On the Self-Image of Chemists, 1950-2000

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Abstract: The field of chemistry is highly diverse. Yet, the aggregate picture of chemists, according to this study, shows them to constitute a highly homogeneous and even gregarious group, in terms of their self-image. They see themselves as creative, as benefactors of humankind, and as craftsmen upholding a tradition of intelligent hands and preserving, even in the time of Big Science, a relatively low-tech profile. The stereotypical public image as the sorcerer's apprentices who befoul the environment and who manufacture chemical weapons is way off target. Chemists find it a caricature, it only reinforces the good conscience within the chemical community. Other conservative forces are the common language of structural formulas, a widespread phobia about mathematics, and the very length of the apprenticeship to be served. Conversely, between the mid-twentieth century and the advent of the twenty-first century, chemists displayed an impressive adaptability in the face of swift changes, regarding the tools of the trade – which the NMR Revolution had contributed to upgrade –, the funding of their activity at a much higher level, the oil crises, and the Biological Turn that affected them during that period.

Keywords: *self-image of chemists*, *20th century*, *chemophobia*, *instrumental revolution*, *biological turn*.

1. Introduction

Why should one attempt to characterize the self-image of a group of scientists, during a relatively recent period? And how to go about it? A tough assignment.

The answer to the former surely has to involve the relationship between the group's production of new knowledge (in the form of publications, patents, and textbooks for students) and the prevailing culture: I cannot do better to define this otherwise vague and catch-all entity than to refer to anthropological practice. I adopt a by now classic characterization:

> (The ethnographer figures out what the devil natives are up to) by searching out and analysing the symbolic forms – words, images, institutions, behaviors

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> – in terms of which, in each place, people actually represented themselves to themselves and to one another. [Geertz 1983]

As to the second question, given the difficulty of the task, my methodology will be eclectic. My first tool for gaining a glimpse at the self-image of chemists takes advantage of the strong iconic dimension of chemical language. Modes of representation may somehow provide insight into their self-representations too. My second tool is to focus on landmark achievements, those which the chemical community regards as highly significant, as another step towards this elusive goal, the definition of a collective self-image. The third, obviously related tool is the consideration of eminent chemists. These women and men, selected for admiration by their peers, stand for something. A fourth tool is topographic: what is, at a given time, the territory of the chemist? What does it include? Even more important, what does it exclude? Blind spots, delayed recognition of achievements can thus be highly significant. The fifth and last tool, all too obvious, is a look at the vogues and fads: when a large number of chemists rush towards the same goals, this involves individual self-interest, which in turn reflects the image individuals hold of themselves, an image both furthered and reinforced by the group.

I shall resort to periodization, to subdivisions into decades.[1] It has the advantage of simplicity, and the further merit of being neither too coarse nor too fine-grained.

The 1950s saw the birth of the research university in the USA, together with that country seizing the lead in world chemistry, both academic and industrial. This was the time of birth and of explosive development of the nuclear magnetic resonance tool (Becker 1996). This was also the period when chemistry opened itself to molecular biology. Also in the 50s, the chemical science restructured itself, focusing on bonding and structure rather than on mere description and cataloguing of chemicals and their characteristic properties and reactions, as had prevailed since Lavoisier's time.

The rethinking of the science went on during the 1960s. This was a time for questioning previously held dogmas, such as the rules governing valence, rudely pushed aside with the discovery of noble gas compounds. But there were other upsets too, to such an extent that one may wish to call the 60s the 'Age of Hubris':[2] biological chimeras, spontaneous generation, high-temperature superconductivity, and the dissolution of potassium permanganate in benzene are some of the examples of such a mental attitude, calling for disrespect for tradition. The 60s were noteworthy also for a major breakthrough, the Woodward-Hoffmann rules governing electrocyclic reactions (Woodward & Hoffmann 1971). And a social phenomenon, actually the converse of a mass movement, should not be forgotten: when in 1968 the university came under attack, worldwide, from students, from the media and from the local communities, and even from its faculties, chemists as a whole camped within their ivory tower, oblivious of outside disruptions.[3]

The 1970s were the time of the first oil crisis. The petrochemical industry suffered from the consequences. As a profession, chemists worked on developing alternative energy sources. Photochemistry, with goals such as catalyzed photocleavage of water providing fuel for a hydrogen economy, came to the fore. There were many other projects, such as gasification of coal, or attempts at direct functionalization of natural gas, which were pushed forward at an impressive pace. The search for substitutes of oil went hand in hand with a significant rise in environmental consciousness among chemists – given especially some of the industrial spills during that period, which the media exploited relentlessly against the chemical profession as a whole, not only against the chemical industry.

During the 1980s, chemists as a group were willing participants in a *fuite en avant*, converts to a mystique of growth, even of exponential growth. This was a time for proliferation of newly made compounds, as if their preparation were synonymous with innovation. This was the time of the rise of combinatorial chemistry (Breslow 1997, Borman 1998), whose products in the tens and hundreds of thousands were screened for lead molecules by laboratories in pharmacology and in materials science. This was also the time of the preparation of organometallics in large numbers, vastly encouraged by the thrust from multinuclear NMR.[4]

The 1990s saw the spawning of nanoscience and nanotechnology, building on some major advances, such as the discovery of carbon nanotubes, following up on the serendipitous finding of buckminsterfullerenes (Baggott 1994). While chemists redirected themselves into these novel areas, they also had to come to grips with chemophobia on the part of the public.

2. Academic and industrial chemistry

Now that I have sketched in these few bold strokes what may have characterized those five successive decades, what exactly is the task at hand?

There is considerable evidence for the strong group identity of chemists. Chemistry has the distinguishing feature of being both a science and an industry. It is important to understand their solidarity. Even at the time of strong media presence by industrial accidents (Seveso in 1976), episodes of criminal negligence (Love Canal, pollution of the Rhine), or catastrophes (Bhopal), academic chemists, even though they felt victimized by the finger-pointing of public opinion, would not turn against their industrial colleagues.

Academic chemistry and industry share an umbilical cord. Universities train chemists for an industrial career. In return, the chemical industry funds some academic training (fellowships) and research (grants). Many professors of chemistry serve as occasional industrial consultants, receiving a handsome fee for their expert advice. Moreover, it is not infrequent for chemical careers to be hybrids, with scientists moving from an academic institution to an industrial environment, or vice versa.

Examples which come to mind are those of Philip E. Eaton, who owed his interest in cage molecules to industrial work on chlorinated pesticides, which led him to the epochal synthesis of cubane in 1964 (Traynham 1997); of Fred McLafferty and George Olah, who both worked for a time in a laboratory of Dow Chemical; of Richard E. Smalley, an industrial chemist before he enrolled as a graduate student at Princeton (Smalley 1996); of Earl Muetterties, who left the Experimental Station at DuPont in Wilmington for a professorship at the University of California; of Howard E. Simmons who, when heading central research also at DuPont, turned down the offer of a professorship at Harvard (Bohning 1993, Roberts & Collette 1999); and so on.

A strong group spirit extends across the chemical board, even at the cost of seemingly exonerating managers of industrial plants from grievous oversights or mistakes – in spite of their belonging to a social group distinct from that of laboratory chemists, whether academic or industrial. Put in another way: do chemists suffer from collective guilt? From none, is the answer.

However, granted that chemists share a strong group identity, what does it consist of, and how did it evolve during the second half of the twentieth century? I shall now proceed to build answers to those queries.

3. The 1950s: rise of the research university and chemistry

The 1950s saw the rise of the research university. The U.S. was its breeding ground: *Science, the Endless Frontier*, the report to the President by Vannevar Bush (1945) had urged such support by the federal government, to which chemist James B. Conant, then president of Harvard University and a close friend of V. Bush, was no stranger. The main feature was funding the advancement of science with public money, on a much-expanded basis (Weaver 1961).[5] Chemistry benefited from it, in like manner as the other hard sciences.

In disciplinary terms, physics was the leader in this novel way of conducting scientific research, which reflected the predominant part physicists had played during World War II (Kevles 1987).[6] Moreover, the close association of physicists with the military during the war translated into mission-oriented research, a concept which trickled down from physics to chemistry.

As a rule, physicists were funded more handsomely than chemists. The latter became envious and compared themselves often to physicists (Lind 1952, Anon 1958).

After Sputnik, one saw a massive increase in the popularity of science (Weinberg 1955) and in the numbers of young scientists. Research became paradigmatic and entered the undergraduate curriculum, even in liberal arts colleges (Kraus 1950, College of Wooster 1959). The way in which chemists did their work also changed. Electronic instrumentation came into the laboratory. Benches which in previous times, for centuries indeed, had been furnished almost exclusively with glassware, started receiving spectrometers (UV-visible, infrared) and chromatographs. Another influence from both physics and the military was the standardization of parts, making for easier replacement. To mention but two examples, Heathkit electronic components started being assembled, by chemists (Roberts 1990) or electronic technicians, in both academic and industrial departments; likewise Bantamware provided chemists with easily assembled glassware, for running reactions reliably on a much smaller scale than had been the norm.

Thus, physics had a deep-seated influence on chemistry. It triggered a mutation from a *craft*, with manual skills learned during an apprenticeship, to a manifold of standardized *procedures*, each of which made of steps, recorded in written instructions, which could then be followed similar to a pilot using a flying manual and a checklist. Chemistry, as a discipline, lost a measure of autonomy; it became more of a physical science, owing many of its tools ('physical methods') to physics too. Moreover, it became solidly fixed in the classification of sciences in-between physics and biology.

The birth of molecular biology saw to such an anchoring between physics and biology. The discovery of the DNA double helix (Watson & Crick 1953) was a turning point in the affirmation of chemistry and biology as sister sciences. Correspondingly, the notion of the interdependency of disciplines came to the fore, chemistry getting its tools from physics and biology, in turn, getting hers from chemistry. The influence from biology was not restricted to molecular biology. To give another instance, prebiotic chemistry came of age in the 1950s with the discovery of interstellar molecules (Ewen & Purcell 1951) and with the classic experiment in prebiotic chemistry (Miller 1953, Asimov 1960).

A chemist, Linus Pauling, embodied such a physics-chemistry-biology alloy. A crystallographer by training, he had taught himself the new quantum physics at its inception in the early 1930s and imported it into chemistry. Later on, during the 30s and 40s, he was one of the pioneers in molecular biology, elucidating key features of protein structure (Nye 2001). Pauling was much admired, an emblematic figure to chemists worldwide (Lipscomb 1993, Hager 1995, Kauffman *et al.* 2001).

However, in spite of and perhaps also because of the tendency to the blurring of disciplinary lines between chemistry and her sister sciences biology and physics, inner lines within chemistry endured and may even have become stronger. F. H. Westheimer tells a revealing anecdote in this respect:

> it is hard to understand the tightly compartmentalized minds of the chemists of that day. (An extreme example of compartmentalization: at the chemistry library at Cambridge University, an imaginary line divided the room into two parts, one for physical chemists and one for organic. The library had two sets of the *Journal of the Chemical Society*, since an organic chemist was not supposed to cross that imaginary line to use the volumes on the physical chemistry side of the library, and vice versa.) [Westheimer 2003]

A traditional aspect of chemistry also became stronger, its iconic language of formulas that the structural theory of the 1860s had established and which G. N. Lewis had bolstered with the notion of the electron pair in the 1910s. Pauling and his concept of mesomery (valence-bond theory) were responsible for this 'formulaic turn', which became a *lingua franca* among chemists. While physicists may have been virtuosi in estimating orders of magnitude for their data or in setting up differential equations, chemists could find shelter in their different language and, instead of back-of-the-envelope calculations, jot a few Lewis structures and make qualitative or semi-quantitative predictions, based on mesomery (Nye 2001).

I have emphasized so far the bonds between chemistry and physics and between chemistry and biology. An overarching notion for chemistry was to be of service to society. During World War II, the penicillin project, second in importance only to the Manhattan Project, had involved synthetic organic chemists and microbiologists (Harris 1999, Raber 2001, Shama & Reinarz 2002). The search for new antibiotics was a major thrust of pharmaceutical R&D during the 1950s. This turned out to be an epochal period in the alliance of pharmacology and synthetic organic chemistry, with the syntheses of morphine (Gates & Tschudi 1952), cholesterol and cortisone (Woodward *et al* 1951a/b), cantharidin (Stork *et al* 1953, Bohning & Fine 1991), strychnine (Woodward *et al*. 1954), penicillin (Sheehan & Henery-Logan 1957), colchicine (Schreiber *et al* 1961), chlorophyll (Woodward *et al*. 1960); with the elucidation of the biosynthesis of steroids (Eschenmoser *et al*. 1955); and with applications of profound social impact, such as the discovery of neuroleptics by Henri Laborit, Jean Delay, and Pierre Deniker in 1950 (Healy 2002), that of the first benzodiazepine tranquilizers librium and valium by Leo Sternbach and Lowell Randall in 1957 (Bello 1957, Baenninger *et al.* 2004) and the devising of oral contraceptives by Carl Djerassi and Gregory Pincus in 1955 (Asbell 1995, Djerassi 1970, 2001). Chemists could see and indeed saw themselves as agents of social change, even more so as they provided society with entire crops of new materials, often made of polymers (textile fibers, plastics).[7] This was also the time for the rise of great industrial laboratories, such as at DuPont (Roussel 1959a/b, Hounshell & Smith 1988), IBM, Bell Telephone, or Xerox from the success of the newly devised (1950) xerography, which all thrived from applying discoveries in pure science (Stokes 1997, National Research Council 1998).[8]

To sum-up this section: chemists may have cultivated an individual self-image of entrepreneur and a collective self-image of social benefactors. This ploy preserved the age-long, traditional image of craftsmen while adapting it to the flow of easy money. Note, in passing, that entrepreneurship was not a new notion for chemists. There had been quite a few examples in the past, such as Perkin's, Baekeland's, or Mond's. Furthermore, Justus von Liebig's laboratory at Giessen had pioneered a two-tiered organization of chemical laboratories, egalitarian among group leaders and feudal within each research group.

The 1950s, if dominated by university expansion and the attendant demographic increase among chemists, also portended things to come. I shall only mention here a few: the interdiction of fishing in Minimata Bay, which the poisoning by methyl mercury had brought to a stop; the first rumblings of opposition to the widespread use of pesticides (Anon. 1959); the devising of a ground-breaking solid-state peptide synthesis (Merrifield 1963); and the discovery of the NMR chemical shift (Dickinson 1950, Proctor & Yu 1950), which went a long way towards explaining the major influence of NMR on chemistry that we shall consider now.

4. The 1960s: the NMR revolution

The impact of NMR (nuclear magnetic resonance) was three-fold, intellectual, social, and psychological. As NMR superseded both earlier spectroscopic tools (such as IR and UV-visible) and contemporary ones (MS), it enforced among chemists a new kind of visual literacy. Chemists learned the skill of reading a spectrum, translating its features into words belonging to the language of chemistry, words such as 'carbonyl', 'phenyl', or 'methyl'. Earlier on, structural analysis had been a field in itself. Henceforth, the NMR tool gave every chemist such expertise (Becker 1993).

The impact of NMR was revolutionary (Zandvoort 1985, Anet 1996, Diehl 1996, Feeney 1996) also in bringing a host of new questions to the fore. These concerned, to mention only a few, nonequivalent groups of atoms and distinctions among stereoisomers, fluxionality, nonbonded atomic contacts. NMR gave a boost to the study of stereochemical relationships in molecules, which the contemporary conformational analysis had brought to the fore in the 1950s and 1960s (Barton 1972, Eliel 1990).

The A-60 was the instrument of change. Built in Palo Alto, California, by Varian Associates, it was a hands-on, routine instrument that graduate students in chemistry could operate. Precalibrated charts, together with an internal lock on a water sample tube, allowed for a single sweep to record a spectrum (Anon. 1961). The A-60 brought NMR to the masses. Chemists took to it like fish to water (Jackman 1996).

The NMR tube replaced the test tube. This assertion echoes that by Victor Hugo when he claimed so perceptively, in *Notre Dame de Paris*, in the superb 'ceci tuera cela' digression, that when Gutenberg invented movable types, the book killed the cathedral and its imagery, which had been so important earlier in educating Christians.

For centuries, chemists were associated with test tubes, not only in the popular imagination. The test tube was used primarily for qualitative analysis. Adding a few drops of a reagent would trigger a response, maybe a color change, a turbidity, or a precipitation, from which the chemist would infer the likely presence of a component in the liquid mixture, be it a ketone or sulfate anions.

With the NMR tube, chemists were offered a lighter and subtler perturbation. The reagent now consisted only of radiofrequencies (RF). Their resonant absorption by the sample in the NMR tube yielded a comprehensive inventory of the groups of atoms present. The NMR tube did not alone replace the test tube, because it came at the heels of other spectroscopic cells, such as for electronic absorption (UV-visible) or in vibrational (IR and Raman) spectroscopy.

Moreover, NMR was revolutionary in provoking a *Gestalt* switch (to borrow Kuhn's phraseology) in the chemists' perceptions. Earlier, molecules were identified by their functional groups, carbonyls, hydroxyls, and the like, which were like beacons (as reflected in chemical nomenclature, in its affixes and priorities). Now, especially at the beginning of the NMR era (50s and 60s) when ^{1}H NMR thrived by itself, not yet complemented by ^{13}C NMR, the hydrocarbon skeleton was primarily revealed by the spectrum.

Matter, when in a magnetic field and tickled by RF waves, gained in meaningfulness. NMR was a micro-scope of a different order. It displayed interrelationships between atoms in a molecule (Barton 1972). Couplings among nuclei, whether scalar or dipolar couplings, were the silk threads in the spider web that the NMR spectrum revealed. NMR was truly the Ariadne's thread guiding chemists to their Holy Grail of structure elucidation.

The web of interactions thus revealed in turn mapped molecular structure. Spectral analysis (the NMR tube) had replaced chemical analysis (the test tube). Analyzing a spectrum, furthermore, was an exact science, not an interpretation fraught with uncertainty, open to skepticism and dispute.

How did chemists greet the new tool made suddenly available to them towards the mid-1950s? Their obvious alacrity was tinged with ambivalence.

Chemists of the old school did not always embrace the new situation that they were dependent on young upstarts, called 'NMR specialists'. They retrained themselves. They had to. In addition, they had to contend with the shadow cast by physics, which obscured the cherished notion of the autonomy of chemistry, of its non-reducibility to physics. The mixed feeling expressed itself in a catch phrase of those times, 'physical methods'.

With the advent of NMR, new values – in the strongest sense of moral, ethical, and axiological values – came to dominate chemistry, while more traditional values were made redundant and obsolete (Campbell 1960). Take the example of elemental analyses. Before NMR came on the scene, they were the equivalent of a moral obligation. They linked laboratory notebooks to the final publication of the results. Now, elemental analyses became dispensable by the information from the new spectroscopies, high-resolution mass spectrometry (Laidler 2004) even more so than NMR. In spite of this new aspect of laboratory life, journals insisted for a long time (measured not in years but in decades) on the continued insertion of elemental analytical data in the experimental part of manuscripts submitted for publication.

Such rearguard action denotes lingering distrust of the powerful new tools. It points to a kind of umbilical cord linking chemists with their distant past. One can date the rise of the elemental analysis, as the first step in establishing the structure of an unknown, to Justus von Liebig's devising of the *Kaliapparat* in April 1830 (Usselman & Rocke 2005). For one-and-a-half century, the elemental analysis had remained central to the activity of the chemist. Quite suddenly, it was taken off its pedestal, to be discarded on the junk heap of history.

NMR caused some unease in the profession for other reasons too. Early on, a chemist, whether inorganic, organic, or physical, had to become self-taught in the rudiments and intricacies of NMR. Given the pace of change in NMR methodologies, that made for some insecurity. Add to this the uncertainty about the true nature of the profession. Was being a chemist simply a matter of isolating a compound, putting it in an NMR tube, and publishing the results? Somehow, this seemed like cheating. Something was lacking. The NMR revolution brought a challenge to one's self-image as a chemist. The profession was visibly changing. It was taking a turn, but was it for the better?

There were many aspects of the interactions between chemistry and NMR. I have commented so far on the impact of this new instrumentation upon the laboratory in its daily activity. One should add that there were chemists who also contributed to the development of NMR methodologies. Names which come to mind most forcefully are those of Frank A. L. Anet, Axel A. Bothner-By (Bothner-By 1996), H.S. Gutowsky (Gutowsky 1996), William D. Phillips, and John D. Roberts (Roberts 1996).[9]

In its spectral patterns NMR embodied a spectacular proof of the underlying quantum physics, and of the uncanny accuracy of its predictions of line frequencies and intensities. Thus, it brought with it, if not an understanding, at least an acceptance of quantum physics. Rank-and-file chemists thus became somewhat informed about the key developments of physics in the 1930s. They became inadvertent converts to quantum ideas.

Any revolution, political or scientific, affects the language. It comes with its own lexicon. NMR was no exception. It gave chemists a *lingua franca*, an idiom that not only extended the vocabulary, but also deepened their understanding of molecules, of their electronic distribution for instance (Anon. 1962). Almost as an aftereffect, such an improved understanding of the structure of molecules vastly improved communication among the tribe. Chemists became more conversant with one another, from being given a language with improved performance.

Equally interesting is the profound effect that NMR had on chemical thinking. An outstanding case is that of Jean-Marie Lehn. In the 1960s, Lehn oversaw NMR in his mentor's, Guy Ourisson's, laboratory at the University of Strasbourg. Lehn thus started his career as an NMR specialist. It gave him a lifelong attention to even minute details in NMR spectra. Their deep chemical meaning for sure would have escaped another chemist's eye. Many of Lehn's experiments were designed either starting from such observations or with NMR in mind as the main tool of study.[10]

Was NMR a mere tool for chemists? It fulfilled several other roles in the tribal culture. It was a prestige item. Status accrued to scientists who, somehow, had access to the latest, most expensive, and state-of-the-art instrumentation. When a visitor came to a laboratory, the tour did not fail to include a proud display of one's NMR spectrometer(s) – usually in the basement of the building, given the weight of electromagnets, until superconducting magnets came into general use during the 1970s and 1980s.

Showing off one's NMR equipment (Huisgen 1994)[11] was also done in scientific journals. Inclusion of an NMR spectrum or several gave allure to one's publications. A cursory look at chemistry journals shows the start of such practice in the early 1960s, with a steep rise to a maximum in the 1970s – 1D spectra making room for their 2D siblings (Ernst 1992) – followed by a slow, gradual wane. The cost of the printed page saw to it. The picture shown was worth, literally, a thousand words. Of course, as the Golden Sixties gave way to the Stagnant Seventies (Smith & Karlesky 1977, Graham & Diamond 1997), expensive laboratory equipment became more difficult to procure; but by that time it had become a vital necessity.

Besides a 'keeping-up with the Joneses' element, publication of NMR spectra conformed to another, more of a long-term trend (in the sense of braudélian *longue durée*): displaying one's evidence, as if scientific journals had turned into courtrooms. NMR spectra, in that respect, were showcase data. They brought forward prime evidence.

In addition to those two roles, showing-off and confounding critics, published spectra were also iconic of the excellence of the laboratory work behind the paper. They underlined how neat and careful it had been. One would see at a glance not only the quality of the spectrum with sharp, well-resolved, tell-tale lines, one could also not help noticing the absence of impurities, or that statements made in the text about the coexistence of different reaction products were borne out by the data, and so forth.

Last but not least, NMR weighed hard on the budget of chemical laboratories. It gave chemistry, starting in the 1960s, a new need for money, for big money. The small science, that of a lone craftsman, was being displaced by another form of small science, no longer that of the single investigator but that of the small research group, a considerably more expensive endeavor.[12] The NMR spectrometer had to be replaced every three to five years with a new instrument (Roberts 1990). In order to stay competitive, one had to somehow secure grant money for such hefty investments.

5. The 1960s: chromatographic takeover and other laboratory changes

The chemistry laboratory changed more between 1950 and 2000 than from 1600 until 1950 (Prelog 1991, Streitwieser 1996). So much is undeniable. Even though chromatography dates back to the turn of the twentieth century (Twsett 1906) – equally significantly it was rediscovered in the 1930s by Richard Kuhn and Edgar Lederer – it underwent a blossoming during the second half of the twentieth century. A.J.P. Martin and R.L.M. Synge devised paper chromatography (1943), due to penury during World War II. Together with other chromatographic methods, such as thin-layer, gas phase, and high-pressure liquid phase chromatography, paper chromatography rose to the ascendancy in laboratories during the 1950s and 1960s. These chromatographic methods superseded earlier separation techniques, such as fractional distillation, crystallization, sublimation, *etc.*, because they were considerably more powerful, could be automated, and required only miniscule amounts of material for analytical purposes; in addition, they could be redirected to preparative purposes.

Many of these chromatographic techniques became implemented in simple-to-operate instruments. In the 50s, they benefited, just like NMR, from the huge progress in electronics brought about by World War II; and later on, from the invention of the transistor. These methods achieved a revolution in the separation and isolation of chemicals from mixtures. The time required for this truly preliminary stage in chemical research was rather suddenly cut-down from weeks to hours. But, again just like NMR, such progress was costly. It weighed on the budget of chemical laboratories.

The chemical laboratory was radically transformed by the mid-1960s.[13] In addition to NMR and chromatography, other molecular spectroscopies, which were then implemented in commercial instruments, first and foremost mass spectrometry, also invaded the chemical laboratory (Morris 2002). The combination of chromatographic isolation and structural determination using predominantly mass spectrometry, NMR, and, to a smaller extent, infrared spectrometry, allowed chemists to go, typically in a week and sometimes in a single day, from an unknown chemical in a mixture to a structure on paper. During the same period of the 1960s, routine use of X-ray diffractometry[14] for molecular structure determination – the timescale was also the week –, at the hands of a few pioneers such as R.B. Woodward started to rival the joint use of mass spectrometry and NMR for the same purpose.

The 1960s were noteworthy too for the arrival of computation at the service of the chemical laboratory, whether industrial or academic. At that time, mainframe computers, housed in a computer center, were still the norm. It was only later, in the 1970s and 1980s that personal computers started to displace them. The introduction of the Macintosh by the Apple company was the analog for computation of what the A-60 spectrometer had been for NMR: it enabled hands-on operation by the individual graduate student. Developments of software were even more important than the steady, Moore-Law abiding, progress in hardware. Roald Hoffmann introduced his Extended Hückel theory in 1963, implemented in a molecular orbital program parametrized for many elements in the Periodic Table. It allowed chemists to calculate properties such as energies and geometries for many chemical structures. Two years later, John Pople came out with another semi-empirical tool for molecular orbital calculations (Mulliken 1972, Ransil 1989), known by the acronym of CNDO. At the same time, the Quantum Chemistry Program Exchange (QCPE) was set-up in 1963 at the University of Indiana as a clearinghouse for software; many of the programs could be acquired by chemists for a nominal fee. QCPE did much for the dissemination of molecular orbital calculations as a new tool. Another highly significant development, based on the computer too, was retrosynthetic analysis, by which synthetic pathways to a target molecule could be identified and compared (Corey 1992).

It would be an oversimplification to subsume under the heading of ‘Big Science’ (Weinberg 1967) the equipment of the chemical laboratory during the 1950s and 1960s just because it included relatively expensive instruments such as chromatographs and spectrometers. One could make a case for the resilience of the notion of ‘chemistry as craft’; supporting evidence would be, for instance, the recourse to thin-layer, paper, or column chromatography in complement with gas or high-pressure liquid chromatographies. The former, low-tech tools required a measure of dexterity and care in their use and turned out to be as important to the daily laboratory life as the latter, high-tech methods. Accordingly, before the Golden Sixties[15] came to an end, chemists to some extent could see themselves with a foot in each of the two worlds, that of traditional chemistry, basically unchanged for two or three centuries, and that of modern chemistry, with a plethora of new and powerful physical methods at their fingertips.[16]

The tension between tradition and modernity expressed itself not only in laboratory practice, but also in the conceptualization of chemical entities, such as key intermediates in chemical reactions. The 1960s saw the flare-up of the 'classical-nonclassical ion' controversy within physical organic chemistry (Bartlett 1965, Weininger 2000). Its acme came in 1967 when H.C. Brown published an article in *Chemical and Engineering News*, presenting his orthodox views and criticizing the unorthodox, non-classical proposal, first formulated by S. Winstein, using the language of molecular-orbital theory.

The controversy failed to be resolved decisively by the end of the 1960s. However, it led to the demise of physical organic chemistry,[17] previously at the top of the totem pole in prestige (and in funding). Synthetic organic chemists took advantage of the very public and extremely acrimonious discussions to grab the lead in the pecking order among sub-disciplines of organic chemistry, and, arguably, of chemistry proper.[18] Further factors were involved in synthetic organic chemistry's rise to the top: the exceptional personality and achievements of R.B. Woodward; his influence in getting his students appointed to coveted faculty positions; the economic pull of the pharmaceutical industry, its need for well-trained preparative chemists to staff its laboratories; the enduring notion of chemistry as a craft, which, in a defensible description, was a silent protest against physics turning into factory mode (Latour 1987, Galison 1997). Furthermore, with the narrowing of the job market, synthetic organic chemistry reasserted itself forcefully, at the expense of both physical and physical organic chemistries.

Accordingly, to go back to the self-image of chemists, one might say that the 'classical-nonclassical ion' controversy helped to firm up a dividing line between, roughly speaking, two groups A and B. Group A was that of the traditionalists and conservatives, led by synthetic organic chemists who draw their power from the labor market, trained people for employment by the pharmaceutical industry, and gave primacy to observation and experiment opposed to theory and calculations (Boyd 1958). Group B was that of the modernists, led by physical and quantum chemists to whom the Schrödinger equation was the Rosetta Stone, who gave primacy to deducing conclusions from first principles and to numerical calculations, and who saw themselves as pioneers.

In the 1950s and 1960s, the chemical community was bound together by a set of assumptions held so deeply that chemists themselves were largely unconscious of them. The first of these shared assumptions was linguistic: everyone spoke English. Turning to organic and biological chemistry, as distinct from physical chemistry, another assumption was a collective distaste for mathematics and any part of chemical theory that called for a mathematical input. Yet another assumption from the same rather reactionary quarters, in industry especially, was the superiority of trial-and-error, Edisonian research to any other approach. This cluster of assumptions affected the behavior in many ways.

In spite of the split between organic and physical chemists, a group identity asserted itself that featured the same ambivalent aspects of the culture as a whole. The combined strengths of organic and physical chemists would enable them to explore, appropriate, rule over, and (in some cases) settle vast territories beyond the horizon.

The victory of the As over the Bs, at the end of the 1960s, was consolidated by the polywater debacle, a story to be alluded to later, in conjunction with some other episodes of lemmings-like collective behavior.

6. The 1970s: acquiring environmental consciousness

I have referred to the Minimata mercury intoxication already. Other accidents, due to chemical spills from industrial production, struck closer to the homes of European and American chemists (Taube 1983). A book published right at the beginning of the decade heralded the new environmental concerns (Benarde 1970). Chronic pollution of the Rhine led countries responsible for the effluents (France and Germany) and countries suffering from the toxic wastes (The Netherlands) to sign a treaty in 1976. In principle, it guaranteed a return to a healthier state of the water. But that did not prevent industrial plants close to the Rhine from continuing, or resuming to use, the river as a sewer. A disaster occurred during the following decade, in 1986: a major dump in the Basel area of 30 tons of fungicide and mercury wastes killed 500,000 fish. The U.S. saw a number of similar episodes. For instance, Union Carbide was sentenced in 1981 for having dumped nearly 100 cubic meters of carcinogenic chemicals in a river in Virginia, the Kanhawa.

In 1974, Sherwood Rowland and Mario Molina published their conclusion of an extremely careful study of atmospheric chemistry: chlorofluorocarbons, chemicals used in a number of applications (*e.g.*, as refrigeration fluids and propellants in spray cans), when bombarded by solar UV-rays in the upper atmosphere, released chlorine atoms, which in turn destroyed ozone molecules to the tune of 10,000 per chlorine atom.[19] These authors drew attention to the threat to life on the planet from the destruction of the protective ozone layer (Rowland 1995, 1997).

At first, and for more than a decade, the response from the chemical industry was belligerent and obscurantist. They tried to impugn the credibility of those scientists. There seems to be a deplorable pattern in the attempts from the chemical industry to smother criticism with personal attacks and the setting of artificial controversies, with its salaried scientists toeing the party line. Similarly disgraceful treatments were meted out to F. Sherwood Rowland as had been to Rachel Carson many years before.[20]

James Farman's report of the ozone hole over Antarctica in 1981 was the turning point. The major producers of CFCs (companies such as Dow and DuPont de Nemours), a $ 9 billion business with annual growth rates of 10% (Thomas 2000) then espoused the worldwide concern. That would finally result in the Montreal Protocol in 1987, banning CFCs, stopping their production, and promoting substitution by HCFCs.

In 1976, the Seveso accident appeared on the front pages of newspapers worldwide: 2 kg of 2,3,7,8-tetrachlorodibenzoparadioxin had been dispersed by a Givaudan plant in the neighborhood. This industrial spill, which did not cause any human casualties (cattle grazing on nearby meadows were killed in large numbers as a safety precaution) nor had any long-term effects on human health, nevertheless raised the level of consciousness about dioxins and their release by papermaking mills and municipal waste incinerators, among other sources. As a consequence, careful controls were instituted in industrial countries, leading within a couple of decades to at least halving the amount of dioxins discharged in the environment in France, for instance.

The chemical community was the butt of unrelenting criticism from the public and the media,[21] who regarded it the chief culprit of environmental pollution, in the form of acid rain (Cowling 1982, Mohnen 1988, Baedecker & Reddy 1993) and harmful chemicals in surface water (Baker *et al.* 1991) such as the Great Lakes (Hilleman 1988), in tainting supplies of drinking water (Ross & Amter 2002), and in irresponsibly and illegally dumping toxic wastes (Love Canal near Niagara Falls, NY;[22] the Hudson River was continuously polluted by PCBs).[23]

The chemical community responded, as a rule, rather promptly – over less than two decades – and responsibly. The chemical industry cleaned up its operation, as a rule allowing only innocuous effluents from its plants. Environmental research was funded handsomely, and drew significant numbers of chemists. Chemical education also followed suit, for instance the American Chemical Society sponsored textbooks of general chemistry for non-chemistry undergraduate majors, focused on environmental issues.[24]

A yet more interesting trend developed. I contend that the new environmental consciousness among chemists, with lasting value (Mossman *et al* 1990) led them to move in the direction of biology (Bensaude-Vincent 2002), rather than continue with traditional mainstream chemical science.[25] This 'Biological Turn' was marked by the vitamin B12 synthesis (Woodward 1973), the much admired biomimetic steroid syntheses (Johnson 1977, 1998), the synthesis of natural products (Woodward 1972, Nakanishi 1991) such as giberellic acid (Corey 1978) or multifidene (Boland & Jaenicke 1979), the considerable research effort by E.J. Corey in the field of prostaglandins, the pioneering studies by Martin Karplus on protein molecular dynamics (1977), and so on (Todd 1983, Williams 1990). Quite a few small start-up companies banked on the public taste for natural products, for instance, in the form of herbal over-the-counter drugs (Anon. 1999).

The 1970s had opened with the first oil crisis. This geopolitical event made political leaders seek alternative energy sources. The chemical profession followed the money. Since grants were awarded for work on alternative energy sources and environmental issues, chemists started work in such areas. They became environmentally conscious (Baird 1995) and started cleaning-up their operation, whether in industrial plants or in academic laboratories. Everybody became safety-conscious, and pollution of the environment became drastically curtailed (Ember 1991, Rodrick 1992, McGinn 2000).

7. The 1980s: mystique of growth

I will now, in some detail, document my assertion of the intoxication by growth for its own sake, basing myself on the Pimentel Report (PR), first published in 1985 by the American Chemical Society (Pimentel & Coonrod 1987).

An illustration in PR shows, as a small icon of spectroscopic progress, the improvement in sensitivity of nuclear magnetic resonance spectrometry. In only 15 years, the solid-state ^{13}C spectrum of adamantane, with only two types of carbons, CH and CH_2, had turned from inaccessibility to an easy routine. Such solid-state ^{13}C spectra became important diagnostic tools for the chemical industry in the areas of polymers, whether as elastomers, textile fibers, or plastics such as polypropylene.

However, if NMR spectrometers improved considerably in performance, their cost increased likewise. A graph in the Pimentel Report depicts these twin trends: the increase of NMR spectrometers in magnetic field strength, thereby in resolution, in sensitivity, and in cost. Chemists in the 1980s were clamoring for increased public support from granting agencies, such as the National Science Foundation and the National Institutes of Health, and battling with one another for grants from those agencies. Instrumentation was costly across the board, a cost-quality diagram in PR made the same point for mass spectrometry. Half-a-million dollars was the price of a state-of-the-art mass spectrometer in the mid-80s. The implicit message was 'give us the funding necessary to study biopolymers such as proteins, we are willing, we only need the money'.

Both academic and industrial chemists became increasingly dependent on computers. The PR includes another exponential plot, showing that American industry had equipped itself with large mainframe computers during the post-World War II period. A complementary graph conveys the steadily increasing cost of computation.

In the 1980s, chemists set up materials science as an offshoot of their discipline. They strove for polymers to replace wood, glass, and metals in many of their uses, and to open-up brand new applications. Plastics underwent an exponential growth during the post-World War II period, which the PR depicted graphically. And chemists took pride in the petrochemical industry rivaling metallurgy in output. One of the illustrations in the PR aimed at informing the public that sales of chemicals were comparable in volume with those of motor vehicles or machinery. The curve goes through a maximum, reflecting the downturn caused by the second global oil crisis of 1979-1980 with the attendant worldwide recession.

The PR prided itself, not only on the production of man-made fibers and plastics, but also on the sheer number of new chemical compounds. It showed their growth, as registered by *Chemical Abstracts* during the second half of the twentieth century, in a picture of self-satisfaction and hubris. Chemists prided themselves on the proliferation of chemicals and the increased density of the chemosphere.[26]

This was the time also of the uncontrolled proliferation of organometallics. In countries with powerful chemical organizations, but lacking a share in the leadership then exercised by American chemists in concepts and methods, primarily in the United Kingdom, Italy, and France, the field of organometallics became the province of unimaginative scientists who would prepare compounds, based on analogies from the Periodic Table, only because such compounds were heretofore unknown. They assimilated newness and innovation.

Chemists in the 1980s enjoyed their self-image as, not only good citizens who had cleaned-up their operation during the previous decade, but as human benefactors (Woodburn 1976) through their crucial contribution to pharmaceutical research. The message of the PR is loud and clear: since chemists are helping humankind by devising new, efficient drugs against disease, if they are to continue doing so successfully, they need more money for equipment.

That brought up the battle for funding with the rival discipline, physics. Physicists in the 80s were better than chemists at grantsmanship and public relations and were henceforth more successful in getting mammoth collective projects financed. Chemists, conversely and as illustrated in the PR, prided themselves on demographics, on beating physicists in sheer number of jobs – or better yet in the production of Ph.D. scientists.[27]

Chemical science was moving at such a furious pace in the 80s that the scientists were unable to keep up with the information they were generating. Fullerenes are an outstanding example of such an information overload. Richard Smalley made a point in his Nobel Lecture by referring to pioneering work by a number of scientists in several countries of which he, Kroto, and Curl were unaware at the time of their discovery of C_{60} (Smalley 2003).

8. The 1990s: attempts at rebranding

The 1990s were the period of rather massive retirement of the post World War II baby boomers from their positions in academia and industry. How would this generation change impact chemistry? Would there be a takeover by Generation X? Would it bring in an entirely new set of human and professional values? The perhaps surprising answer is that continuity stifled mutation.

I base this assessment on a study I made of young faculty members in American chemistry departments, and of their stated research goals, using the standard source for such information (ACS 1995). I picked at random the letter G. These then were my findings.

Faculty members born after 1955 – my arbitrarily chosen cutoff date – numbered 44; only five were women. By far, the dominant research area was biochemistry and molecular biology – even though the names were selected from the chemistry section of the directory that excluded departments of biochemistry. Ten of the junior faculty members were engaged in such biowork. Half-a-dozen other areas, more or less equally populated, accounted for most of the remainder of this sample: organometallics (6), organic synthesis (5), surfaces (5), NMR (4), spectroscopy other than NMR (4), and catalysis (3). To my surprise, traditional topics going back to the 1960s continued to be popular with some of these budding academics. Stereochemistry and conformational analysis was one of them.

Indeed these young faculty members, the vast majority of which were still assistant professors, were playing a safe game. They obviously picked areas of work that were both funded more easily and promised to provide publishable results in short time. I failed to identify highly adventuresome profiles. To the contrary, quite a few among those individuals posted a number of different research orientations: ‘something will have to work out’, seemed to be the guiding motive. One of these young investigators for instance – he was quite representative in such an across-the-board sweep – announced he was working on “organic and polymer self-assembly; electronically conductive polymers; liquid crystals; self-assembled monolayers; host-guest interactions; organic electrochemistry."

The background for such a behavior – and the US were representative of an almost worldwide trend – was an erosion in public support of chemical science. To give a simple figure, between the 1960s and 1990s, there was a ca. 60% drop in funded research proposals submitted to the National Science Foundation and the National Institutes of Health. From 1983 to 2002, funding of chemistry and physics by the US Departments of Energy and Defense, had plummeted. Correspondingly, there were 9% fewer graduate students in chemistry in the US in 2000 than in 1993 (Eiseman *et al* 2002).

In response to the wane of public support for chemistry, some chemists became more vocal. They praised not only the field and their individual contributions with a more emphatic tone. Hyperbole as a rhetorical tool was resorted to more often. It marked scientific publications especially, research proposals even more. Advertising hype displaced the more modest, objective, and neutral style of reporting. Results became routinely 'divulged' or 'revealed'. Facts were praised by the authors as 'unprecedented', 'novel', and 'remarkable', to quote some of the routine adjectives.

Such advertising belonged to a relatively new trend. Science, in addition to being conducted according to the norms, had to be marketed adequately. Science results were sometimes publicized as if they were a commercial commodity. Hence, one witnessed attempts at renaming and rebranding chemistry, in part or as a whole.

Renaming as a whole? The Department of Chemistry at Harvard took the lead. It added to its name the complement 'Chemical Biology'. Other universities (Cornell) followed suit. They had a good excuse in improving their name and their image, so they thought: their new name was more accurate. As mentioned above, the younger faculty members were moving towards more biological topics.

Against the profit background (Hounshell 1998) and in the face of decreasing public support of science, the research university sought to reinvent itself (Kumar & Patel 1995). With academic and industrial sciences continuing to be structured in different ways (Ziman 1994), both the chemical industry and the pharmaceutical industry supported two of the vogues[28] which were foremost in the conceptual landscape of the 1990s, those of biotechnologies – both within small start-up companies and within large corporations such as Novartis in Basel[29]– and of nanoscience and technology (Welch Foundation 1996, National Science Board 2002, Schummer 2004, 2005).

Even within academia, chemists were shifting their focus (Ivory Tower, intellectual distinction, remoteness from public concerns) to the marketing of their productions. Those products continued to take the form of publications and patents. The novelty was the hype, often combined with the perceived need to disassociate oneself from the negative image carried by the discipline.

A solution was to rename the subdiscipline one belonged to. The new name would avoid the 'chemical' adjective. Surface science thus thrived during the 90s. Another renaming occurred with the nanoscience and nanotechnology bandwagon.[30]

That brings up the twin issues of branding and rebranding (Rivkin & Sutherland 2004). Were you living off your heritage as a traditional chemist, without paying attention to negative public opinion, without thinking about the future at all? Such a stance was risky at a time when, to mention just one country, the government was closing down chemistry departments in British universities.[31] It was much more advisable either to brand yourself as trading on your heritage, but with the future in mind – which accounted for combinatorial chemistry.[32] Better yet, you could establish your business as a brand-new endeavor, with the future predominant on your horizon (Schultz 2000).

Green Chemistry was such a ploy. It was a renaming and rebranding of industrial chemistry, terming research in environmentally-friendly processes: under mild conditions of pressure and temperature, dispensing with organic solvents, releasing innocuous wastes only, if at all.[33,34]

The Internet was influential in such attempts at rebranding parts of chemistry, to the benefit of individual scientists. Those were able to build an allegiance with their constituency through the Web, to set-up networks too, in a much shorter time than within the traditional pre-existing brands – analytical or physical chemistry, say – and became more relevant to perceived needs in doing so (Schultz 2000).

The Internet made networking easier (Bachrach 1996), so much is incontrovertible. However, would it facilitate regrouping the chemical community, its various segments each into its mainstream? Or conversely, would it make it easier to splinter into finer and finer subdisciplines, as a symptom of disciplinary maturity? Both of these trends, the centripetal and the centrifugal, can be discerned.[35]

The former, the rush into conformity,[36] explains the continued pull of various fads, the appearance of ephemeral vogues that would enlist support from chemists. This was neither new nor original to chemistry. Chemists had earlier on been swept into brief stampedes, such as the polywater 1972 episode (Franks 1981) or the cold fusion 1989 foray into pathological science (Langmuir & Hall 1989), Gold Rushes and inflationary bubbles both ending with a quick crash.

That the 1990s were not immune from such collective and highly cooperative phenomena was a sign of the maturing of the discipline. This permeated the collective consciousness of chemists. The perceived needs for renaming and rebranding came against such a background of a mature, well-established science.

Some topics for scientific research at times become very popular, to such an extent that a chemometric description as an epidemic may become relevant (Franks 1981). During the 90s, within synthetic organic chemistry, the devising of pathways to target molecules such as taxol or brevicomin was highly fashionable (Lowe 2004). Petrochemistry had its own vogues too. One of those was triggered in the 1990s by the devising of efficient catalysts for the olefin metathesis reaction: "a few years ago, it seemed that everyone with two alkenes in their lab was finding a way to get them in the same flask with some Grubbs catalyst" (Lowe 2004). The Nobel Prize in chemistry occasionally puts the ultimate seal of approval on such popularity polls. The metathesis reaction was thus distinguished in 2005.

9. Conclusions

Chemists have to face chemophobia. Such public hostility is rooted in a multiplicity of factors. Some are mythical, such as the Biblical account of the destruction of Sodom and Gomorrah from the befouling of air for breathing and of water for drinking, or the twin medieval anguishes over the cognate poisoning of wells or polluting of the air. Some are very real, such as chemical warfare or Bhopal.

Hence, a significant segment of the population sees the chemists as if in a ghetto. Such scapegoating of the chemical community – a scapegoating indeed, chemists bear the brunt of complaints against environmental pollution, of which other industries and the public itself (automobiles) are largely responsible – was met with stoical equanimity[37] during the 1950-2000 period. It made for a remarkably close-knit, worldwide community, united through its use of a common language, that of structural formulas (Cram 1990).

Outer pressure from criticism did not only make chemists close ranks, it also sharpened their self-image as providers of material benefits to society. For the whole period (1939-1980s) during which DuPont advertised its motto "Better Things for Better Living … Through Chemistry", chemists identified readily with this slogan and regretted its passing. They saw themselves as a social group, honest (Roussel 1959a) and well-meaning, doing essential work, and unjustly attacked, not recognized properly.

The worldwide chemical community has countered otherwise potentially disruptive tensions in embracing rapid change (in laboratory tools, in the attendant training of young chemists [National Research Council 2003, 2004] in topics of interest, *etc.*), while upholding traditional values of craftsmanship and small science. The truly remarkable adaptability, in the face of enormous changes (Havinga 1991),[38,39] arguably stems from chemistry being the science of material transformations, and from chemists thus being trained in both monitoring and controlling change.

Moreover, chemists see themselves as creative (Pacifico 1958, McGrayne 2001). To quote an eminent member of the profession (Gortler 1999):

> There's always going to be chemistry, because we are so special, because we create new things. There's almost no other science that creates anything. We create new things, and that creation involves an intellectual creativity in realizing what to do, what to make, and why to make it, and what to do with it when you've got it, that is very special to our field.

This notion of the creativity of chemists not only fits the 'individualism methodology' (Boudon 1979), it also ties in nicely with a metaphor favored by chemists in recent years, that of 'Chemistry, the Central Science' (Breslow 1997, Brown *et al* 2003).

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Notes

[1] The periodization I suggest is of course debatable. For instance, it has been proposed to decompose the activity in chemical science during the last 25 years along the categories of determination of protein structures; genetic engineering's debut; molecular mass production (combinatorial chemistry); microscopes, polymers, and lasers; light, sound, and atmosphere (sonochemistry & atmospheric chemistry); buckyballs and beyond (Gwynne 2001).

[2] The culmination of which came about later with the synthesis of palytoxin in 1989 by Y. Kishi's team at Harvard, a monument to the Promethean ability of chemists.

[3] Only negative evidence can be adduced in support of this assertion. For instance, the series of autobiographies that Jeffrey Seeman edited for the American Chemical Society does not include, to my recollection, evidence of such concern on the part of the academic leaders of the profession who were authors.

[4] The field started being populated substantially only after the discovery of ferrocene (Laszlo & Hoffmann 2000, Rich 2004).

[5] A development of which a few lone voices warned (Warner 1957).

[6] Whereas World War I had been termed 'The Chemists' War' (Kevles 1972), World War II was 'The Physicists' War', witness atom bombs, radar, sonar, *etc.* (Kevles 1987).

[7] Stereoregular polymerization was invented by Karl Ziegler and Giulio Natta in 1953.

[8] A lesson the pharmaceutical industry had implemented early on (Bohmfalk 1953b).

[9] Roberts was made a convert in part by a scientist at DuPont (Ferguson 1996).

[10] A representative example is one of his numerous studies in supramolecular catalysis (Hosseini *et al.* 1987).

[11] The evolution was lightning-swift, with superconducting magnets superseding electromagnets (McLauchlan 1996) and with the advent of Fourier transform NMR, which ushered in multidimensional NMR.

[12] The simple running costs for daily operation of a chemical laboratory also escalated. This is easily seen on the example of fluids. Prior to the 1950s, water and gas for Bunsen burners were the only ones routinely provided. After the 1960s, supplies that became standard included: compressed air; tanks of helium, argon, nitrogen; liquid nitrogen; special oils for thermostatic baths or vacuum pumps; *etc.*

[13] A development that swept through the whole of chemistry like wildfire. It was not confined to, say, organic chemistry. There is a contemporary testimony of the impact on inorganic chemistry (Lewis & Nyholm 1961).

[14] By tackling large biological molecules, pioneers from the British School initiated by J.D. Bernal paved the way for such ultimately routine use (Hodgkin 1972).

[15] A representative example is that of the University of North Carolina, in Chapel Hill (Bursey & Crockford 1982).

[16] Physical organic chemistry, as a sub-discipline, embodied such a duality. In the hands of leaders such as J.D. Roberts or J.A. Berson, it synthesized the molecules it needed for physical measurements. A contemporary survey of the mechanistic role of the physical organic philosophy is available (Hine 1960).

[17] The recollections of J.A. Berson, a leader of the field, make very interesting reading for his historical consciousness (Gortler 2001).

[18] Many people thought the non-classical ion controversy was the modern equivalent of the medieval argument about how many angels could stand on the head of a pin. In addition, physical organic chemistry was to some degree a victim of its own success. By the late 1960s mechanistic thinking was everywhere, and physical organic chemists who wanted to be on the cutting edge (and get funding) went into bioorganic chemistry, enzymatic biochemistry, and even molecular biology. So hordes of people were using and even contributing to physical organic chemistry but not calling themselves physical organic chemists (Weininger 2005).

[19] The devising of highly sensitive detection of CFCs by Lovelock was an important part of the story (Dronsfield & Morris 2002).

[20] To be fair, the position from industry was sometimes stated in a temperate manner (Anon. 1962).

[21] Such hostility to chemistry from the public can be dated back to the publication of Rachel Carson's masterpiece, *Silent Spring*, in 1964. This book drew the public

attention to the inconsiderate use of pesticides such as DDT by farmers, with dramatic effects on the ecology and on animal species accumulating the toxic chemical in their tissues (Baker & Wilkinson 1990).

[22] Hooker Chemical and Plastics Corporation dumped an estimated 350 million lbs of hazardous chemical wastes in this trench between 1942 and 1953. After it was filled and covered, it was sold to the Niagara Falls Board of Education. The scandal broke in 1977, after the New York Department of Environmental Conservation reported the extent of soil and groundwater contamination (Levine 1982).

[23] Unsurprisingly, public support of chemical research went down. In the US, federal funding decreased by 14 percent in constant 1972 dollars between 1971 and 1975 (Smith & Karlesky 1977, pp. 86-101).

[24] Chemistry departments lowered their standards during that period. The average student quality dropped. Faced with declining applications for admission, departments wanted to avoid losses in undergraduates (Smith & Karlesky 1977, pp. 114,156).

[25] Synthetic organic chemists were thus updating their concerns, turning away from their more traditional pursuit.

[26] A valuable chemometric study on the proliferation of new chemicals is available (Schummer 1997).

[27] The statement has to be balanced with the disaffection of undergraduate students with the sciences, for chemistry in particular, starting in the 70s (Neckers 1979).

[28] Industrial research is not immune to fashions that, as a rule, hit at the academic-industrial interface. The pharmaceutical industry, in its search for blockbuster drugs, has tried a whole litany of means, among which one may recall Hantzsch partition coefficients, linear free energy relationships, MO calculations of electronic distribution in molecules, docking into receptor sites using computer modeling, *etc*. The chemical industry proper has had its own vogues, which influenced also some academic research. For instance, shape-selective catalysis, as performed by zeolites, led to a major undertaking in the area of pillared clays, whose results little matched the expectations.

[29] Other pharmaceutical companies, in Germany in particular (Boehringer, BASF, Hoechst, *etc*.) chose another option, subcontracting to specialized companies, in the U.S. predominantly. By and large, this choice was an economic failure.

[30] The move was protracted, as so often in history. The incentive came from the discovery of the DNA double-helix by Watson and Crick, and from the ensuing lecture by Richard P. Feynman in 1959, calling for the devising of small mechanical engines and devices operating at the nanometer scale.

[31] 18 British universities closed their departments of chemistry between 1992 and 2001, an overall 27% reduction. The trend did not stop with the decade though. By 2004, 28 departments of chemistry had been closed since 1996. Some pessimists predicted that only six departments of chemistry might survive in the UK.

[32] A name with an obvious redundancy, most of chemistry being combinatorial by nature. This vogue was long overdue, Merrifield had pioneered his peptide synthesis on small beads of polymers in the 1960s (Merrifield 1963), meeting collective indifference from the profession. When such a technique was rediscovered towards the end of the century, it came back with a big splash with, behind it, big financial support from both materials science and the pharmaceutical industry. The latter was intent, as it always is, upon finding shortcuts to promising lead molecules, in terms of drugs both patentable and profitable.

[33] James Clark at the University of York was one of its leading advocates. Professor Clark is Head of the York Green Chemistry Group, Scientific Editor of the Green Chemistry Journal, Director of the Green Chemistry Network, Joint Co-ordinator of the Green Chemistry Research Network, and Series Editor of the RSC Clean Technology Monographs.

[34] *The New Yorker* published on July 5, 2004, p. 84, a cartoon by artist Mick Stevens. It depicts the headquarters of a chemical company. While the chimneystacks are spewing-out dark, presumably noxious smoke, the corporation advertises its efforts at controlling its own image. The cartoon illustrates how comically self-defeating such an attempt appears to the lay public. As illustrated in this cartoon, the efforts by the chemical industry to redeem itself in the public eye were doomed. There was a backlash in public opinion against the new image promoted by the chemical industry. The very fact that, at the same time, high-profit pharmaceutical branches divorced from their low-profit, cyclical chemical siblings, made even more unbelievable the claims by the chemical industry that it was safe and clean – even though such claims were the truth. To eradicate a prejudice ain't easy!

[35] It was the Age of Globalization too, which greatly affected, in particular, recruitment of graduate students and postdocs in American universities (Tobias *et al.* 1995, COSEPUP 2000, Walker *et al.* 2004).

[36] Michael J. S. Dewar has been not only lucid, delightfully sarcastic too, about this unfortunate tendency (Dewar 1992).

[37] Which outside observers often mistakenly equate with arrogance.

[38] Consider for instance the manufacture of polymers. Each of the various grades of polyethylene was made using different processes and catalysts during each of the decades under study. Recollections of one of the pioneers make for useful reading (Ziegler 1972).

[39] The visual idiom changed considerably. In the 50s, it was dominated by the octant rule, valence-bond structures, cage molecules, IR and UV-visible spectroscopies. In the 60s, proton NMR, mass spectrometry, linear free energy relationships, and the valence shell electron pair repulsion (Gillespie-Nyholm) came to the fore. The 70s saw the rise of carbon-13 NMR, molecular orbital diagrams, and coordination geometries. In the 80s, zeolite structures, 2D NMR, computer simulations of protein structures with inclusion of molecular dynamics became iconic. The 90s saw new kinds of images with AFM and STM becoming popularized, with entities pictured such as nanotubes.

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