond the deployment of weapons along borders and can involve public health, information sharing, and industrial technology. They demand wide-ranging technical expertise. Not every future President is likely to have a Vice President who can surf the Internet and spell chlorofluorocarbon, as President Clinton does in Al Gore. Artificial divisons among top-level structures for receiving advice on national security could endanger decision-making by marginalizing scientific and technical information just where and when it is needed most.

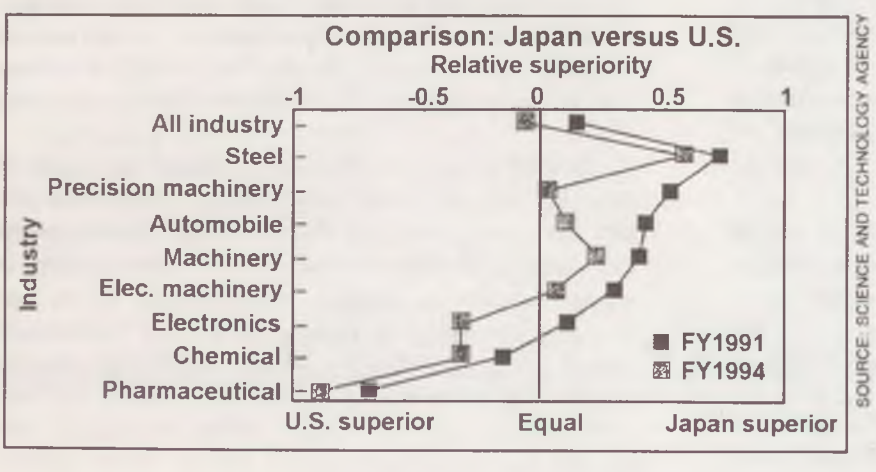
Finally, Bromley seems to believe that he "failed miserably" to convince the State Department that science and technology are an integral part of international relations. History offers him some consolation: This political stand-off long predates him. Its roots go back to the 18th century, when science and technology were pegged as tools for national development, not strategies for diplomatic portfolios. Global environmental controversies have recently raised the overall profile of scientific enterprise among diplomats, but real change can only be initiated from within the State Department and in schools of international studies, where science and technology policy courses and programs struggle for recognition.

Despite the satisfaction of having served both President and country well, and in the process having sustained a reputation for integrity in office, Bromley probably returned to Yale in 1993 with considerable relief. In its brutal assessment of White House political jockeying, his account echoes the sentiments of 19th-century physicist Henry, who wrote in 1861 that "Of all places in the country Washington is I think the worst in which to [pursue] scientific investigations. The constant drudgery and anxiety of an office unfits a man for profound and continuous thought; and as he is under the restraint of the sentiment of the dominant party he finally looses [sic] his mainly independence and that love of truth which constitutes an honest man."

Bromley can relax now; once again he's a scientist's scientist, able to concentrate on pursuing scientific truth rather than dodging political bullets. In "The President's Scientists" he makes a compelling case for national service, however, demonstrating that science serves society best when it participates as an informed political partner instead of standing and complaining from the outside.

Marcel C. LaFollette,
C&EN, (January 30, 1995) 35

Japan Worries About High-Tech Lags



Japan's high-tech elite feel they are being overtaken by the United States and other countries in electronics, automotive design, and other areas, according to a survey conducted last fall by Japan's Science and Technology Agency (STA). And it's a dramatic change from a similar survey conducted in 1991, when high-tech companies felt they were comfortably ahead of the United States in everything but chemicals and pharmaceuticals (see chart).

(Continued on next page)

Improvements in U.S. productivity and efficiency in machinery, autos, and electronics are termed "remarkable" by STA's annual White Paper, released last November. An official notes, for example, that the U.S. auto industry has turned itself around both by improving quality and by more aggressively marketing its wares in Japan. As for electronics, he noted that U.S. computer makers have led the trend toward workstations and personal computers, while Japanese companies were still focused on mainframes (Japan is still strong in PC components, but U.S. companies dominate the market for overall systems.) Something of a puzzle, even to the STA, in the change in the area of precision instruments, a category that includes advanced medical devices, where the United States is strong, and cameras, where the United States is not even in the picture.

The about-face, in part, reflects larger economic cycles: In 1991, the United States was in a recession while Japan was riding high; now Japan is just pulling out of a protracted recession, and the bloated yen is making it difficult to buy research as well as sell goods abroad. Yasutsugu Takeda, who is in charge of research and development at Hitachi Ltd., noted that Japanese firms are just now beginning the kind of restructuring that U.S. industry has been undergoing for several years. "Now we have to make greater efforts," says Takeda — especially with the Asian tigers also breathing down Japan's neck. Half the companies in the recent survey said the competitive environment is going to get more rugged as other Asian economies expand; about one third predicted that it will only be 3 to 5 years before Chinese companies are in full roar.

Science 267 (10 February 1995) 788

Dynamic Science And The Steady State

"Prometheus Bound: Science in a Dynamic Steady State," by *John Ziman*,
Cambridge University Press, 40 West 20th St., New York, N.Y. 10011-4211, 1994, 289 pages, 24.95

On June 30, 1994, President Bill Clinton convened the first meeting of the National Science & Technology Council (NSTC), the science policy equivalent of the National Security Council. Twenty-four members — Cabinet secretaries, agency heads, and senior White House officials — gathered together to ponder how the federal government's $75 billion R&D budget might be better managed to meet Presidential priorities. NSTC established nine committees, forming what Vice President Al Gore called "a virtual agency".

Whether the new body will work as intended remains to be seen. That it exists at all points up the reality facing science and technology today — the effort to manage this far-flung establishment at the highest levels of government. That reality is one of the consequences of what John Ziman calls steady-state science.

In "Prometheus Bound: Science in a Dynamic Steady State," Ziman defines and explores the implications of steady-state science. An emeritus professor of physics at the University of Bristol, England, and a long-time observer of relations between science and society, Ziman believes that the scientific enterprise, after expanding for centuries, is reaching its limits. The budget crunch being felt by most industrialized nations is not the only reason for this; forces within science contribute equally to its undoing. More and more scientists want more and more money to extend the endless frontier. "We can no longer afford the ever-increasing cost of exploring ever-wider research opportunities," he says.

The new era is in contrast with the expansionist science that typified much of the period since World War II, Ziman points out. In an era of science expansion, budgets rise, and new scientists are educated and find stable jobs. Basic science is favored over applied research, and universities are supported via substantial grants that pay indirect costs. Research is discipline oriented. Governance structures permit "bottom-up" approaches in which peer review is widely valued. Broad accountability is the norm, and there's little government interference. Scientists, who make many of the key decisions concerning science policy, have little need to make explicit choices about scientific priorities.

When did expansionist science give way to a steady state? Ziman believes it started in the mid-1970s, when R&D activity in most developed countries hit between 2 and 3% of national income, a level it has maintained ever since. With more scientific mouths to feed and science requiring ever-more sophisticated and expansive technology, it was inevitable that eventually there would be limits to how much science could grow. It just happened sooner than many observers expected. "The likely prospect," Ziman believes, "is that science will have to exist for the future within a fixed or slowly growing envelope of resources."

The author discusses the transition from expansionist science to the new, steady-state regime. He talks about how resources are allocated and institutions respond to change. He addresses the impact of steady-state science on scientific careers and globalization. As a British scholar, however, he doesn't give as much attention to the end of the Cold War and its role in the evolution of science policy in the U.S. as most American readers might wish. For 50 years, much science was justified in the name of national security. Under this umbrella, science was well funded, and scientists had a great deal of freedom in their research. Now, U.S. agencies that thrived during the Cold War period are searching for new missions, and they do not have the money or the inclination to support science that they had in the past.

Limits mean topside management in any institution, including government. Thus, buzzwords such as accountability, evaluation, priority setting, and strategic or targeted science come into fashion. The issue is not the words, however, but the deeds that ensue. Can a balance be found between the need to choose research priorities when resources are scarce and the value of allowing some slack in the system for unfettered inquiry?

As his subtitle implies, Ziman sees the steady state as a dynamic one that allows considerable activity within certain boundaries. Improvisation takes place as government or other policy-makers learn how to cope with the new era. Science policy literature offers little theory to guide them, so they're more likely to learn from their own successes and failures.

Increased emphasis on coordination of federal science support is one example of steady-state policy-making. The basic purpose of President Clinton's NSTC is to coordinate science activities. Such coordination, useful even when science is expanding, becomes a necessity when funds are short. Many large-scale science programs such as study of the greenhouse effect, oceanography, and remote sensing cut across various government agencies. Coordination is intended to avoid duplication and bring a measure of coherence to multi-agency activities. Some believe coordination would be best achieved by reorganizing existing agencies, creating new ones (such as a National Institute of Environment), or creating a coordination czar position. Others support the idea of an interagency coordination body such as NSTC.

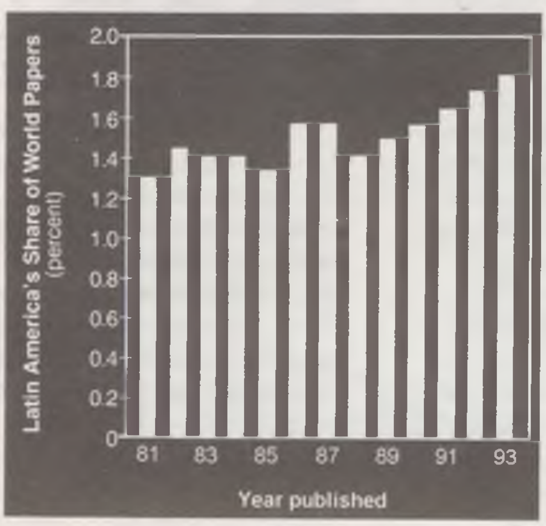
Steady-state science policy for global change, biotechnology, high-performance computing, and materials — priorities of the Bush Administration — was coordinated by the Federal Coordinating Council on Science, Engineering & Technology. FCCSET linked comprehensive science planning with cross-agency budgeting, and these priority fields were able to expand their science, provided they were coordinated by the "FCCSET process."

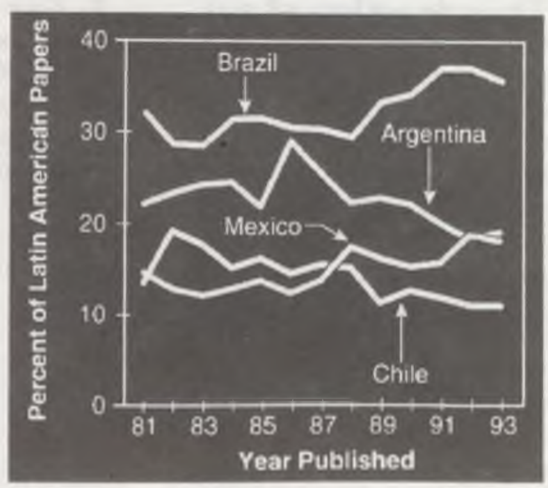
Now the Clinton Administration is elevating the FCCSET process to coordinate practically all federally funded R&D. This is a prodigious leap, and many science watchers are sceptical it will work. But it represents this Administration's ambition and perhaps sense of urgency to manage federally funded science and technology in the national interest. This scenario is symbolic of the dynamism of which Ziman writes, as well as of the improvisation of government science managers.

Ziman's book is a good overview of what is happening in contemporary science policy. It is provocative, showing why more conscious efforts in making choices are taking place. It carefully points out the pitfalls that come from trying to micromanage so creative and freewheeling a process. We might wish for the golden years of expansionist science, but the steady state is here. We have to govern it carefully to make the most of limited resources. That won't be easy.

W.H. Lambright,
C&EN, 41-42, November 14, 1994

Publication Trends: Uneven Growth





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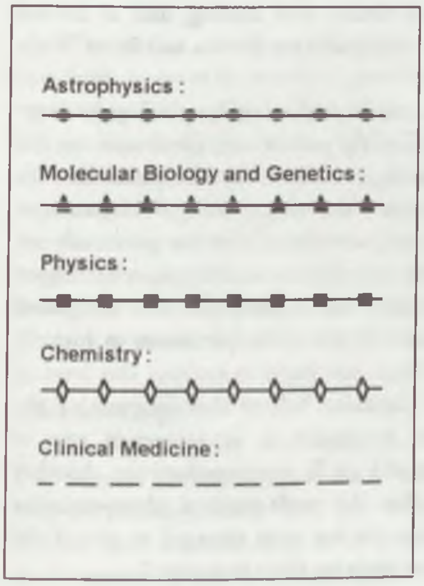
In 1993, almost 13,000 papers with at least one author from Latin America made it into the world's elite scientific literature — the 3300 journals indexed by the Philadelphia-based Institute for Scientific Information (ISI). Although Latin American papers account for only about 1.8% of the total number of papers in those journals, an analysis performed for *Science* by ISI shows that since 1981 their share has risen substantially, from about 1.3%. Four countries — Brazil, Argentina, Mexico, and Chile — account for about 85% of those top Latin American papers (see charts below).

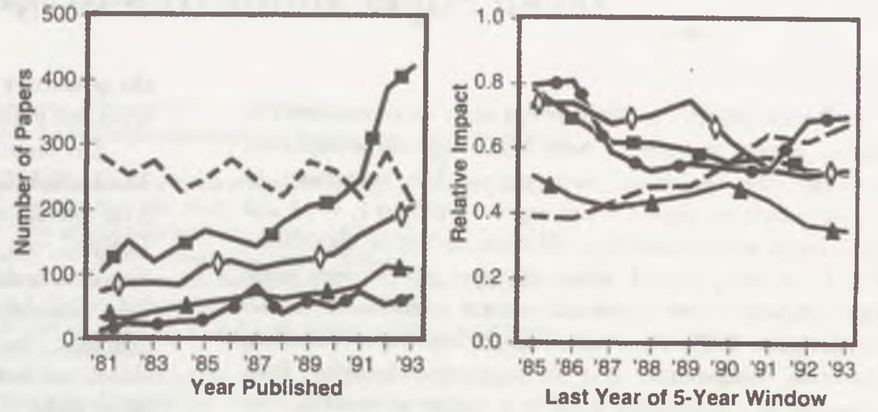
The eight charts to the right show publication trends in five disciplines in the big four countries. The data, from ISI's National Science Indicators On Diskette, show a wide variation in growth rates in different fields in each country. They also indicate that papers from these countries are cited on average 40% to 60% less often than the world average for papers in the same field. (Relative impact is a comparison with the world average, which is set at 1.0) One exception is astrophysics papers with Chilean authors, which score higher than the world average.

Science 267 (10 February 1995) 808

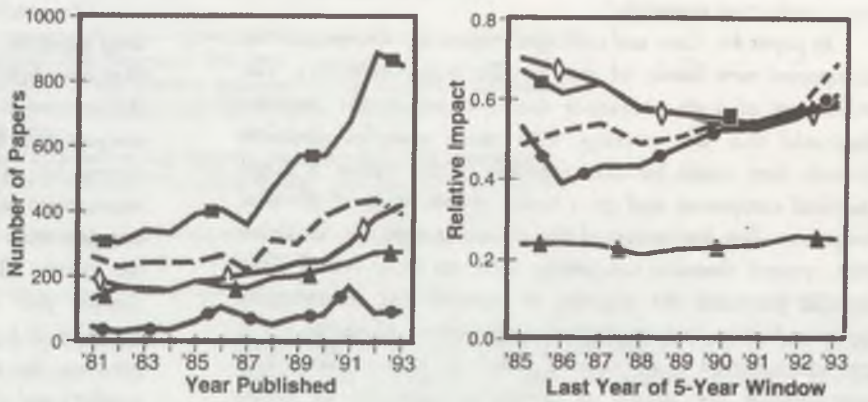
Note: Because the scales of the graphs differ, comparison between countries should be made with caution.



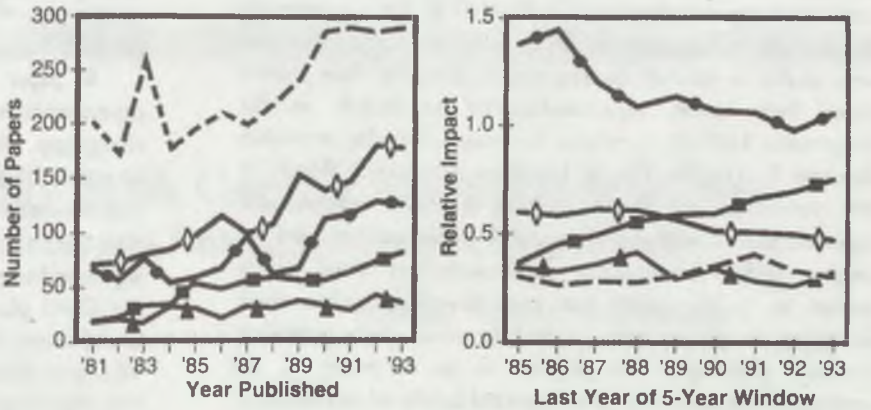
Mexico



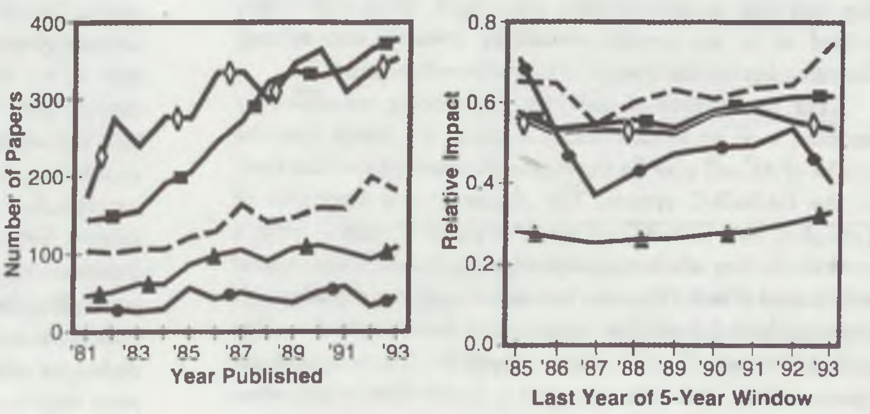
Brazil



Chile



Argentina



Seven Superconductors Reign Supreme

Seven papers on high temperature superconductivity swamp physics this period, with cosmology and astrophysics snatching the remaining three places. The supremacy of superconductivity papers, which capture positions 2, 4-7, 9 and 10, is neatly encapsulated by a reflection for *Science Watch* from Bob Cava, the principal author of paper #4: "Copper oxide superconductors have dominated research in condensed matter physics since 1987. Their astoundingly high superconducting transition temperature (T_c) took superconductivity from obscurity to a household word in a matter of months. The early progress was driven primarily by the discovery of new superconducting materials."

In paper #4, Cava and colleagues report the discovery of an unexpected new family of intermetallic superconductors. The attainment of high transition temperatures in the cuprates illustrated that by working with more complex chemical systems they could balance opposing forces within a single chemical compound and get a better optimization of physical properties. But for many of the desired properties, materials with optimal chemical complexity have yet to be found. This thought provided the impetus to research the intermetallic superconductors "whose study has languished in recent years, and which almost never show T_c above 15 K, says Cava. The intermetallics are almost all binary compounds, or rarely tertiary compounds. The group responsible for #4 recently announced superconductivity, at T_c = 23 K, for a quaternary intermetallic system, yttrium-palladium-boron-carbon, but they were unable to identify the responsible phase. In their present paper they report superconductivity at 16.6 K in the compounds $LnNi_2B_2C$, where Ln stands for the rare-earth elements Y, Tm, Er, Ho, or Lu. Cava tells *Science Watch* "A few years ago we began looking for new intermetallic superconductors with the same search techniques we used for copper oxides. Intermetallic superconductors were widely studied up to the 1970s but were abandoned when their increasing complexity went beyond the point where prevailing synthetic philosophy was prepared to go. In paper #4, we announced the discovery of unexpected family of intermetallic superconductors, some of whose members have T_c as high as any ever seen in intermetallics. Our paper, along with others related to it, are greatly revitalizing research into a long-forgotten but fruitful branch of new materials research."

For an example of one Hot Paper being stimulated by another, look no further than newcomer #7, which cites the results of #4, and goes on to describe two new phases that form in the Lu-Ni-B-C system. The elegant crystal structures of $LuNi_2B_2C$ and LuNiBC are given in paper #7, and — what a surprise — they are for quaternary compounds. Theo Siegrist tells *Science Watch* "The new intermetallic phases showing high superconducting transition temperatures from a new class. The crystal structure is remarkably simple for a true quaternary system where each atom occupies a distinct lattice site; often the quaternary phases are based on ternary or binary structure types, but in this case we have true quaternaries."

Dr. Siegrist describes the crystals as chemically layered. The $LuNi_2B_2C$ is a stacking sequence of one LuC rocksalt type layer alternating with one Ni_2B_2 inverse Pb_2O_2 type layer. In LuNiBC two LuC layers alternate with one Ni_2B_2 layer. Siegrist adds that "variations in the stacking sequence establish a new homologous series $(LuC)_m(NiB)_n$. The chemical layer character, the mixture of light and heavy elements, strong directional bonding effects and the possibility of homologous series make this new class of compounds akin to the layered cuprate superconductors."

The mechanism of high T_c superconductivity remains a deep mystery, but a group at Stanford University may have shed new light on the problem by studying photoemission in the cuprate $Bi_2Sr_2CaCu_2O_{8+\delta}$, for which T_c = 88 K. In the conventional BCS theory of superconductivity, the current is carried by pairs of electrons called Cooper pairs. The total momentum of a pair is not changed when one of its electrons interacts with the crystal lattice of the superconductor, and so the current flows indefinitely. The spatial coherence of the Cooper pair is described by a spherically symmetric wave function. In high T_c the electron pairs are different: the distance between the charge carriers in each pair is about 100 times smaller, and the wave function has higher-order symmetries. One of the key quantities describing the Cooper pair is the energy gap, which characterizes the energy required to break the pair.

In paper #2, Zhi-Xun Shen and his colleagues describe experiments on a superconductor with an anomalously large energy gap, which reaches about 20 meV along the Cu-O bond direction. This is at least an order of magnitude larger than in conventional superconductors. But that's only half the story. In this case, high-resolution photoemission has shown that the superconducting energy gap varies as a function of angle along the Cu-O plane. This is totally new finding, and so *Science Watch* asked Shen and colleagues Dan Dessau and Barry Wells what it might mean.

"We found the gap in $Bi_2Sr_2CaCu_2O_{8+\delta}$ is highly directional dependent. This finding puts strong constraints on the pairing mechanism for superconductivity. In particular, the anisotropy we found is consistent with a strong *d*-wave component in the wave function, something that has previously received a great deal of theoretical interest. Our paper has helped spur a great number of other experiments that have the goal of establishing the symmetry of the order parameter in high T_c superconductors. We think our paper is exciting this level of interest because many physicists believe that determining the behavior of the order parameter is an important step to unraveling the mystery of high T_c superconductivity. Another reason, of course, is that the angle-resolved photo-emission technique which we describe has now emerged as one of the most important tools for studying these materials."

What is hot in physics...

Rank	Paper	Citations This Period (Sep-Oct 94)	Rank Last Period (Jul-Aug 94)
1	**G.F. Smoot and 27 others**, "Structure in the *COBE* differential microwave radiometer first-year maps," *Astrophys. J.*, 396(1):L1-5, 1 September 1992. [11 U.S. institutions]	56	1
2	**Z.-X. Shen and 13 others**, "Anomalously large gap anisotropy in the *a-b* plane of $Bi_2Sr_2CaCu_2O_{8+\delta}$," *Phys. Rev. Lett.*, 70(10):1553-6, 8 March 1993. [Stanford U. Calif.; Los Alamos Natl. Lab., N.M.]	22	6
3	**G. Schaller, D. Schaerer, D. Meynet, A. Maeder**, "New grids of stellar models from 0.8 to 120 M. at Z=0.020 and Z=0.001," *Astron. Astrophys. Suppl. Ser.*, 96(2):269-331, 1 December 1992. [Geneva Observ., Sauverny, Switzerland]	21	+
4	**R.J. Cava and 12 others**, "Superconductivity in the quaternary intermetallic compounds $LnNi_2B_2C$," *Nature*, 367(6460):252-3, 20 January, 1994. [AT&T Bell Labs, Murray Hill, N.J.; Delft U. Technol, Netherlands]	21	+
5	**A. Schilling, M. Cantoni, J.D. Guo, H.R. Ott**, "Superconductivity above 130 K in the Hg-Ba-Ca-Cu-O system," *Nature*, 363(6424):56-8, 5 May 1993. [ETH Hönggerberg, Zurich, Switzerland]	20	10
6	**S.N. Putilin, E.V. Antipov, O. Chmaissem, M. Mareziqa**, "Superconductivity at 94 K in $HgBa_2CuO_{4+\delta}$, *Nature*, 362(6417):226-8, 18 March 1993. [Moscow St. U., Russia; Crystallograph. Lab., CNRS-UJF, Grenoble, France]	19	5
7	**T. Siegrist, H.W. Zandbergen, R.J. Cava, J.J. Krajewski, W.F. Peck**, "The crystal structure of superconducting $LuNi_2B_2C$ and the related phase LuNIBC," *Nature*, 367(6460):254-6, 20 January 1994. [AT&T Bell Labs, MurrayHill, N.J.; Delft U. Technol, Netherlands]	19	+
8	**E.L. Wright and 20 others**, "Interpretation of the cosmic microwave background radiation anisotropy detected by the *COBE* differential microwave radiometer," *Astrophys. J.*, 396(1):L13-8, 1 September 1992. [10 U.S. institutions]	18	4
9	**W.N. Hardy, D.A. Bonn, D.C. Morgan, R.X. Liang, K. Zhang**, "Precision measurements of the temperature dependence of lambda in $YBa_2Cu_3O_{6.95}$: Strong evidence for nodes in the gap function," *Phys. Rev. Lett.*, 70(25):3999-4002, 21 June 1993. [U. British Columbia, Vancouver, Canada]	17	+
10	**D.A. Wollman, D.J. Van Harlingen. W.C. Leem, D.M. Ginsberg, A.J. Leggett**, "Experimental determination of the superconducting pairing state in YBCO from the phase coherence of YBCO-Pb dc QUIDS," *Phys. Rev. Lett.*, 71(13):2134-7, 27 September 1993. [U. Illinois, Urbana-Champaign]	17	+

Source: ISI's Hot Paper Database

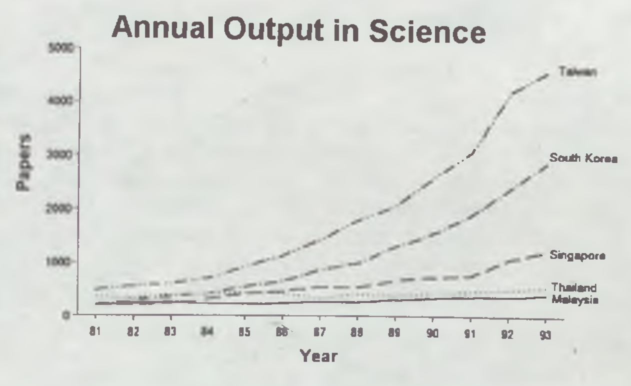
The search for the pairing mechanism is taken up in paper #10, which point out that although higher-order symmetries are possible, the experimental evidence points to *s*-wave and *d*-wave states. Paper #10 described an experiment designed to give an unambiguous determination of the pairing state in YBCO, in which the conclusion is that the pairing state has *d*-wave symmetry.

Simon Mitton, Science Watch, 6 (2)(1995) 6

Asian Tigers Steadily Stalk Greater Scientific Standing

Singapore, South Korea, and Taiwan Up Output Significantly

The nation of Singapore, while small by geographical standards, seems to be steadily increasing in scientific stature. In updating a 1993 report on the nations of the Pacific Rim (see *Science Watch*, 4[3]:7, March 1993), *Science Watch* turned to ISI's National Science Indicators on Diskette, 1981-1993, which is a compilation of summary publication and citation statistics for the sciences overall and in 17 separate fields. The data suggest that Singapore, along with its neighboring nations known as the "Tigers of Asia," is contributing in increasing measure to the world's elite scientific literature.



Upturn in Output

These countries are only now entering into the international community in terms of scientific publication, and they are gaining ground. First, they are simply producing more. Second, their scientists seem to be publishing more frequently in the scientific journals indexed by ISI.

As shown in the graph, the Tigers have published more papers each year over the past 13 years. Taiwan led the others in 1993 with a total of 4,630 papers and was followed by South Korea with 2,915, Singapore with 1,220, Thailand with 539, and Malaysia with 419.

In fact, the rate of increase for papers from the Tigers of Asia is greater than the rate of increase of the ISI database as a whole. A look at the change in percentage of the ISI database held by each of these five nations in 1981 and in 1993 shows that Taiwan rose from 0.12% in 1981 to 0.83% in 1993, South Korea rose from 0.06% to 0.52%, Singapore increased from 0.04% to 0.22%, and Thailand and Malaysia moved from 0.09% and 0.05% to 0.10% and 0.08%, respectively.

Narrowing the Scope

Three of the five nations stand out for their increased output during the last decade in ISI-indexed journals. Here, we focus on Singapore's surge in publication. Its number of papers published in all fields of science totalled just 167 in 1981, while in 1993 the total was 1,220. As shown in the charts on page 12, Singapore is making its mark in a number of different areas. The charts illustrate Singapore's relative citation impact (citations per paper compared to the world average) by field. When grouped according to physical sciences; plant, animal, and earth sciences; and biological and medical sciences, certain trends become apparent.

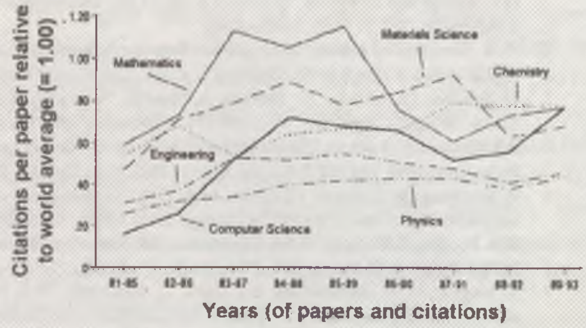
Of the three groupings, the physical sciences are the strongest performers for Singapore with chemistry, materials science, and mathematics leading the way. Singapore's advances in physics had been steadily increasing to a high point of 0.43 in 1991, but took a dip to 0.38 in 1992. Singapore scientists have already begun to pick up the slack, however, and have improved to a score of 0.44 in this field. In the next grouping, both agricultural sciences and plant and animal sciences are the standouts. The biological and medical sciences are still trailing the other groupings, but both biology and biochemistry and molecular biology and genetics are on a sharp upswing. Output in the fields of astrophysics, immunology, and geosciences remains modest, so citation impact data are not illustrated for these fields.

In examining which institutions in Singapore accounted for the nation's growing share of the literature, *Science Watch* detected the unmistakable dominance of the National University of Singapore (NUS). Inspection of Singapore's most-cited papers from 1981-93 — on notable topics such as wave packet propagation, disposition of ibuprofen enantiomers, and use of DNA probes for prenatal diagnosis of cystic fibrosis — indicates that they were authored or coauthored by scientists at NUS in the vast majority of cases. In fact, 7,491 of the 9,685 total papers published during the period were authored or coauthored at NUS, which makes for 77% of Singapore's total.

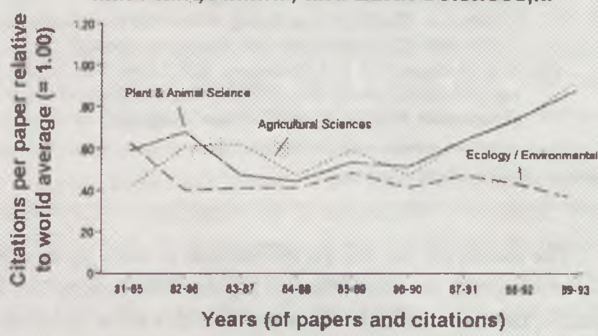
Another vital link to the wider scientific world for Singapore is through the many World Health Organization Collaborating Centres that operate through the NUS. The number of countries that participate in each study varies according to the nature of the investigation, but the information from Singapore is valued for a number of different topics. A few of these study areas are human reproduction, histology, and enteroviruses.

Relative Citation Impact of Singapore

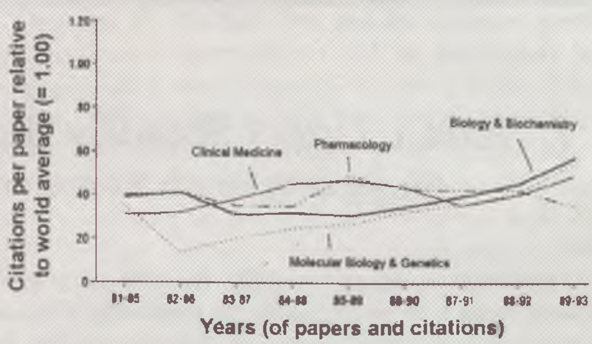
in the Physical Sciences,...



...in Plant, Animal, and Earth Sciences,...



...and in the Biological and Medical Sciences



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IMPAKT — Tények a tudományos alapkutatásról
Felelős kiadó: az MTAK főigazgatója
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Készült az *Argumentum Könyv- és Folyóiratkiadó Kft.* nyomdájában

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