My Research in Marketing : How It Happened

Andrew Ehrenberg

Fifty years of research into marketing topics ranging from
consumer behaviour (e.g. brand loyalty)
to how advertising works

This invited paper outlines how I think my research over the last 50+ years came about. With hindsight, I was probably always aiming at findings that were both simple and generalizable.

Simple findings so that I and others could see the patterns in data which at first often looked complex. Generalizable, within stateable conditions, to provide validated benchmarks, possibly lawlike in due course.

But this neat aim became explicit only slowly. At first I just unthinkingly did what I did. It seemed natural – like the bits of science I had picked up at school. I didn't set out to be different.

At University I managed a first in mathematics at Newcastle. I then read mathematical statistics at Cambridge with a nugatory outcome, which Sir David Cox called 50 years later a spectacular piece of maladministration by the University and which at the time cost me £50 a year in salary. Cambridge was followed by three years' statistical lecturing and consulting with or against Hans Eysenk at the Institute of Psychiatry (at the well-known Maudsley Hospital) in London.

An interest in social-science applications had already begun to emerge early in Cambridge. But also two aversions almost from the start: (i) to complex (and hence questionable) statistical analysis techniques being imposed on simple data, and (ii) to statisticians unnecessarily parading their second-hand mathematics in public.

A Brand's Heavy Buyers

My first finding in marketing arose in the late 1950s from a practical problem with the Attwood Consumer Panel, a precursor of TNS (Taylor Nelson Sofres), now about the world's biggest market research company and then my first marketing employer.
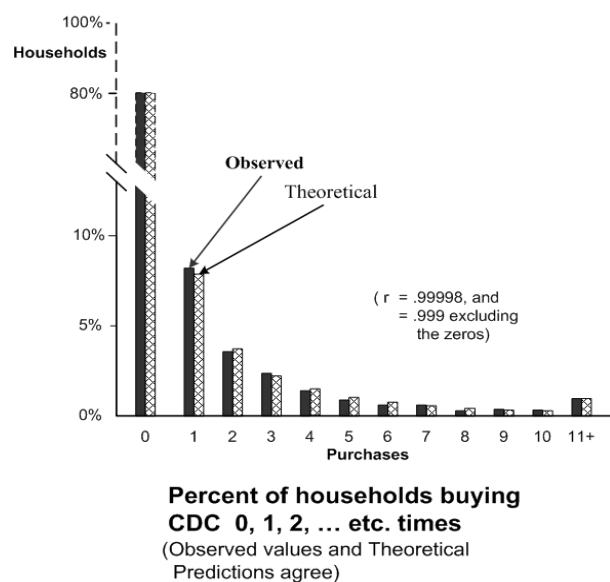
This panel measured consumers' purchases and was reporting too much buying of one brand, Cadbury's Drinking Chocolate (CDC). My boss Douglas Brown – the first and last I ever had who knew what I was supposed to be doing – wondered whether the excess was due to the panel having too many heavy buyers of CDC. (Heavy or loyal buyers normally are crucial for a brand's sales success.) Could I fit a theoretical distribution to the numbers of people who had recorded buying CDC 0, 1, 2, 3, or more times in a year say, so as to let us legitimately excise any excess heavies?

Most people buy a brand like CDC only quite occasionally. I therefore tried the traditional statistical way of modelling rare events (as a so-called Poisson process). But that did not work too well.

So I next tried a heterogeneous mixture of such Poissons (called a Negative Binomial Distribution or

NBD) which would allow different consumers' average purchase rates to differ (as of course they do) and which I had come across in Cambridge in modelling certain biological and social happenings (e.g. the occurrence of rare accidents).

This model gave a very close fit to the observed purchasing data, as in the graph. There was no excess of heavier CDC buyers. As a result, our analysis was of no help to management in its problem with the panel's alleged biased reporting of CDC's sales (which was then dealt with in other ways).

**Percent of households buying
CDC 0, 1, 2, … etc. times**
(Observed values and Theoretical
Predictions agree)

However, the big research issue for me suddenly was: Was the fit of this new NBD model more than a once-only fluke?

In the event, the NBD was found to fit again and again to other data: big, medium, and small brands of very varied grocery-type products from soap to soup (it took much work and time). And mascara. With correlations still well over .9. And one persistent minor discrepancy that was in the end fully explained in work by Chris Chatfield, my first doctoral student.

And slowly also for different countries, analysts, points in time, shorter and longer analysis-periods, younger and older consumers, etc.

Such a quantified regularity was unprecedented in marketing. So it was (and is) exciting. We had stumbled on an empirically widely-grounded theory to benchmark how many people do or should buy any brand how often. (That should have been the end of marketing's pipedreams of just recruiting heavy-buying buyers, but of course wasn't.)

The theory itself was also exciting. It assumed that consumers behaved "*as-if* probabilistically". That is very irregularly, but not literally at random, as many physicists famously assume for the elementary particles in quantum mechanics. But "Gott würfelt nicht" (does not play dice), any more than His housewives literally toss mental pennies for just when to buy their soap powder, and for which brands to choose.

Further regularities. The model also led to other theoretical predictions (e.g. for the period-to-period flow of "new", "lapsed", and "repeat" buyers of any brand, and for their associated buying rates). These predictions again held for very varied data and showed what to expect from healthy brands in a more or less steady market. (The model had avoided the theoreticians' mostly unnecessary complications of "loyalty erosion" and "purchase feedback".)

The NBD model's close fit also supported its underlying assumptions (20 years later Gerald Goodhardt proved its main "Gamma" assumption mathematically in *Nature*, one of our five contributions to that renowned science journal).

For some years we went on examining consumers' buying of individual brands. (I was fairly soon joined by Gerald Goodhardt and then Martin Collins in a very productive commercial

three-man set-up, Aske Research Ltd, where we paid our way, by way of facing bothersome facts rather than as a Lawrentian self-obsession. All three before very long also served as chairmen of the UK Market Research Society, and later became business school professors.)

Polygamous Brand-Buying

Suddenly an unknown US company researcher, Gary Grahn, noticed that the different brands in his product-category were bought at much the same average rate.

This simple finding was a revelation, at least once we established that it generalised.

We already had many such average buying-rates for different brands lying around from our past tabulations (done for years by hand – bars-and-gates – and then with Hollerith/IBM punched-card counter-sorters). But like everybody else, we had never bothered to compare these rates with each other because we expected no single or simple outcome. (Philip Kotler's uniquely-successful marketing text had already long stressed that marketing was complex and downright difficult.)
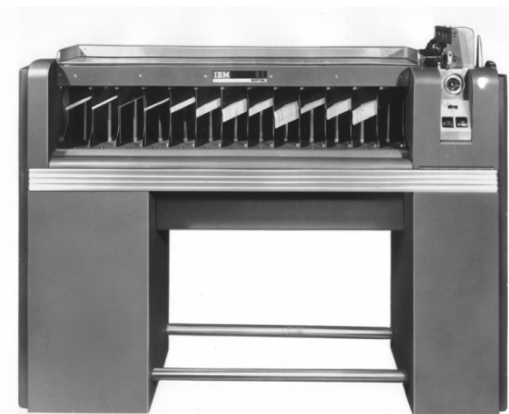
A Hollerith (IBM) Card-sorter

Grahn's result passed through three stages with us, over some years:

First, we thought and often said that the average purchase rates of competitive brands, big or small, were about equal. Or "constant" plus a bit of error – about 3 or 4 annually in that product category, about 6 or 7 in another. (The traditional approximately-equal sign of successful applied mathematics is to me its defining symbol, at its best with correlations of .9 or more.)

Later we noticed that the small "errors" were rather consistent over time and hence mattered. (Calculations were done on electro-mechanical desk calculators or on handheld mechanical Curtas if at home or travelling. Slide rules were not useful.)

A hand-held Curta

Later still, we noted a small but common systematic trend in these buying-rates: they decreased slightly with the brands' market-shares.

Abe Shuchman at Columbia then identified this small trend with William McPhee's recent "Double Jeopardy" (DJ) phenomenon in quantitative sociology: Compared with a bigger brand, a smaller brand is of course bought by fewer consumers, but it is also bought somewhat less often by them (because it has more exposure to the competition). Hence it was "punished twice", which McPhee, also at Columbia, had thought unfair on the smaller guys.

Brand-switching. Further empirical regularities in people's buying of competing brands also slowly

emerged (partly while visiting at Warwick University in the UK, and at Columbia during the 1968 student riots). This led to a picture of consumers of branded grocery products as polygamous, with several steady partners (i.e. brands), some consumed more often than others. Two examples are:

- Very few customers of a brand are 100%-loyal to it over a series of purchases. Nor do they buy it often. They therefore have few opportunities for being disloyal. Marketing's common target of "more loyal-buyers" was seen to be deeply unreachable.

- How many customers of brand A also buy brands B or C in the analysis–period varies directly with how many then buy B at all, or C at all. This was sanctified as the "Duplication of Purchase Law", with a simple adjustment discovered later for any partitioned market. (The then-intricate data tabulations were done by Gerald scrounging time at the UK Atomic Energy Laboratory on their *Atlas* computer – one of the then-world's three largest.)

All this led to my first real book, *Repeat-Buying: Facts, Theory and Applications,* first outlined while visiting at the University of Pittsburgh ("Pitt", in contrast with Carnegie-Mellon).

In 1970 I was headhunted by London Business School (LBS) to be professor of marketing and communication, having never read anything on either subject. I stayed 23 years and learnt, followed by 10 or more at London South Bank University.

Family Background

Over the previous forty years, I had had no great urge to be a professor. My extended family had "been there done that" – a dozen or more professors (including the Regius chairs of history at Cambridge and Oxford, Sir Geoffrey Elton and Sir Michael Howard, and a Nobel physicist); the influential Heidelberg psychiatrist Viktor von Weiszäcker (uncle of the late German President) and a lifelong friend like his wife Olympia; cousins such as Franz Rosenzweig (the leading early-20th-century philosopher of Judaism) and Ashley Raeburn (treasurer of Shell pre-Watts, and vice-chair of Rolls Royce for 15 years, who has lived "down the road" for the last 30 years); the highly anti-semitic Martin Luther longer ago; and also some more public performers like Olivia Newton-John (whom I repeatedly dandled on my knee, baby-sitting in Cambridge), and Ben Elton (who, when I asked after his father's third inaugural lecture whether he'd ever heard him lecture before, came back with the unrehearsed Yes, but not in public).

In 1939 my close family and I, aged 13, had sought UK asylum from Germany. My mother (who was what I had learnt to call Aryan) had married a dishy Heidelberg professor of philosophy just before WWI. He had already become a Christian, and after the army trained for the Ministry, selecting one of the bleakest of the black parishes in the Ruhr, Germany's coal-mining region. (My mother became good at telling Jewish jokes.)

By the early '30s my father had become a prime non-pin-up for the democratically-elected Nazi government party (a strongly-opposing Lutheran and ecumenical clergyman, an intellectual, an academic theologian, a scribbler, und der Jude (Jew) Ehrenberg). In April 1939, he was unexpectedly

released from his concentration-camp (Berlin-Sachsenhausen), through a bribe by the then-eminent Bishop of Chichester George Bell, as I learnt only quite recently from my father's two-volume biographer Professor Günter Brackelmann.

In England now in 1940, my father was interned by our Brits for some months as an Enemy Alien. After that he toured the United Kingdom, preaching Christianity with a strong German accent. As always he wrote a lot, typing fast with two fingers: 300 articles on Google, and books earlier on Goethe, Idealism (3 vols), Eastern Christianity (2 vols), Germany, and more (the collected correspondence, etc.). He was also very much a people-person.

After the war, my parents returned to Germany, Heidelberg in the end, where my father felt he still had tasks to tackle. (A high school was founded in his name by the former U-boat captain, friend, and headmaster Karl-Heinz Potthast.) My late sister, having gone to India as a missionary hospital matron, married an Indian theology professor, Elavinakuzhy John, who was visiting with us last month, one of their daughters and her husband now being professors in New Delhi.

In Newcastle much earlier, I had met Clemency (the daughter of one of my maths lecturers there, I discovered). We happily married, and after three years in Cambridge have lived in Dulwich, London and had three children, Stephen, Carey, and Deborah, and six grandchildren!

As a young boy my mother told me various funnies – all very brief but quite deep, as I recognised more fully only later. (She had always just let me think about them.) One introduced me to the typical correlation-is-causation conclusion of popular science (50 years later it was also told about Winston Churchill). She said she had had two great-uncles. One smoked two cigars every day and died at 83. The other never smoked in his life and died when he was 2.

AE the Younger

Sponsors

Over the years, our research was generally supported by industry, especially by Unilever and Beecham (GlaxoSmithKline) already to begin with, and Esso, Shell, J. Walter Thompson (JWT), General Foods-Kraft, Procter & Gamble, Mars, Cadburys, Heinz, General Motors, Pfizer, CBS, and a good many others on both sides of the Atlantic.

Recently the work has been run as the *R&D Initiative* at London South Bank University and the Marketing Science Centre of the University of South Australia in Adelaide, with Byron Sharp there as R&DI director now. Numerous companies (competitors even) can share and discuss our results early.

A Super-Model

Back in the later '70s we now needed, if possible, a general theory of consumer behaviour to account for our many different repeat-buying and brand-switching regularities: it was all very well in practice, but how did it work in theory?

After some thought, Gerald Goodhardt and Chris Chatfield realised that, in brief, all our very diverse empirical generalisations could be "predicted" by technically mixing the earlier probabilistic model for repeat-buying with the so-called Dirichlet probability distribution to cope with brand-choice (itself a mixture of not very far-fetched Beta-distributions).

Unlike other theories in marketing, this so-called Dirichlet model was "simple" – it needed only a single identifying data-input for each brand, how big it was (its market-share)! Yet it had many validated outputs for each competitor (with one minor but still quite unexplained deviation).

Marketing-mix inputs like changes in advertising, product- or service-quality, retail-availability, or price were not needed in modelling marketing's usual near-steady-state markets. We felt, and feel, that any dynamic situations potentially involving such complex intervening factors were far better tackled separately, against the model's steady-state norms. Some longer-term erosion of loyalty was, for example, isolated later in just this way by Robert East and Kathy Hammond, as well as numerous subsequent insights into new brands, price promotions, and so on.

However, I also realised slowly that our kind of theorising – which at base describes and explains already-established and generalised empirical discoveries and which thus *post*-dicts them – was anathema to many American academic marketing colleagues. They espoused much more ambitious and complex-looking econometric procedures which never worked in practice, with the recent citation for a Nobel typically not referring to any established empirical patterns: It is what a close colleague had already many years ago at the AMA's Chicago conference labelled the Scientification of Non-Knowledge. Hence "I SoNK therefore I Am" (not uncommon also in modern theoretical physics).

Sadly, there has been little dialogue with US academics over the years. Was I too outspoken?

Consumers' Attitudinal Beliefs

We also tackled consumers' attitudes from the mid-1960s on, for the late John Treasure at JWT in London and New York working with Michael Bird, and later with Patrick Barwise at LBS. Would people's expressed attitudes – what they think they feel about brands – also follow simple and generalisable patterns?

Not yet knowing what we were specifically looking for, it first slowly emerged that consumers' expressed intentions-to-buy a brand in fact foretold their past purchases of it, and hence also their future ones if they were, as usual, much the same. But not any future *changes*.

Users of a brand usually liked it (i.e. ticked evaluative questions like "Tastes Nice"). That shouldn't have been news, but was. For example, the widely measured "Appreciation Index" (AI) for all UK TV programs then was largely platitudinous:

"People mostly said they quite liked what they watched. And they mostly watched what they said they quite liked".

We also found increasingly that competing brands had much the same "image" among their users, notwithstanding David Ogilvy: Users of brand *A* would feel about *A* pretty much what users of brand *B* felt about *B*, at least for "evaluative" beliefs ("Tastes Nice", rather than the descriptive "Is Blue"). This made sense (to us) since competitive brands generally copy each other, whatever people may say about the supposed need for brand differentiation.

An extensive attitudinal repeat-interviewing study (with Neil Barnard and Patrick Barwise and later Francesca Dall'Olmo Riley) showed inter multa alia that

- People's attitudinal responses wobbled "stochastically" over time (as-if-randomly), like their buying behavior.
- Interviewing respondents did not affect them subsequently (long an industry-wide nightmare).

For *systematic* attitude shifts, various indications were that attitudes changed *after* behaviour.

Numeracy & Literacy

In the early 1970s, Stephen King of JWT (he had been the progenitor of Campaign Planning) wrote that "Andrew Ehrenberg has green fingers for data". This seemed at first oddly absurd. Surely the patterns and exceptions in our data were obvious – anybody like Stephen could see them (if they looked).

But then I realised that no one (certainly including me too) had ever given any explicit guidelines or rules for how and why we had come to set out our tables of data so as to make any patterns in them become self-evident. Making this tacit know-how explicit, oddly aided by my slight dyslexia, took much time and effort (Thank you, Stephen). It ended up as the *Data Reduction* book (the perceptive review in JASA, the leading US stats journal, said "My first words must be, buy two copies now – one for yourself and one to lend"). Followed by the briefer but still quietly-contrarian *Primer* in statistical methods, and two popular training videos.

For any table of data, two steps – drastic rounding and ordering rows and/or columns by size – always make the data vastly more graphic (the

famous John Tukey's "interocular" – hitting you between the eyes. Or green fingers for everyone).

Few people, if any, can divide 35.2% by 17.9% in their heads (without mental rounding). Two mathematicians in a seminar at Purdue University years ago said that they could. But they gave different answers, so that at least one of them was wrong. Yet when rounded to 35 and 18, we can all see that one number is obviously about twice the other. The fault, dear Brutus – lack of numeracy – is not in ourselves, but in our data.

For statistical graphs, we noticed that briefly summarizing a typical line-graph's wiggly messages in words (e.g. by saying "Sales mostly went down") worked wonders for the struggling onlooker. Verbalised captions were also remembered far better than the most explicit wordless pictures (like the earlier graph here without the "agree").

Tests of such precepts were carried out with Chuck Chakrapani (more are needed), and many theoretical explanations came from psychologists' vast pre-neuroscience understanding of memory processes. For example, the great Herbert Simon had reported that people could not remember numbers of more than two digits if they were interrupted in any way, even if only by their own thoughts – as in thinking about the emerging answer where mentally dividing 17.9 into 35.2. As my LBS colleague David Chambers said when showing me Simon's monograph, that explained my enthusiasm for drastic rounding and Simon's Nobel Prize.

For writing technical reports, where I had long learnt much from Helen Bloom, we developed in some detail precepts such as, in brief,

- *Start at the end*. (Give all the conclusions and main findings first.)

- *The 'Fog Factor'*. Patrick Barwise's great simplification of the guiding rule for having few long words (three or fewer of three or more syllables per average sentence).

- *Be Brief* (e.g. like 70+ years in 5,000+ words).

- *Revise strenuously*. (This 26th version of this my pre-obituary has itself had over 400 changes:

  > ars est celare artem

  the art is to conceal not just the art but also the laborem. Or as *Dr. Johnson* said: "What is written without effort is in general read without pleasure". (For 'effort', i.e. REVISION, click on the AE website shortly.)

  Dr. Johnson had also quoted an Oxford don's astounding injunction: "Read over your composition, and whenever you meet a passage which you think is particularly fine, strike it out" (unless of course you already know from the reactions of others that it *was* fine.)

The same pattern under different conditions: Differentiated Empirical Generalisations

Over the years, we found that the same Dirichlet repeat-buying and brand-switching patterns as for Soap and Soup also recurred for the very different Gasoline – the world's biggest private labels brands, and sold at solus-site outlets (with John Scriven); Aviation fuel contracts (with Mark Uncles); Ready-mix cement (Chris Pickford and Gerald); Store-choice (Kau Ah Keng); and (with Jay Singh and Gerald recently) category-variants like different flavours or pack-sizes as in the table, where the observed loyalty-measures O and the theoreticals **T** typically correlate .99).

Loyalty to Large and Smaller Pack-Sizes
(Observed O and Theoretical Dirichlet **T**)

Laundry Detergents UK, 1999		Purchases per Buyer		% 100%-loyal	
SIZE†	*% Share*	O	**T**	O	**T**
Medium	*57*	6.8	**6.5**	23	**28**
Small	*23*	4.1	**4.3**	9	**10**
Large	*12*	3.3	**3.7**	10	**7**
Extra Large*	*5*	2.5	**3.4**	11	**6**
Average	*24*	4.2	**4.5**	13	**13**

† In market-share order   * Some discrepancies

And for GP's prescriptions – which the GPs neither consume nor dispense (Philip Stern); Computers and Impulse purchases (Colin McDonald); and as below, media consumption, and price changes. All this was remarkable (i.e. worth remarking on).

In "subscription" markets the patterns of customer retention and defection looked superficially very similar but were fundamentally different (Byron Sharp, Malcolm Wright, and the ubiquitous Gerald). "Subscriptions" occur with financial services, and also, as it were, with durables like the makes and types of automobiles which motorists in France, the UK and the States bought next (with Bruno Pouilleau, John Bound, and Dag Bennett), and for TV sets in China (every fifth baby and one in four new TV sets are Chinese – Dag Bennett).

TV-Viewing. The studies here were very extensive, initially with Tony Twyman, and then very much with Goodhardt, Collins and later Barwise, producing 100 reports for the late Ian Haldane at the IBA in London (the erstwhile Independent Broadcasting Authority or UK FCC), and more for Lloyd Morriset at the generous John and Mary R. Markle Foundation in New York, and then the BBC. And also two books.

The patterns that occurred in TV viewing (e.g. DJ for Program- and Channel-choice) were generally already familiar to us from our well-established empirical generalisations for people's brand-choices. Many of these patterns however still go counter to common expectations. Thus people were by no means fully "glued to the box": repeat-viewing of program series was only 50% or so week-by-week in the past (now, with more channels, down to roughly 30% in the UK and USA).

Lord Birt's inanely clever-clever "there's less in this than meets the eye" because people's lack of repeat-viewing could in fact be explained, ignored TV producers' shock if and when they learnt that half or more of their audience this week would not be watching the programme next week.

A prudent questionnaire-survey with Pam Mills also showed television viewers' wide yet sensibly-bounded willingness to pay for more (broadly replicated later in Canada, the United States, Japan, and by the BBC recently).

Our various TV findings helped to thwart Margaret Thatcher's determined intent to privatise the BBC. The main findings are now being updated for the current polychannel situation, with the detailed findings from the '70s as fantastic benchmarks for how it was so long ago.

Price-Changes. In the '80s, our study of price differences and price changes was opened up by effective though expensive in-home experimentation with Len England.

Less costly *laboratory* experiments, then developed with John Scriven, generated consumer responses to over 1,000 price-scenarios under controlled conditions, including for some durables and services. Price-elasticities were found not to be specific to a particular brand or product (totally unlike the classic "X has a price elasticity of –3.1"). Instead, the sales effects of price varied with the context, differing for price going up or coming down, or for passing a reference price, and so on. (Our account here was last month given the 2004 "best paper" award by the *Australasian Marketing Journal*.)

John Scriven also established from scanner-panel data that consumers had Dirichlet-type harems of acceptable polygamous Price-Bands.

Other Practical Applications

Over the years, our findings were also applied to various other marketing issues, from new brands say, to advertising:

• **New Brands.** Having one day declined an invitation from Jim Figura at US Colgate to hold an in-house seminar with them on new brands (since we then knew nothing special about new brands), the realisation struck overnight that our theoretical Dirichlet norms would of course show what to expect for any new brand once it had "settled down". (That became our most popular seminar-topic for some time, with repeat-performances at Procter & Gamble's Cincinnati headquarters for instance, and elsewhere.)

Thirty years later, scanner-panel analyses of successful new brands (with Gerald Goodhardt) showed that loyalty to new brands unexpectedly stabilized almost instantly, with no special settling-down or "learning" period.

This finding was in fact not totally new. Three earlier isolated cases of near-instant loyalty for new brands (one case ours) had previously been dismissed by us and/or others as obvious aberrations. But these earlier findings had in fact already gotten it right.

• **No Brand-Segmentation.** An emperor's-clothes check of marketing's basic notion of brand segmentation showed that there wasn't any – different competitive brands appealed to much the same kinds of consumers in extensive large-sample Target-Group-Index data with the TGI's 200 potential segmentation variables across 40 UK industries. The paper in 2000 with Rachel Kennedy received three of our five "best paper" awards roundabout then.

Andrew Ehrenberg at home recently,
standing in front of Ludwig Alfred Jonas' portrait
of his great-grandmother Julie (aged 92 and blind).

• **Price Promotions.** Scanner-panel analyses with Kathy Hammond and Gerald Goodhardt explained why price-promotions fail to attract increased sales afterwards: The usual price cuts were far too small to persuade people who for five or more years previously had strenuously refrained from buying that brand. This study well merits to be replicated to increase its impact.

• **Advertising.** In our view advertising works differently from what is commonly thought in five ways:

1. Few if any advertisements are strongly persuasive, or even *try* to be so. (Pam Mills checked the latter out empirically with both professionals and consumers, for TV, Print, and Outdoor.)

2. After extended discussions with Neil Barnard, Helen Bloom, Rachel Kennedy and others, we mostly see advertising as 'mere publicity' for the advertised brand, to remind already-knowledgeable consumers (with ads resplendently saying "Coke Is It" and later "Always Coca-Cola", for consumers who already knew Coke).

3. A brand's *salience* increasingly seems key: Any propensity for the brand to come to mind or to be noticed (a notion developed further by Jenni Romaniuk and Byron Sharp).

4. Ads might very occasionally also nudge towards a purchase.

5. There is no evidence whatsoever that advertising induces people to buy products as a whole – cigarettes, alcohol, now fatty foods, salt and sugar. The lack of such evidence is convincing because the industry would of course love nothing better than positive proof that it can make people buy things. Instead, I believe that people largely buy them because they like them.

No Use for Statistical Techniques

As a quondam- or anti-statistician, I have never found my subject's commonly-used techniques, e.g. Gaussian least-squares regression or multiple factor analysis, to be of any use, like blood-letting or cupping.

My main reason is simply that these techniques have not led to a single lasting scientific discovery over the last 100 years or more, or even to mere *claims* to that effect. (Other reasons are more technical.) In contrast, my colleagues and I have been finding plenty of generalisable empirical

regularities without any such techniques, just like any other successful non-statistical scientists.

My early statistical doubts from Cambridge-days were fleshed out in various papers (some unduly long), with constructive counter-proposals. These were continuously applied, in our own data-analyses. They were also discussed in often standing-room-only seminars at MIT, the then-glamorous Bell Labs, and elsewhere ("Preaching what we Practised").

Over the years, we have also never sought to justify any result as being "statistically significant" – to verify that it had actually happened – as many data-analysts still do. Instead, scientific results are empirically replicated by hard slog, in our case for different brands, products, countries, analysts, points in time, etc.

In the 90s, these things were checked further with the *Car Challenge* (partly while visiting at NYU): Some 30+ leading modellers worldwide were invited to apply their own preferred analytic procedure to some simple repeat-buying data for new cars (the new make acquired and the previous make). The data had been replicated with very large samples in two countries and over four separate years.

We found, mainly with Richard Colombo, that the 20+ participating modelling experts had given 20 answers which differed about as far as 20+ answers could.

Many had moreover analysed only one of their eight data-sets: few had checked whether their new finding was replicable. They were heading fast, as far as they could tell, towards Cold Fusion.

Sum Ergo Cogito

Inverting, as a workaday scientist, the famous philosopher's "I think therefore I am", I still see my emphasis to have been on results which were both generalisable and simple (and hence, at times, even beautiful).

I do not see very many uncorrected mistakes in what we did (critics have perhaps been too kind). But I have become increasingly aware of gaps.

My oldest London friend for 50 years from the Maudsley (though we had never talked my shop) wrote last month:

> *59 Dulwich Village*
> *London SE21*   9 Dec 2004
>
> Dear Andrew,
>
> We enjoyed the dinner.
>
> This is about your paper. Needless to say, so far as work etc. is concerned, for me you exist on a very different planet. So your life and achievements were a delightful surprise to me. A whole secret world was opened up where wizards play with numbers and 'results' tumble out.
>
> I have often wondered what you got up to. Now that you have given us a glimpse, I find it fascinating. Endless challenge, a sequence of puzzles asking to be solved.
>
> And all you had to do was think. Marvellous! I hardly ever had to think: a very good thing for all concerned.
>
> Even though your technical stuff is written in a foreign language, I found your writing overall very refreshing to read (I'll overlook 'gotten').
>
> Thank you very much. I'm filled with envy and admiration. And the nice thing is that lots of other people seem to be too!
>
> Yours
> John
>
> *(Dr. John Fleminger, formerly Physician in Psychological Medicine. ca 1950-80, Guy's Hospital, London.)*

But not everyone. When I suggested two months ago that the middlebrow magazine *Significance* of Royal Statistical Society, my alma mater, might like to mention my piece and its website

very briefly, as a newsy example of an applied statistician's work, the Editor replied (01/11/04):

> Dear Andrew, … I needed some advice from the editorial board on this one.
>
> We think that the article although very interesting, **. . .** would be a difficult read for someone outside marketing, and doesn't actually describe much statistics (i.e. I didn't employ any classic statistical "techniques").

No wonder perhaps that the student numbers for stats courses have sadly been dropping.

READINGS

Published papers are listed at www.lsbu.ac.uk/bcim in a bibliography of some 300 titles prepared by my colleague John Bound, self-styled at 80 the world's oldest Research Assistant.

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