CHAPTER 8 LISTENING TO VOICES

It has long been held – well, at least since the dawn of modern Market Research – that the conceptual foundations of all new products must be firmly rooted on the careful assessment of the customer's requirements – the basic notion behind the "**Voice of the Customer**" (VOC) initiative, which implicitly asserts:

> "The customer is always right, meaning, better informed of the special needs of his organization than we, the product supplier. Our primary objective is to satisfy the customer; ergo, we must listen to his needs very attentively. Only then can we act".

It would clearly be foolhardy to deny the value of paying close attention to our customers' needs – particularly a large group of customers expressing similar needs. But, seriously now, is this how innovative products and solutions *really* break through into the market? Is this our *actual experience*, or rather is the pursuit of the VOC initiative a kind of fear, that our designers know so little about the market, that they must be allowed to work only on products whose objectives have been supplied by a marketing engineer, who in turn has been listening to a few specially-selected customers? This question is addressed at some length.

Clearly, the Company – the thousands of employees, including hundreds of chip designers, the IC processes, software tools, and IP environment – and world Markets have changed radically during the past thirty years. Today's approach to innovation must necessarily differ from what worked so well in the 1970's and '80's. We have far more *designers with scant experience*, and we cannot just tell them to "Be like Tesla!". Rather, they need a clear understanding as to how they will get their various assignments. This is how it should be, even though we might wish every one of them was an experienced self-starter: skilled and resourceful in technical matters, and well-informed about specific markets and opportunities.

It is agreed that, for a considerable fraction of the design community, *guidance is needed more today than in our earlier days*. The question really comes down to this: Where should this guidance come from? How do we help the new recruit to see the need for balance, between "getting on with the job (and never mind why these are the objectives)", and acquiring a broader understanding of principles, of the application(s) for their product, of the nature of our industry and markets? It is essential to understand this dynamic, since there is *a deep philosophical schism* between those who perceive the VOC process as one of the most potent tools of innovation, and those, like myself, who feel that a close consideration of the *customer's often unstated needs* has always been an instinctive and essential aspect of testing, validating and selecting promising product concept.

This is just a natural and necessary facet of *attaining personal mastery* – which must be every individual's ultimate goal. For a designer, this mastery must extend well beyond the local technical milieu, to the rest of the Company, the rest of the Industry, and to the fundamental nature of the market for integrated circuits. Stated differently, it is about feeling the *pride of ownership of one's field of endeavor.*

Listening, no matter how attentively, to the customer regarding his current needs, even probing with tough questions, is *by no stretch of the imagination the path to leadership*. Our competitors can, and actually do, just the same sort of "research". Nevertheless, listening is undeniably a critical aspect of remaining competitive, and essential to ensuring that the most optimal and attractive derivative products will be included in one's portfolio. My criticism isn't about whether or not to "listen to the customer". Rather, it is that *we appear to be slipping inexorably into a new paradigm,* in which fewer young designers ever have a chance to come face-to-face with customers, and in which the default expectation is that the development objectives will be provided by the relevant Marketing Team. *This cannot be the path to long-term leadership.*

Surveying modern commodity items you would probably discover that a sizeable fraction were introduced simply to pander to a detected consumer preference. These are invariably utterly predictable, yet elevated to something approaching PhD dissertations; such as the "discovery" that people prefer pastel colors for facial tissues over primary colors, that products with attractive packaging outsell those with black-and-while labeling, and the like. A large fraction of "innovations" are *trivial adaptations*, developed merely to maintain the momentum of competitive interest or achieve a critical level of product differentiation, like the umpteen variations on the plastic-entombed razor blade, all of which are incompatible, yet all of which do precisely the same mundane thing. Of those that were slightly less trivial, you can find some that have a minor practical value, such as the bend in the neck of a plastic bottle of toilet bowl cleanser, or the room deodorizer that plugs into an outlet and thereby benefits (maybe) from being slightly warmed.

These "advances" were probably the outcome of protracted marketing exercises. As you sifted through the ashes left behind after burning off all the incremental "improvements", you'd probably find some genuinely clever and useful ideas. We can be sure that these latter products would share one significant attribute: they would be the *completely unexpected* things: the developments about which we feel compelled say "Brilliant*!* Why didn't I think of that?", or, "How did we ever get along without this?", or "Now *that's* really neat*!*". Chances are that a high proportion of these products came from *spontaneous proposals* made by engineers.

Consider for example the telephone answering machine. Do you really think that, some many years ago, the phone companies carried out a survey of their customers asking what new features they would like to see on their phone? Do you think that a similar VOC exercise was launched to find out whether we would like hands-off dialing based on voice recognition as a new telephone-network feature? Or do you think it more probable that these were the result of engineers who saw *the possibilities* afforded by an emergent technology, and then worked to promote them?

Somewhere, somebody has surely seen the potential of a phone married to a computer, using a hard disc[81] for storing everything, including messages; but the average customer is unlikely to be able to envision the potential benefits of this combination, far less articulate them. Quite the opposite is true: customers for today's technogadgets often complain that they are bamboozled by all the features loaded into these little monsters. In one survey it was concluded that most folks used less than 30% of the built-in capabilities of their various gadgets, from microwave ovens, to entertainment systems and their remotes, to PCs and cameras. Clearly, the inclusion of all these excesses is not based on listening to the "Voice of the Customer", but from *the all-pervasive competitive gradient*.

An excellent example of *an engineering initiative* is provided by the development of the Philips Compact Cassette and later the Compact Disc. I rather doubt whether users of the standard reel-to-reel tape recorders of the day were begging for a miniature cassette format. Reporters had their ruggedized field recorders using a 3½" reel of ¼" half-track tape. Professional audio studios had their huge reels of ½" tape running at 15" per second, and would have scorned the idea of giving it up. The consumer had perhaps a Sony for making the official record of Jimmy's first words, and then his first birthday, or for compiling anthologies for their far-flung relatives, using as a source their very-adequate-thank-you LP's and EP's – even a few 78's. *I imagine practically everyone was happy with the status quo.*

Now… can you imagine the field-day enjoyed by the naysayers when some hot-heads in a small town in Holland (Eindhoven) proposed the use of an ultra-thin 3-mm-wide tape, on which they expected to lay down *four tracks* side-by-side and provide adequate audio quality? Who will make such films? Where were the players going to come from? How can you make such a complicated multi-component system cheaply enough for the mass market? How about the transport mechanism? *What?!* You are going to run the tape at *1⅞" per second?* Uh-oh. So how do you propose to get the record-head gap down to the 1μm range needed to resolve 15kHz? And wow and flutter will be *awful*….

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[81] This section is another throw-back to the 1994 essay.

Imagine how much greater was the criticism that was probably leveled against the Compact Disc! No-one had ever proposed such an ambitious use of *optical recording techniques*, using *digital data* for high quality audio on a neat 11.4-cm disc. The data density would have to be unprecedented! The disc surface would be too easily damaged. Why put all that effort into something that no-one has ever asked for? You are going to need *inexpensive lasers* to read those miniscule pits: but none are available. And who's going to make the D/A converters? Our friends in Nijmegen? (Yes, as a matter of fact: Rudy van de Plassche for one). And how do you propose mass-producing these marvelous optical memories? *What?!* By *a printing process?* We wouldn't be surprised if some, if not all, of these negative comments were hurled at the proposers of the Philips Compact Disc. Yet who of us needs to be reminded of the influence that this *anticipatory innovation* has had on the world? All the same questions of cost and the challenges of building in the extraordinary precision into the transport mechanism in the home CD player would have arisen as for the Compact Cassette, except much more so. But they did it.

An Engineer's View of VOC

At a company such as Analog Devices, an earnest and sincere interest in, and involvement with, our customer's problems is important – and frequently essential – right from the start of any new product development. *Skillful probing* of a carefully-selected sample of users who plan to incorporate some new component into their next system design, and *close attention* to their stated requirements, may well lead to valuable, and perhaps even unexpected, insights, which need careful assessment before the development of these products is undertaken.

Occasionally, theirs may be the *only* opinion that counts, and their specifications drive the development from start to finish. Usually, though, it is the responsibility the development team to *creatively interpret* what is heard, drawing on a fund of knowledge and exercising a flair for identifying beneficial new combinations of functions and architectures to better serve the customer's needs (which are frequently unstated or inarticulate), in a *predictive, anticipatory manner*. We need to be especially careful of falling into the trap of believing that *generalized* marketing research methods, of the sort that helped to define the color and styling of the nth-generation of Kleenex boxes or detergent bottles, will be equally effective in pointing the way to new directions, in the surge of microelectronic "fashions". We may sometimes kid ourselves that radically different types of IC products actually came out of market research, the actual **"Source of the Art"** is often quite fuzzy. Many of our most valuable product breakthroughs, particularly in the arena of standard linear ICs (SLICs), were rooted in *one individual's personal experience* of established practices and a knowledge of trends in electronics.

In such cases, the solution the designer eventually promoted was based as much on a *vision* – a long-term view over the horizon, and a wealth of *stored ideas* about, say, critical performance requirements for products in the customer's particular field of endeavor, or about suitable implementation techniques with which to address those needs, painstakingly accumulated and codified over a long period of time – as it is was based on some customers' voices, attended to during a few short interviews. *The launching of radically innovative products has always been a risky business, and more a matter of engineering judgment than marketing science*.

Indeed, our field trips to the customer are primarily undertaken not so much to nourish the innovative processes (although that is certainly a hope) but rather, to establish the realism and scope of the *business opportunity*: the unit volumes, the package style, the pricing, the NRE, the schedule, the finger-in-the-air promises of first samples.

Doubtless, this is important information on which to base difficult decisions *about multiple-opportunity selection and project resourcing*. So yes, it's prudent to be attentive to what customers may tell us about specific technical matters and spins. However, it is – or it certainly ought to be, if one is on top of one's field – *unusual to learn something about the function or specifications that will be unfamiliar to one deeply immersed in a specialist area of product design.*

In contrast, the success of application-specific ICs (ASICs), special-purpose linear ICs (SPLICs) and user-specific ICs (USICs), which invariably provide specialized and *hitherto unavailable complex functions or combinations of functions*, depends critically on meeting an external, customer-driven set of requirements. It requires *highly focused listening*, a keen sense of *the privilege of being invited* into the customer's plant, to participate in their planning processes, and a palpable appreciation of the importance of taking thorough notes on every point being made by the customer, during these brief and precious opportunities.

In this process, one is looking for *subtle murmurs and undercurrents* in the stream of conversation, which could be valuable clues about crucial details that are nevertheless left *unstated* by the customer – sometimes out of a concern for revealing too much about a project. The highest hope of the VOC advocate is that, when all this is done according to a tightly-prescribed process, a clear picture of the customers' real needs will emerge, providing the basis for an action plan. On occasions, though, even these highly-specialized products don't get *completely* defined by the customer, using the VOC process. That may happen during the rigor of sifting through all the requirements of a system specified in *general* terms, frequently by reference to *operational standards that are freely available* in the public domain, equally to us and to our competitors.

Numerous case-histories underscore this lesson. In one instance, a leading engineer at Apple knew in *broad terms* that he needed an encoder to convert RGB computer video into an NTSC or PAL television image; but he had no idea how this should be done. It later transpired that the he had approached me at the NW Labs only because he knew that some sort of *modulation* was involved and that *we were the undisputed leader in analog multipliers!*

Well, of course, multipliers *can* perform amplitude modulation and mixing; but none of the available products would have been suitable for implementing the desired function. Thus, we had been the company's first choice on the basis of *our leadership in an almost irrelevant field!* On the other hand, our broad analog signal-processing *skills and experience were totally relevant*, and of course, we would say that, all things considered, they made an excellent choice*!*

However, this engineer could not supply us with any specific technical objectives. In the months that followed, it took an enormous amount of research to find out *what really needed to be done*. We drew on our prior knowledge of television practices, consulted relevant standards documents, and talked with specialists in the industry, including an old friend of mine in the Television Instruments group at Tektronix, John Horn. Bit by bit (one might say pixel by pixel), the AD720 incorporated the best of all this wisdom. Since its release as a catalog part, it has acquired a reputation for providing the best image quality of all the encoders on the market, due partly to the use of an on-chip luminance delay-line and accurate on-board chrominance filters, which are link-trimmed by laser.

Much of its success is a direct outcome of the dedicated product engineering work of Jim Martin and Tom Kelly, without whom the AD720 would still be vaporware. Thus, as is always true, this product was the result of a team effort: invention plus innovation. But the architecture, specifications and ultimate success of this product cannot, in all honesty, be attributed to any fine insights we learned from its original customer. We got 'invited' into the TV encoder business because earlier, based on our awareness of their *general* utility, we had developed certain other products (analog multipliers and mixers) and because, once we had sized up this particular opportunity by a few trips down Customer Lane, we then *independently researched the subject to internalize the challenge.*

Later, the NW Labs developed a second-generation design of this encoder; it needed only a single 5V supply, was built on a CMOS process and used an improved delay-line and automatically-tuned filters. Here again, although we talked to several customers about this second-generation part before proceeding, we found that *few were qualified* to make significant technical suggestions. Most of them simply wanted to know what might be coming along soon, the package and pin-out it would use, when it would be available, how much would it cost. The road to leadership remained wide open. By careful listening, *we discovered that there were many spin-offs* that we could have entertained, if we wished. These adaptations of a seminal core competency could provide numerous new capabilities, each specific to a small fraction of the total customer base. In the end, our second-generation product *was* shaped by further talks with new customers, but still only in minor ways. It was the beneficiary of all the learning about TV encoding that went into the first-generation design. More recently, we developed a third-generation part, the AD723, an all-CMOS encoder for operation on a single 3V supply.

During the early days of Analog Devices this was the *dominant approach* to new product development. We would first demonstrate general competence in some field, usually by having made *a unilateral decision* to add some novel function to the catalog, without a clear voice from the marketplace, and often on the whim of *a solitary product champion*. Later we'd discover that these generic competencies aroused the attention of new customers looking for ASICs and USICs, often in quite unexpected corners of the market. We didn't have to poll them or visit them: the product – our silent ambassador – spoke volumes on our behalf.

We can reasonably conclude that listening to the voice of the customer is of limited use in pointing the way forward to *revolutionary, trend-setting* products, of the sort about which others would later say, in irritated amazement: "Aaagh*!* Why didn't *I* think of that?". On the other hand, such VOC exercises are essential to the crafting of yet more specific, yet more effective, yet more efficient, *evolutionary* products, of the sort about which one will probably say: "Yup*!* It's *about time* somebody did that*!*".

Revolution is more fun than Accommodation.

Back in the early seventies, the task of picking winners out of our portfolio of new product ideas, as well as new IC processes, was entrusted to a small committee, called[82] the N3PC. We met, sporadically and infrequently, in pizza parlors, private homes (usually Paul and Sonja Brokaw's, the latter with a ready supply of cold beer in even colder glasses) or at Chinese restaurants, and usually under the chairmanship of Dave Kress. Our batting average was probably no better than what might have been achieved by simply giving the product champions of the time full rein to pursue their pet projects.

I'm still not sure, all these years later, how the recommendations of the N3PC ever became enacted*!* It was a pretty much a matter of first come (design complete) first served (a place in the wafer fab flow).

[82] The "New Products and Processes Planning Committee". If we'd included Packaging (as we probably should have) I guess it would have been the N4PC.

Somehow, though, the new-product process worked: the catalog of parts swelled, and the company did well. *Innovention was happening apace*, and not just in product design, but in many new integrated-circuit processes, new packaging techniques, new testing methods, and continuous improvement in proprietary thin-film technologies and laser trimming at the wafer level.

This practice of delivering to the marketplace an annual harvest of *anticipatory generics* – the "70's paradigm" if you like – remains a serviceable strategy for Analog Devices. Such *bird-in-the-hand products* are always of considerably more value to one's customers than a quadrivium of questionnaires and a plethora of preemptive promises. On the other hand, it would be foolhardy to overlook the importance and value of broadening, deepening and strengthening one's relationships with key customers. *Without them, the most daringly innovative products would be just so much artfully-coordinated sand*.

It would likewise be shortsighted to advocate a return to the fortuitous methods that just happened to work well in an earlier age, in an appeal to the style and mannerisms that characterized the company in the seventies and eighties. *Luck and circumstance always play a role in any history,* including that of the individual. But circumstances change. And today's global IC business has been in upheaval, with the advent of the large foundries, and numerous tiny, quick, clever design houses.

Nevertheless, the period between 1965 and 1995 was undoubtedly a time of usually strong innovention and rapid growth at Analog Devices, the period during which we were *sinking down deep tap roots*. In the current ongoing corporate process of "Creating the New Analog" we must be sure that we are correctly assembling our priorities, so as to utilizing precious finite resources in an optimal way. In part, this will mean continuing to apply our *established skills and aptitudes.*

Ontara is a Sanskrit word meaning "taking the next step in balance", the idea that a bold new journey should be undertaken only after thoroughly assessing one's present circumstances. Every new day is the right time to practice *Ontara***.**

CHAPTER 9 HOW TO SUCCEED WITHOUT REALLY TRYING

Here's a little "***What if?***" interlude. If you found yourself running your own start-up company, and had limited means with which to fund product development, you might consider *two scenarios for success*:

1. Bet on a few designers who have proven to be well-rounded, resourceful and *intimately familiar with the techniques and systems* used in your target market. Instruct them to spend a significant part of each day mentoring the less-experienced employees, encouraging them to sift through the professional and trade literature, and relevant standards, to further *broaden their awareness of the field*.

Then, equip them with the most powerful CAD tools and versatile IC processes that are available, then *set them loose* with *carte blanche* to apply their most imaginative creative impulses to their mission.

2. Train your small marketing staff to become *more effective communicators*. Send them on *frequent trips to distant places* to tune into customer's minds. Have them prepare psychologically-tuned *questionnaires and market surveys*, and carry out *statistical analyses* on the replies. Have them generate *revenue projections*, development budgets and schedules, and *prioritize resources*.

Then, hire a team of designers whom *you direct to act out these plans to the letter*, with clear rules for decision-making and *reporting* at every step of the way.

Obviously, this is a maliciously distorted representation of the alternatives. Both styles of organization have flaws. But the question remains: *If you were forced, as the owner of this fledgling start-up operation, to make a binary choice, which style do you think best characterizes the entrepreneurial spirit, and is likely to provide the better pay-off?* There can be little doubt about the answer in *that* context. It's clearly the first of these styles. Now, the next question is: Can a mature, multi-billion-dollar company like ADI afford to spend its resources *in a similar but proportional way?* Without doubt, companies developing advanced and ever more specialized components need all the market intelligence they can get, but at what price? And, in this new, strategy-based, market-focused, team-building world, what is the risk of under-funding – or even, God forbid*!*, barring – the development of those *high-risk entrepreneurial products, arising from the singular visions of a few lone mavericks?*

In today's Analog Devices, we can find examples of the first style still going strong. For example, our involvement in the communications world took a major step forward several years ago when a famous Japanese manufacturer of cellular phones requested a quotation related to the development of a special multi-stage log-amp. Their interest had been piqued by their earlier evaluation of the AD640 Logarithmic Amplifier. *Here was yet another product that was not the result of any sort of market definition process. Even on the day it went into production, not a single customer had yet been identified.* This clearly defies the street wisdom that says you make what the customer tells you he needs. You make what you know is a superset of those needs.

A Case History Elaborated

I will make the case that a small percentage of the Corporation's products should be totally entrepreneurial: products that the *customer doesn't yet know he needs*, to have them ready for that moment when he realizes the value of the new function.

The AD640 was the world's first five-stage monolithic log-amp, and the only log-amp to use laser trimming to provide exact calibration of its scaling parameters. Frankly, I chose to quietly develop this product because I personally felt it would be valuable in two ways. First, I felt we needed to augment our growing repertoire of wideband nonlinear circuits, which I had been developing for many years, and add to the catalog the logarithmic amplifier function, which uniquely converts its multi-decade input signals (over a dynamic range of 100,000:1 in later products in this family) to a simple-to-handle linear form, scaled directly in decibels. Second, I could foresee the widespread need for *RF power measurement* becoming significant as the pace of the then-emergent cellular telephony picked up speed. I was my intention to continue to pursue other like products of general value, *without waiting to be asked*.

I apologize if that seems to be an immodest statement, but it would be doing an injustice to the history, and color our understanding of how leadership innovation *really* occurs at Analog Devices, to pretend that this family of products, and many more like them over the years, had any other genesis. Of course, like all products, the AD640 needed the full support of a well-qualified team to bring it to the marketplace, including *the outstanding stamina and tenacity* of the unheralded *product engineers* who are hiding behind every successful product. And eventually, like all products, it had to acquire the all-important *part number* before it could be formally released. That piece of the development came out of the "Product Definition" process, which, as often happened in those times, was *written just weeks before release, after all the part had been fully developed!*

But I was telling you about that RFQ from Japan. What this customer wanted seemed preposterous: *twice the dynamic range* of the AD640, *single-supply* (rather than dual-supply) operation, at about *one-tenth the power*, some new and very tight *phase requirements* (though not quantified), and various other extra features, all in *a much smaller package* and all, of course, at some *small fraction of the cost* (I recall it was one tenth) of the two AD640s then needed to implement the function.

I vividly recall standing by the new-fangled fax machine just outside of Paul Brokaw's office, in the old Ballardvale building, reading with much amusement the request that had minutes before come in from our Tokyo office, wondering what kind of innocents they must think we were *to even consider bidding on such a thing*. After all, we were a high-class outfit: we didn't make jelly beans for the masses.

But *this* particular bean was planted, and *the technical challenge took root*. I couldn't put it aside. In this sense, the customer had certainly provided the needed *stimulus*, though not the *specifications*, which were pretty much limited to those stated above.

Later, doing sims throughout the night (when our time-sharing VAX-780 was more open-minded) I became excited by the possibility of meeting *a new set of advanced performance objectives* (which I judged to be the correct ones) and their cost objectives. Out of that customer's sketchy request I designed the nine-stage AD606. I dispensed with the laser-trimming used for the AD640, pared the design down to accurate essentials, found new ways to extend the dynamic range and meet the phase skew requirements, threw out one supply, and whittled 2 x 20 pins down to 16.

However, even though the AD606 was strongly based on that one customer's request (which I emphasize had been articulated as little more than a general desire to combine the function of two AD640's in a single low-power, low-cost chip), and even though we were listening hard for every scrap of guidance they could provide us, *the detailed performance specifications for this new part were all but impossible to elicit from the very systems engineers for whom we were specifically designing it.* They, it seemed, knew even less than we did about what really needed to be done*!*

So, where these specifications were missing, *I interpolated, extrapolated and applied my best judgment* as to what an ideal receiver would need in the circumstances, and added some useful features that weren't in the original request, aimed at *enhancing the range of anticipated applications* without excessive elaboration beyond what was essential to meet the original objectives in a cost effective way.

The learning process surrounding the AD606 project – about the systems in which the part was to be used, as well as the accumulated know-how of designing, specifying and testing such parts – grew into a major team effort that substantially furthered our capabilities in RF receiver circuits for digital phone systems, and *opened doors to new opportunities.* In developing this product, we gained invaluable experience and learned much that was later to help us advance the state of the art in multi-stage wideband amplifiers into even less familiar territories, eventually operating at GHz.

Before long, this same customer was back, clamoring for *yet more function* (the addition of a UHF mixer), an even lower supply voltage (2.7V min), lower power consumption (20mW) and, of course, all this for an even lower price! Now, we were *really listening to the voice of this customer*, because, in spite of the tight margins, the business opportunity looked like a good one.

Yet, once again, that C-voice was very, very weak. We were not really being given a performance definition for an ASIC, so much as being asked to add *generalized new capabilities while lowering the supply voltage, power consumption, size and of course, cost.* Always cost. We were again forced to do a lot of independent system research to produce a design, for which the number AD607 was eventually assigned. My design deviated in important ways from the customer's expectations[83]; it used a novel approach which I developed to address dynamic range issues and circumvent the technical limitations of the all-NPN process used for the AD640 and AD606.

My proposal was to use a *fast-acting servo loop* based on cascaded variable-gain amplifier cells, each having an exponential (that is, an "anti-logarithmic") gain-control law. This unconventional approach can accurately implement the desired logarithmic function, used for the Received Signal-Strength Indication (RSSI) function in a cellular phone system. This technique is now widely used in NW Labs' state-of-the-art power-measurement (true RMS) products, such as the AD8362. At this time, however, the customer didn't believe our approach would work – partly, I suspect, because *no-one had made log-amps in this unusual way before*. So the project was put on a back burner.

I remained convinced that the forward-looking concepts it embodied, which also included a novel *variable-gain mixer*, would have general market potential in new cellular phone systems. After some re-thinking we eventually developed the AD608, a fixed-gain mixer plus multi-stage log-amp, which was just what the customer wanted, using XFCB, a new, advanced and as-yet unproven silicon-on-insulator IC process[84]. Later, the AD607 was also redesigned on that process, for use in GSM phones. (That one part had reached 10 million units by 1999). Since this ground-breaking work, we have designed many such log-amps for use in cell–phones and their base-stations; they are now used by the tens of millions, and constitute *a significant business operation* in their own right. Inevitably, their success has attracted competitors like bees to the honey-pot, and we have seen some very vulgar plagiarism by certain competitors.

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[83] Though not from a suggested architecture or detailed performance requirements: neither of these were provided. Practically – and almost literally – all we had was a 'back-of-the-envelope' block diagram.

[84] XFCB, for 'Extra Fast Complementary Bipolar', a DI process bringing long-anticipated benefits to low-voltage, low-power circuitry. See later comments on the genesis of this ground-breaking IC process.

Similar case-histories show that the barely-audible **Voice of the Customer** often had to give way to the much louder and more authoritative **Voice of Committees** (such as those writing the relevant public standards) and the **Voice of Consultants** (to whom we had deferred to allow us progress more rapidly into the TV encoder field). Somewhere in all this you might have even heard the **Voice of the Champion**. They are *all* **VOC**s, and all are important. In pursuing an innovative approach to product development, a wide range of voices are heeded, including those all-important ones that come from within, the **Voice of Conviction** and the **Voice of Commonsense**.

We need to be careful in connection with the last two of these voices, though. *Comfortable commonsense can be the nepenthe that lulls invention into the sleep of complacency.* It's all too easy to view things in the same old fading light, particularly if some long-accepted solution seems "just common sense".

In this connection, I am bound to think of *the lemming-like use of op-amps* wherever voltage gain is needed. Op-amps are very familiar to us, and to our customers, but they are far from the best choice in many applications. How alluring is their promise of "infinite gain" but how far from the truth[85]. Nevertheless, they remain the first choice for thousands of users, while many new, and often far more suitable, amplifier techniques are ignored.

Dry Sticks and Microwave Ovens

Only a few years back, the commonsense way to boil a pan of water was to add heat directly, either by dissipating a kilowatt or two in an electric resistor, or by the oxidation of some energy-rich material (gas, oil, wood, whatever). Few would have been so crazy as to have suggested the use of *an arcane and almost-forgotten vacuum tube called a magnetron*. In fact, it's a safe bet that no-one actually working in the kitchen (the Customer, in this case) would have *ever thought* about the need for a different approach to something as prosaic as heating food. *I seriously doubt whether a VOC exercise was ever undertaken to poll cooks about their heating preferences.* The overnight success of the microwave oven is just one of innumerable examples of products which owe their genesis to a *truly innovative* approach to the marketplace - one that *anticipates an opportunity* before it is generally recognized, or which sees a way of *generating a demand* where there currently isn't one[86]. Out of the introduction of the microwave oven came the totally new, co-dependent industry of *instant meals*.

[85] Consider, for example, an op-amp having a unity-gain-bandwidth product of 100MHz, operating at a signal frequency of 10MHz. In this case, its nominal open-loop voltage gain is a paltry 10*!* Op amps are just integrators.

[86] More recent examples of technology push are ultrasonic clothes washing machines and 'flash cooking' using intense light rather than microwaves, an adaptation of equipment used in the IC industry.

As earlier noted, a whole new industry was created to support a radically different approach to the recording of music, using digital techniques: the CD. Invented by engineers at the Dutch company Philips, *this was a bold act of faith*. Surely, no-one was clamoring for all the features we now take for granted from this medium, and initially the technical challenges of implementation were formidable. The first players were very costly, and with an extremely limited repertoire of music to choose from, the CD player appealed only to a few brave purchasers, fascinated by its novelty. Now, it is *all-pervasive*, the dominant medium for music[87], and this remarkably innovative technology has opened up yet further markets, as the disc format allowing the recording and storage of prodigious amounts of digital data.

It's easy to be blind-sided into thinking that the right solution is the one that uniquely addresses the customer's *well-articulated, specific and immediate* requirements, as may be revealed in the VOC process. The truth is different. The product innovator must ceaselessly figure out *what precisely is the problem that is in need of solution*. If a customer is willing to look afresh at the specifics of his challenge it is usually not difficult for him to accept a proposal for a better overall solution. This seems to be widely understood at an intellectual level, but the lesson is quickly forgotten during a meeting at the customer's site. *It is an essential aspect of the innovative process to keep an open brief on all possible solutions*, even after the first one (the commonsense, obvious approach) has been discovered and appears to be 'good enough'. However, it is unlikely that the customer, with a particular mind-set shaped by his particular environment and pushed by the overall demands of pressing program objectives, will be interested in our delineation of numerous alternative techniques.

The customer's view is necessarily a limited one. Time and again we find that the invention, and much of the actual innovation, occurred when *an individual* saw a bold new way of achieving a commonplace task, or a function that is not yet available, but for which a *future demand* was felt to be likely, and heeded the internal **Voice of Courage** rather than that of the Customer, pursuing its development without the slightest guidance from the marketplace.

This relentless, even reckless, and *self-eclipsing search for "a better way"* is the hallmark of the committed innoventor, who is thus the iconoclast: not content with merely making a moderate and useful contribution to advance the state of the art, he or she seeks to *redefine that art*, to restart the art, all over again from a totally different perspective, often rushing to obsolete last year's best ideas in the process, *regardless of personal inconvenience, embarrassment or pain.*

[87] Well, that was true at the original time of writing this essay.

This story is told: In medieval times, a priest, a thief and an engineer were found in violation of the rules of some burgh, and they had been committed to death at the blade of the guillotine. This was the best public entertainment in town, so every-one flocked to see these events.

Two concessions made to the poor unfortunate: first: he could choose to lie with his neck on the block, looking up, or face down; and second, there was a rule that if the blade mechanism jammed, it was understood to be a sign from God that this person's life was to be spared.

So, first up was the priest. "I'll lie face down" he said. "I have a thing about sharp blades". When the latch was released, the blade stuck mid-way down the guides. Now, fully assured that God was on his side, he stood up and waved jubilantly to the roaring crowd, most of whom were actually shouting "No fair*!* No fair*!* Try again*!*".

Then it was the thief's turn. "I'll lie face down" he said, "so that I can smile at the crowd". So it was, and the blade again jammed. The thief smiled cheekily at the roaring crowd, who were shouting "No fair*!* No fair*!* Try again*!*", and he stealthily picked the executioner's wallet as he left the platform, emboldened by the certainty that Lady Luck was clearly on his side.

Finally, with the crowd impatient to see blood, the engineer made his choice. "I'll lie face up", he said, with a smile. "I like to watch advanced machinery like this guillotine". He lay there for but a moment, looking up. Suddenly, he cried: "Hey*!* Wait a minute*!!* I can see your problem*!*"…

This really is how engineers think*!*

CHAPTER 10 HISTORY THROUGH DARK GLASSES

One chilly winter's evening in November, 1878, a young man of thirty sat by the fireplace reading a new self-help book. Originally entitled "O So-E-See", by O. Saki of Osaka, it had been popularized in the US by the liberal adaptation *"How to Find Out What People Really Need and Thereby Become Rich and Famous"*, written by the Harvard professor of marketing Ulysses K. Ansell (known as "U-Kansell" by his students). The introductory chapter noted *"....the first step to Invention is to find out what people really need; and this can only be achieved by market research".* In a later chapter, it finally got around to a discussion of the key idea, the Saki Number, which had caused Tom to jump right out of his armchair.

Excited by this new knowledge and fired with enthusiasm, he had set out the next day to systematically poll the residents of Newark, New Jersey, and thus find out what they *really needed* to light their homes. He was getting a little tired. He'd walked miles in the rain, and the responses were all so boringly and predictably similar. He felt he'd already amassed enough information for a reliable sample. But he resolved to complete the full 111 inquiries. "That will tell me *exactly* what these people really need," he told himself.

In fact, he was subconsciously recalling the words of U-Kansell: *"It has long been believed that the greater the number of people (N) one interviews, the higher will be the accuracy of one's prediction as to What the People Really Need. However, the mathematical analysis of Saki (which, I confess, is rather beyond my humble powers of comprehension) proved conclusively that there is a unique value of N – the Saki Number, $N_{\text{采}}$ (I think he said it's exp($3\pi/2$), or something) – which, surprising as it must surely seem, provides an exact (how did he put it? ah yes: "analytic") answer to this question, and in far less time! My humble contribution has been to misapply this remarkable insight to the sacred matter of market research, and I conclude that: By listening to exactly one hundred and eleven opinions, no more, no less, the probability of discerning precisely what clever invention is needed to satisfy the market demand will be exactly 100.0000%."*

Tom knocked respectfully on the one-hundred-and-tenth Newark door, which eventually opened, tentatively and cautiously. Once again, he started from the top of his research algorithm, speaking his usual script to the figure in the dark 15 degrees of the opening.

"Good evening, sir! My name's Edison, and I'm interested to learn what you might find inconvenient or inadequate about the present way you illuminate your home. Is there perchance some improvement that you would like to see on the market?"

"I dunno who you are, young man," growled the burly homeowner, "but yes, as it so 'appens I *can* suggest a couple of things, seeing as 'ow I am m'self an inventor – just here on a shart trip, visitin' America".

Then, opening his door wide and extending his hand, large and practical, he said: "Joseph Swan's th' name. Glad to meet ye. Look - First, you need to invent a stronger but brighter gas mantle. Those durned things are *always* breaking. The new type should use a stronger, more durable emitter a – metal – say… say, a *tungsten wire*".

History tells us that Joe Swan wasn't telling all he new about light bulbs; but Tom's script didn't allow any deviation, in order to keep the data flat over the sampled population.

"Yes – that's right, that's right*!* jot all that down. And while yer at it, you might do something about them bloomin' leaking gas pipes. At least, add something pungent to the gas supply, say hydrogen sulphide – to make the leak immediately apparent, y'see? If you can do that sort of thing, young fella, you'll quickly find yourself off these dark streets*!* Good luck to ye*!* Here, take this quarter and buy yurself dinner: you look starved*!*".

Tom gave fleeting attention to the thought of inviting this evidently inventive man into partnership. But at that moment he was feeling very discouraged. Yes, he was hungry, soaked to the skin and nearing the end of this long day of customer interviews; but he didn't need any charity. While only twenty-three, he'd already made $40,000 from the invention of the **Universal Stock Printer** for Western Union; and he'd developed some derivatives of the popular **Morse Telegraph**; he'd breathed new life into Bell's primitive telephone by selling the rights to his invention of the far more sensitive **carbon granule microphone**; then there was the **Phonograph** venture, using little, molded clay cylinders, whose demand was slowly catching on as a mass-market product.

No-one had asked for the phonograph, of course; nor the improved telephone; in fact, *none* of his inventions, come to think of it. But Thomas Alva Edeson (his Dutch family's spelling) had a pretty shrewd eye for what would sell, and he routinely shrugged off myopic naysayers, as all original thinkers must. He claimed he was a *commercial inventor* who worked for the silver dollar.

In fact, he was the quintessential opportunist, relentlessly, and sometimes recklessly, focusing his energies on the development of devices that *anticipated yet-unarticulated needs* – novel devices which only later would enter into popular use, when people eventually realized they truly needed them. He was a Pioneer–the *active market-maker*, not a passive market-responder. He was quite happy to leave developments that merely *followed some existing demand* to all those homesteaders who would came later.

At least, that was his *modus operandi* before his conversion by the famous Prof. Ansel. But ah, those heady free-wheeling days, he now realized, as he plodded wearily along the muddy street, were the old-fashioned way of doing things. Market research was the modern thing. Just ask, and ye shall receive the jack-pot. Still, he felt just a mite resentful. When it came to home lighting, he would have welcomed an opportunity to promote his *current* ideas. But with the Professor's words emblazoned across his forehead, young Tom went resolutely up the Seven Steps to the final door, the Saki-dictated 111th, and oscillated the heavy brass knocker.

A muffled conversation issued from within. While waiting, he thought: *"Hmmm... I could fix things so that the mere touch of a little button on this door would melodiously ring a bell in the living room; then an annunciator panel would show which door was involved...".* Many elaborations of the idea jetted back and forth through his buzzing consciousness. Then he quickly corrected himself. *"Nah, no-one's ever asked for that, so it's probably not a good idea",* he concluded.

While reflecting on the stupidity of *even thinking* about ignoring the Harvard Professor's sound advice, and actually inventing and marketing something that no-one had ever asked for (how foolish*!*), the door abruptly swung open, and he was confronted by a stern, ruddy-faced matron of ample proportions, in all three dimensions.

"Yesss?*!* What is it?", she hissed, clearly irritated at having her supper interrupted.

"Good evening, ma'am, my name's Edison, and …".

"Who??*!*".

"Um, Tom Edison, ma'am, and.. "

"Haddison? Never heard of ye".

"Well-ah… no; probably not. I am keen to learn what you might find inconvenient or inadequate about the present way you light your home. Is there any improvement you could tell me about – something you'd like to see on the market?"

"My boy", she said, her chest on his nose, "there's *nothing* in the slightest *"inadequate"* about the lighting in this house. We use oil lamps, same's we've done for generations in our family. Nothin' better*!* Now, if you can find a way to make them <u>twice as bright</u> and burn <u>twice as long</u> from one filling, well now... *that* might be something you could sell".

For the briefest moment, she allowed a flicker of a smile to show, as she reflected with self-satisfaction on the incredibly brilliant cleverness of her advice to the wretched rain-soaked youngster. But then she snapped

"But since you can't, be off with you*!* Find somethin' better to do with your life*!*"

The odor assailing his nose and the sound of the heavy oak door being slammed in his face convinced him that he'd listened to quite enough New Jersey voices for one night; and anyway, he had completed the one-hundred-eleven interviews specified by Saki. Tom hailed a hansom cab[88] and began the 15-mile ride back to his lab in rural Menlo Park. Within less than an hour, he had reaching the familiar building on unpaved Christie Street, right at the peak of the hill, conveniently located just a couple of hundred yards from the Lighthouse Tavern. Edison paid the cab fare with decimal exactness (generous tipping was not his forte) and shimmered into the lab through the back door. Then, pouring himself a generous Bourbon, he sank into his favorite old leather chair.

Nursing the aromatic tumbler in one hand, he heaved a long sigh of the sort only a tired marketeer knows, as the fingers of his other hand ran through prematurely graying hair. It was now quite dark. All the rest of his guys had long ago gone home to their wives and children – their *real* lives. Reaching over, he flipped a switch screwed to the desk. It was quickly illuminated with a warm yellowish light, emanating from a glass bottle of some sort, connected to a couple of closely-coiled wires; which, it transpired, flirted all the way down to a rotating machine in the basement, whence drifted the distinctive whiff of ozone emanating from the sizzling commutators of a small DC generator.

The desk was littered with scores of rough, handwritten notes – his current arsenal of great ideas – not all of which he could honestly claim were of his invention, though the patents somehow managed to use only his name. Nevertheless, they included his own experiments with filament materials, and sketches of different filament geometries. And here was that yet-to-be-signed patent application, entitled *"Vacuum-filled Light Emitters using Carbon and Tungsten Filaments Heated by Electrical Currents",* the ideas behind the very light that now flooded these documents. But there, on top of this kaleidoscope of ideas, was the good Harvard Professor's best-selling and popular guide to success, heavily dog-eared and profusely yellow-highlighted with Tom's fluourescin-filled fountain-pen ("Just another of my many 'bright' ideas, heh-heh", he quipped).

It was that august and seminal tome that had taught him a wonderful new way to greater success. It was so simple, so obvious, so downright commonsensical: you just have to ***Listen to the Voice of the Customer.*** Blinded by his lust for increasing market share, Edison was eager to embrace any tip or ruse in order to achieve his goal of being the richest inventor in the world (while continuing to discredit that crazy young fool Tesla with his iconoclastic and threatening new ideas about adopting *alternating-current* power generation and distribution using… what was it?... yeah, "transformers" – **Ha*!***).

[88] The brainchild of another youthful inventor, Joseph A. Hansom, (1830-82) originally a British architect by profession. These one-horse two-seaters were widely used in London and New York in the 1830's.

Since discovering the Saki Number, Tom never questioned the absolute necessity of discarding all his former foolishness – all that stuff about inventing products before the market had been consulted*!!* After all, the study was by Ansell, a *Harvard* professor.

Reaching into the deep pocket of his trench coat, Edison wearily pulled out his spiral-bound reporter's pad, and reviewed the day's research. Of the twenty-two people who had actually voiced a definite opinion, seventeen offered constructive suggestions. Moreover, and very significantly, he felt, **three** had actually concurred about the need for certain key improvements in their home lighting systems: They wanted stronger gas mantles and longer-lasting wicks for their oil lamps. *"All the same, it's too bad no-one ever asked me if I had any ideas as of my own,"* he sighed; and then ruefully recalled the stern caution of Prof. U. Kansell, that the VOC process must be conducted *"with decorum"* and *"in such a way that ..... one only elicits [those facts which the customer] freely wishes to impart to the research, without placing any ideas of one's own in the customer's mind."* – Chapter 13, page 1313,尜13. (That's Tom's fluorescent marker at work, in this most revealing historical document).

Struggling up from his chair, the young Thomas Edison walked over to one of his large oak filing cabinets, and opened a drawer. Heaving yet another huge sigh, and with painful resignation and reluctance, he tossed all those useless papers about "filaments heated by electrical currents" into a cardboard box, lying on the floor nearby, labeled "Waste paper only". "*Perhaps someday, someone, somewhere* will find a way to capitalize on all those ideas", he thought. Then, he sat down at his desk again, opened a blade on his pen-knife, methodically sharpened an HB pencil with all the élan of one who had consumed a great many pencils in his lifetime, placed the familiar gray-green engineering sketch-pad at a comfortable angle across the desk, and wrote:

Trip Report, 18th November, 1878

Did a VOC in Menlo Park. Interviewed exactly 111 people re: lighting improvements. Good info from seventeen… well, three actually. Action items: (1) Write a Product Start Document on "Improved Gas Mantles and Oil-Lamp Wicks" (do this before Monday's Executive Council meeting). (2) Convene a KJ to consider "Weaknesses in Present-Day methods for Manufacturing Gas Mantles". Ted Brown to facilitate. Monica: Check we have enough Post-Its and marker pens of the proper colour at hand. The development code-named "Eclectic Light" will be deleted from the 1879 Benchmark Plan. TAE

Time passes, as it is wont to do. For many years, the residents of Newark continued to absorb their Goethe, their Dickinson, and their Thoreau under the warm, reassuring light of the cozy-home-smelling oil lamp.

Time passes. Newer technologies - which share with the arts in defining our human life - creep stealthily forward, advancing in ten-thousand tiny steps, each no longer than an inch-worm's, toward that Brighter Day that commerce is prone to promise you and me. Each novel techno-toy offers instant delight. Of course, they are all powerless to present the gift of lasting joy[89].

Time passes. In Romania in 1906, *homo neuronus* is finally taking to the air under machine power, as Traia Vaiu makes the very first powered flight - an astonishing twelve metres in distance, at an intimidating height of one metre, scaring all the children playing in the park. Later down history's trail, Americans will hear only of the success of two kids with a bike-repair shop.

Time passes; and as it does, fashions change, too. People are now more inclined to lay back a bit, hosting cocktail parties at their homes each afternoon at about four-ish, inventing names for scores of highly toxic drinks, all of which exploit the willing chemistry of the mischief-making molecule C_2H_5OH. Stovepipe hats are now right out of style, since they tended to get blown away at the frightfully high speeds at which people now travel in the new AutoMobile – actually the invention of a German called… No, we won't go there. But all this time, progress in *lux humanis* has been stalled. Nothing any nicer than the improved oil-lamp (using Edison's EverReady™ wicks) and the new, easy-to-operate GasLite™ (using "his" tungsten mantles, of course) has yet come along.

[89] Okay.. I confess: having the means to play Mahler in my car, my bedroom, or even inside my electronically-silenced head, or watch an opera on a big screen in high-def, ranks high on my score-list of useful advances; along with digital photography.

CHAPTER 11 WHAT HAS CHANGED SINCE EDISON?

Of course, Edison didn't work that way, and would scoff at the very thought. We can safely assume that he never pounded the streets desperately *fishing for ideas*. Indeed, it is highly unlikely that he ever conducted a single market survey. He certainly did not spend his time in conducting faddish and peculiar KJ sessions[90], or writing new product proposals for committee consideration. But of one thing we can be sure: He had a strong internal 'voice' – the **Voice of the Convinced** – and he possessed a distinct, innate flair for knowing what was marketable[91], even though his ethics are questionable.

Clearly, we can't pursue invention in precisely the same free-wheeling fashion as Edison did. Our world is very different to his. Nevertheless, that boisterous, flamboyant and self-assured entrepreneurial spirit, which he and other long-dead pioneers had, can still be a source of inspiration to us today. The fundamental challenge, after all, remains essentially the same: thoroughly master your domain; become intimately familiar with the present needs of the market in the broadest terms; attend to them diligently, but spending only the minimum necessary time in so doing; rather, put much more time into understanding the limitations of the extant products, while also – as a matter of routine – pursuing in a background mode numerous alternative and novel anticipatory solutions in readiness for that certain time when all the product opportunities that you saw on the far horizon finally come into full view of everybody.

It must remain true that there are major differences in the way we innovate nowadays, and it's worth thinking about the nature of these differences. What is it about our world that has changed so much? Why *can't* a single, strong-willed inventor turn out product ideas in such proliferation as Edison did? Is that even an historically correct assumption?

Well, first, we have to acknowledge that he, and Tesla, Faraday, Fleming, Marconi, Steinmetz, Zworkyin – to name only a few – were extraordinarily gifted men, possessed of *relentless energy and self-assurance*. It's sometimes said that they were "bigger than life". Furthermore, they were born into a period of history when the sheer scope of opportunities for electrical, magnetic and proto-electronic devices had not yet been fully grasped, and their potency in everyday life was still not widely appreciated. Arguably, it's dead easy to be a pioneer when numerous untried and exciting ideas surround you like so many low-hanging plums. Hmm… but wait….

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[90] The KJ method for gathering and analyzing data, named after Jiro Kawakita, and first published in 1964 as "Partyship" (in Japanese, Pati gaku) is based on the belief that one can reduce complex engineering and strategic matters to a few simple statements through a particular kind of by group interaction. *See A New American TQM: Four Practical Revolutions in Management*, by Shoji Shiba et al., Productivity Press, Cambridge, MA, 1993.

[91] Usually, anyway; but in defending his empire of DC generators and distribution systems, Edison even used mendacious disinformation and public animal electrocutions to impede Tesla's promotion of AC as a better choice than his own.

Is this really the correct explanation of their success? *Are we not today born into a world where electronic technology, the potency of personal computing and ready access to information is poised to alter the lives of all humans far beyond anything we are witnessing today?* This is surely one of the most exciting and stimulating times in human history*!* Opportunities for invention and innovation abound. We are clearly in serious need of advances in energy harvesting, and must pursue every avenue that shows promise. The advance of medicine – in monitoring and sustaining health – and genomics offer further such **Meadows of Opportunity**.

It is clear that the fundamental nature of human society is undergoing an immense and terrifying transformation, much of which can be accredited to the all-pervasive micro-computer and to advances in global communications, enabled by satellites and optical fibers. We live in a world of multi-million-transistor[92] CPUs and DSPs, of extremely fast heterojunction transistors, of quantum-level devices, the fragile emergence of quantum computation and perhaps single-photon encryption paradigms, cheap lasers, direct optical signal-processing, neural networks, nanomachines, and much, much else.

These meadows are blossoming all around us, just waiting to be exploited by the eager innoventor; others are less ready but no less promising. Cannot a modern IC company such as Analog Devices, with its broad range of technologies and extensive knowledge of electronic hardware, provide a personal springboard to unimagined new conquests, just as effectively as was Edison's dusty lab in Menlo Park? *Surely it's no harder to be a pioneer today than a hundred or so years ago. The entrepreneurial opportunities afforded by the convergence of modern technologies cast a bloom of childlike naiveté over those early days of the Grand Electro-Adventure.*

Edison, of course, never was inspired by some mythical Prof. Ansell specializing in cute organizational methods. But he did devour the trail-blazing works of another remarkable innoventor, Michael Faraday, himself burning with the red-hot zeal of an adventurer and world-class discoverer. Faraday worked at the fringe. So often, when we study the history of the great inventors, we find that they were fired by ideas which, in their day, were barely in the foothills of some totally uncharted territory. Many of the ideas which Edison later turned to practical advantage in the USA were first conceived, but only tenuously exploited, in less market-oriented Europe.

[92] I suppose that must be changed to multi-billion, in this 2011 revision.

Edison also owed a huge, though never admitted, debt to his numerous employees, including the Serbian genius, Nikola Tesla, who worked for him for a while[93]. Incidentally, Tesla points to another necessary quality of the innovator: long hours. His were from 10.30am to 5.00am the following morning, with a brief break for a ritualistic dinner, every evening, in the Palm Room of the Waldorf-Astoria hotel, where he lived, and where he ran up huge bills he later would be unable to pay.

The contrast between these two innoventors makes a fascinating study. Edison was the eternal pragmatist who disliked Tesla for being an egghead; he prided himself on "knowing the things that would not work," and conducted his experiments with a tenacious but tedious process of elimination. Of this empirical dragnet, Tesla would later say, amusedly:

> If Edison had a needle to find in a haystack, he would proceed at once with the diligence of the bee to examine straw after straw until he found the object of his search. I was a sorry witness of such doings, knowing that a little theory and calculation would have saved him ninety percent of his labor.[94]

Tesla was an eccentric, a touchy and difficult man for others to work with. He expected the same long hours and dedication from his technicians as he himself put into his work. For these men at the turn of the century, electrical engineering was a vast unexplored frontier, bristling with opportunities to innovate precisely because there was yet essentially no electrical industry. Delivered into this vacuum, even basic inventions could have a dramatic impact; the competitiveness of a mass market would come only later.

However, these general circumstances are not unique to any age. Only the details differ as the river of time passes our way. Nowadays, there are already plenty of light bulbs[95] and electric motors, plenty of op-amps and DACs and microprocessors and memories. Taking a leaf from the deductive methods of Sherlock Holmes, today's burning question, surely, is: **What are there *not* plenty of?**

That was the essence of Edison's quest: he constantly thought about *eminently practical* electrical, mechanical and electro-mechanical devices, always with the pursuit of profit unashamedly in mind. Tesla looked out on precisely the same world with a startlingly different vision, a kaleidoscopic pastiche of radio and radar, world-wide communications, VTOL aircraft, robotics, geothermal energy and much else. However, just like Edison, he was possessed of *a deep reserve of personal energy* and *immense self-assurance;* he was fully aware of his unique personal talents. Above all, he had a very strong *sense of mission*, and what some might regard as a fanatical single-mindedness.

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93 Tesla introduced him to the wonders of alternating current. Edison treated him very badly, even cheating him out of $50,000 after successfully-completing a project which Edison challenged Tesla to undertake, and didn't think he'd achieve. Edison later launched smear campaigns when it looked like Tesla's visionary ideas about AC power systems threatened his empire based on DC practices.

94 Quoted from *Tesla, Man Out of Time*, by Margaret Cheney, Barnes & Noble, 1993, p32.

95 And for the first time, we are seeing the emergence of lamps in the marketplace that are meant to replace the hot wire.

Opportunity, Imagination, Anticipation

Innovention of the highest calibre springs from passionate personal attitudes. Strongly creative people feel they *own the domain*, that they're pursuing a sort of pilgrimage, toward destinations, and with intellectual assets, which are *largely of their own making*. Indeed, we should not be engaged in crafting the best technological products in the world just because a customer suggested them to us, even if assured us of fat orders; nor because of a formal job description that defines our work; nor because we want to impress anybody, but principally because we're fired by a *relentless compulsion* to bring some art, in which we have a *lifetime investment,* to the pinnacle of perfection.

When we survey today's world, what do we see? *Boundless opportunities for Invention!* Not fewer, because "all the good ideas have already been thought of" and "all the needs of the market are being fully satisfied by a huge industry", but instead, *far more, precisely because of the massive infrastructure that now exists*. How hard it would have been in Faraday's time to wind a solenoid*!* Where would he have obtained a few hundred feet of 20-gauge enameled copper wire? Not from the local Radio Shack. No: undaunted by a *paucity of materials*, Faraday had to machete his own path through uncharted jungles.

Today, making a solenoid is quite literally child's play. While Faraday may have spent a week, or a month, or even a year getting the materials together and then winding a coil or two, we just order what we need from the Allied catalog, for next-day delivery. Thus empowered by the infra-structure, *the level at which one can innovate is now far more potent.* Surely, we should be able do great things with all of today's technical resources at our disposal. The notion that *"Well, inventing back then was easy"* doesn't stand up to scrutiny. It surely must have been considerably harder to practice electrical engineering at the turn of the century. We're beset on all sides by astonishing technology begging to be put to new uses. But *lacking a vibrant imagination and exuberant anticipation of the value of one's own work*, even a Faraday would not benefit from the possibilities afforded by the entire modern technological infrastructure, which of itself points nowhere[96].

In no essentially different way to that exhibited by the pioneers of our field, we need to seize on contemporary opportunities, by understanding the value of new components, techniques, processes, and possibilities, based on what we know of the world's most pressing, immediate needs, and what we guess to be its most likely future demands. Intimate familiarity with these *opportunities* must then be matched with *imagination*, and an unswerving commitment to carry ideas forward, surely and speedily, to realize their potential before anybody else does: to *anticipate* future needs, not wait for them.

[96] Arguably, an abundance of means may actually impede the creative development of the young engineer. One of the most important assets a young mind can gain is the experience that comes from a paucity of resources. Having a few transistors, resistors, a battery and a soldering iron is probably more stimulating to the curious mind than owning a powerful PC.

Complexity and the Need for Teams

How else has the context for innovation changed from earlier times? Is the difference to be found in the *high complexity* and "sophistication" of modern technology projects? Faraday's solenoids, Edison's filament lamps, carbon microphones, DC motors and dynamos, and Tesla's super-coils and induction motors all seem rudimentary, picayune and naïve in retrospect, even while we recognize them all as equally revolutionary. This is undoubtedly where the greatest difference in today's technology workplace is to be found: *modern products and projects are frequently deep, complex and even arcane..*

But underlying even the most complex of modern systems, there are usually just a few simple ideas. Even the largest and most complex silicon monolithic integrated systems known to man – memories, microprocessors and DSPs – make use of *conceptually simple and repetitive logical entities*, the technological details of which have become secondary in executing the overall system design. Further, digital systems are *strongly evolutionary*. Being based on a large body of pre-existing knowledge, they advance incrementally from one year to the next. In this respect they differ profoundly from the iconoclastic and revolutionary character of the innovations of a hundred years ago.

Designers of modern mega-transistor entities might agree that, while the search for innovative architectures is crucial to protecting one's competitive position, the amount of radical invention in this arena is modest, roughly comparable to that exhibited by those advancing the state-of-the-art in the underlying cell-primitives, such as logic gates. *A truly revolutionary computer architecture comes along only infrequently.* Nevertheless, as the sheer scale of system integration continues to escalate, today's microprocessor designers have many more issues to consider, and to bring into balance, than formerly. Strictly to cope with the complexity challenge, they are invariably to be found working in teams.

The challenge in modern innovention now lies as much in project management as in coping with technicalities. Any time many minds have to work together, the possibility arises that the natural diversity and disparities of human viewpoints will impede progress. To address that, we play close attention to the schedules that coordinate the efforts of large teams; we standardized procedures; we fully define deliverables. The sheer number of steps and details in modern projects require the use of uniform design methods (so that all team members speak the same language), and standard system interfaces (so that all signals speak the same language). A greater dependence on re-utilization of previously proved (and necessarily very well-documented) solutions is usually apparent, so that the risk can be concentrated in a minimal number of new and unfamiliar steps.

But the very fact that these circumstances require *a submission of individual will* to the will of the community – as captured in a "Mission Statement", or simply an agreed list of objectives and operational rules – *they unavoidably become less conducive to innovation than a loosely-structured, few-rules environment*. This is why small start-ups often enjoy early success. These nucleic entities are only later faced with difficult management and growth decisions. They may eventually need to abandon the very thing that made them so successful at the outset – their individualism – to become a larger, more stable and credible supplier of a rather mundane product. Several recent cases could be cited.

Projects on a grand scale raise the complexity of the undertaking. Once the province of an individual experimenter or a free-thinking group of visionaries, today's innovation cannot progress without coordinated support from numerous others, having a wide range of disciplines, or without relying on ensembles of expensive and arcane equipment.

One still may be wondering: was it *so* very different in Edison's time? He was, after all, the "Team Leader" behind the construction of the generating station on Pearl Street; and I recall he was the individual responsible for managing the wiring up of several hundred mansions in New York city served by his power station. One does not need to know all the details to be fairly certain *this was an interdisciplinary task of considerable magnitude and risk,* which similarly involved the commissioning of expensive and (for that time) complex plant. The operative word in this case was not *"Team"* (of course a lot of people were needed for these undertakings) but *"Leader".*

Edison must have been a competent leader, although the popular image of a highly-admired team manager, patiently orchestrating clusters of dedicated manpower, is not supported by the pages of history. He is to be credited in the way *he anticipated emergent needs*, understood the potential of his ideas, and steered others to actually generate the reality. Nevertheless, he wouldn't conform to the image of a modern, democratic, team-builder. He was, by all accounts, *a self-assured, self-centered opportunist, a maverick;* driven, single-minded, yes, but committed to *a private agenda*; designing stuff that no-one ever asked for, yes, but fiercely committed to self-aggrandizing and to *the fabrication of his own myth*. What would Analog Devices do with a man like Edison? I suspect he would not be very well-liked; and perhaps even despised.

The markets for the fruits of modern technology also differ radically from those at the turn of the century: they are very mature, and *today's customers are far more sophisticated, better informed, more selective and more demanding*. They're well served, even at times overwhelmed, by a plethora of effective, attractive solutions, offered by numerous competing companies. But I think *even this difference* bears consideration.

It certainly is easier to innovate when there are simply no existing solutions, and no-one else in the field with whom to compete. "Edison had it easy*!*", you might say; "Bring him back into *these* times and see just how well his genius would serve him*!*". But of course, in today's world, he wouldn't be inventing light bulbs, and it seems likely that a man of his tenacity would be just as successful, *but as a manager*.

The realist's view of the world - with an awareness of the deeply seductive power of myth, illuminated so well by the outstanding writings of Joseph Campbell - leads one to the pallid realization that in all probability *the innovative genius of the great figures of history was not in any essential way much different from that of the lowliest of us*. Their achievements were frequently less than stellar. The notion that the era of Great Innovation and Pioneering is forever past could be enervating. Without doubt, Edison would be *a very different* figure in today's world, but we can only speculate about whether he'd achieve more, or less. Anyway, he's not here, but you are.

The Risks of Innovation

There are further ways in which our environment differs from those of the last century. Modern corporations are obliged to be fastidiously concerned about their *obligations to the investment community* and the good appearance of the financials in the quarterly report. In the hey-day of the late 19th century, this was probably not quite such a critical issue in governing business decisions. As a consequence, there may nowadays be less room for taking risks. The minimization of risk for fear of what may happen on Wall Street might signal a retreat into the safe harbor of incrementalism. In discussing this view with me Ray Stata made the following observation[97]:

I don't really believe this is true... We necessarily must take great risks as part of living in a rapidly changing high technology industry. The challenge is to arrange our portfolio of product lines and businesses so that large risks can be taken without necessarily threatening the survival of the firm. For example, in my opinion, the XL50 from the very beginning was a very large risk, but it is one that we can afford to take and lose without jeopardizing our business, thanks to our stable platform of SLIC products. In the same vein, while DSP may not have been a huge risk for the industry, it certainly was a huge risk for a company whose name is Analog Devices. We have made many mistakes in pursuing a new technology that no-one in the company really understood. But if we are successful, as I believe we will be, DSP will prove to be one of the largest and most important risks we will have taken. So I say Analog Devices does not shrink back from risk today, nor should it.

Ray mentions two risky projects: the XL50 and DSP. They share the quality of being risky, but are poles apart in many respects. The XL50 was the outcome of the sort of technical innovation that I have had in mind in writing this. In its earliest form, this novel accelerometer had only a vague strategic orientation. *It came out of* ***Why?****,* ***What If?****,* ***How About?*** *attitudes, that is, a pure R&D culture.* It was the spontaneous work of a few creative people who, without being told, went ahead on their own, recognizing from the outset that, if ever it could be pulled off, if the difficult and unproved micro-machining process could be made robust and the device brought to

[97] October 19th, 1994, after reading the first draft of this essay.

volume manufacturing, *there would be little problem finding customers*, and that it might prove to be the foundation of a whole new industry. This approach can be called *Innovative Leadership*.

On the other hand, our decision to enter the field of digital signal processing (DSP) was based firmly on *business considerations*. It was not made because we had first demonstrated some unusually innovative DSP concepts; these came later. Certainly, the decision shares some of the common properties of innovation: it came out of a *vigilant boundary watch*, and in *anticipation* of the likelihood that our domain of 'real world signal processing' was going to be threatened by the emergence of a new paradigm for the realization of signal processing.

It was, of course, the right decision, but I would have to say that the risk was moderate. In fact, the risk of our traditional analog product line being eclipsed by worthy digital alternatives was far higher*!* Indeed, we had *no choice* but to become expert in DSP. All the actual risks related to our ability to adapt, to learn new skills, think in new ways, adopt new tools and exploit new technologies. It meant managing large teams and effectively changing our image. This was *Strategic Leadership*.

Traditional technical leadership was based on trusting of a few singularly gifted individuals. It was risky, dependent on fickle and unpredictable invention, and more concerned about *"what might be",* rather than with strategy, which is all about *"what must be"*. Now that strategic leadership and the minimization of corporate risk are given greater emphasis, traditional views about spurring and managing advances in technology are in decline. *Clearly, we need both kinds of leadership: Innovative and Strategic. They are co-dependent and co-nourishing.*

We can minimize risk by keeping a very close watch on what is going on in the world; by listening to our customers, and by forging close alliances with some of them; by exerting an unremitting attention to matters of quality and service; by constructing a strong, informed and coherent strategic vision. *But we must also allow ample room for disorder, for ambiguity and flexibility*, supporting those *spontaneous, unplanned and risky new ideas* coming from a small number of isolated pockets of originality, persisting in their aims even when every customer and market expert says *Niet!*

Upsetting the Status Quo

There are yet other differences besetting the modern innoventor, compared to our predecessors. I think it's important to understand that Edison, and pioneers like him throughout history, were rarely seeking just better solutions – such as those stronger gas mantles or long-life wicks. Rather, they were committed to finding *radically different ways* to address widespread unserviced and unarticulated needs. Indeed, the word Innovation embodies the essential idea of "something new", not merely "much the same, improved".

Unavoidably, a large fraction modern microelectronic engineering will be derivative: the lower-power op-amp; the even-lower-power op-amp; the lowest-input-current op-amp; the dual, the quad op-amp; the current-mode op-amp; the faster op-amp; the op-amp with rail-to-tail output stage .... all doubtless useful and addressing real needs, but all based on essentially the same traditional topologies for feedback amplifier design dating back to the 1940's. *Innovative details* may be found in a new generation of op-amps, but we must ask: What is the problem *really* being addressed by them, and are op-amps so certainly the optimal solution? The op-amp designers of the world need to continually challenge themselves, by asking: How might this *function* be approached if the *customer's expectations* were extrapolated beyond the commonplace? What lies *beyond the op-amp?*

Should we prepare to overthrow the status quo, our imagination must be unfettered, our questions must be provocative and perhaps even absurd, following de Bono's lateral thinking. What applications can we devise for a commodity microcomputer[98] internally having a long word length and perhaps uses tens of millions of transistors, but which has just three package pins (+1.5V, COM and DATA-CONTROL-I/O) and sells for $1? What happens when you marry such a tiny, disposable microcomputer to a shopping cart, a walking cane, a table-lamp, or a library book? What might be possible, when such a μC is an everyday part of the communicators and locators woven into the everyday clothes worn by our grandchildren? When will cochlea implants become commonplace? When will an olfactory prosthesis be available? Can you make a useful transmitter powered by human tissue? How do you make a really useful neural-based computer? (The problem is the needed massive connectivity).

Some of these are imminent possibilities; others must for now remain fantasies. Still, it's always useful to spend some time thinking about the *far and ridiculous future,* which each of us is having a small, but nonetheless significant part, in creating; or in entertaining ambitious visions, while spending some time to consider the global and sociological implications of our inventions. At their weakest, such thoughts are harmless indulgences; at their finest, they can change the world for the better.

[98] You may have read of Micron's Microstamp in the trade press. It was an 80,000-transistor microcomputer in 0.6μm CMOS which included a 2.44GHz transceiver, designed without a corporate proposal by Bob Rotzoll. Only three pins were used (to make it easy to flip-bond) and the data I/O was via its loop antenna. Since then, the RF-ID tag business has made great strides.

CHAPTER 12 LEADERSHIP AND INNOVATION

Along with "Excellence", the word "Leadership" has become a meaningless leitmotif in many modern corporations; and according to some, "Management" is slowly on its way to becoming obsolete, or at least, in its traditional styles. The difference between managers and leaders, say Kouzes and Posner, is the difference between night and day. Managers honor *stability and control* through *systems and procedures*; they see *passion* and *independence* as words not fit to pass corporate lips; the objective is ***What Must Be***.

On the other hand, Leaders thrive on *dynamic change*; they exercise their "control" by *providing a worthy and inspiring vision of **What Might Be***, arrived at *jointly* with team members. They understand that empowering people by *expanding* their authority, rather than shrinking it and standardizing them, is the only course to sustained relevance and future vitality. Steve Jobs is a great example of a modern corporate leader.

In the introduction to their book[99], Kouzes and Posner refer to a survey that they conducted in 1983. It used 38 open-ended questions, such as: Who initiated the project? How were you prepared for this experience? What special techniques and strategies did you use to get other people involved in the project? What did you learn about leadership from this experience? In all, some 1,400 participants were polled. The authors reported as follows:

> The stories they told *seldom sounded like textbook management*. They were *not logical cases* of planning, organizing, staffing, directing and controlling. Instead, they were *tales of dynamic change and bold actions*. [My emphases]

This should hardly come as a surprise to anyone who has worked in the high-tech industry. We've always suspected that the prosperity of a company does not stem from planning, organizing, budgeting and reporting, necessary though these are. Yet, in many companies this same old mistake of *putting process ahead of content* keeps being made. Instead of delegating, empowering, trusting and then standing back to let great things happen, managers can't resist the temptation to keep a firm grip on the reins. That this should happen is of course entirely predictable, since they often have primary responsibility (and the biggest rewards) for a well-executed business plan, and can't afford to see it backfire. Commenting on the work of Kouzes and Posner, in the Foreword to his book *The Leadership Challenge*, Tom Peters says:

> Management is dead, at least as we know it, the[se] authors believe. I disagree. Sadly, management as we know it is not dead. But it darned well ought to be*!*

As noted strenuously throughout this essay, innovation cannot thrive in a rigidly regulated atmosphere. Striking a balance between *control* and *freedom* is difficult but essential.

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[99] *The Leadership Challenge: How to Get Extraordinary Things Done in Organizations*, by James M. Kouzes and Barry Z. Posner, Jossey-Bass Publishers, Inc. San Francisco, 1987.

For a Manager, the appearance of *unsolicited novelty* is often a cause for panic. The function of the leader is to articulate a daring vision, work to gain acceptance of that vision, and modify it through a *democratic process*, while avoiding manipulation. The leader must of course inspire and motivate his team, but not just during certain times, when, for example, the competitive threat is particularly evident, or merely at the start of a new project, or during some crisis, but consistently. On the other hand, the Leader must *keep the finale in focus*, keep the adrenaline flowing, through lean and flat times, as well as during the exciting times of felicitous discovery and rapid progress and corporate growth. The Leader must also be a *mentor*, passing on valuable experience while yet encouraging original thought, always ensuring that the project never flags. This is challenging because – unlike typical managers – *many leaders are themselves strong individual contributors*, and may find it hard to lay time aside to provide guidance to others when there is so much to do at the direct level. And, at times, even Leaders need a little encouragement and hand-holding*!*

Leaders Lead, Laggards Lean

I would like to return to the intrinsic tension arising between "leading" and "responding to" the market, which my little "History Through Dark Glasses" parody sought to illuminate. Imagine you have encountered an ad in which this slogan appears: *"National Maxilinear of Texas - Your Leading Supplier in Microelectronics, Responding to Every Need of the Modern Marketplace!"*. This is not *quite* a contradiction, but it comes pretty close, since the reference to "leading" is immediately *weakened* by the subsequent reference to "responding". Surely leadership must involve going *ahead* of the pack, stealthily and methodically seeking *new paths*, taking the *risk* that the road ahead may be littered with unseen dangers. This doesn't demand genius. It was *pioneers*, not geniuses, who fought for and claimed new territories; the *settlers* – weighed down with their gilt-framed "Home Sweet Home" pictures, the rocking chair, the solid-oak bed, the Wedgwood china and the silverware collection – cautiously followed only later.

Edison certainly took risks in connection with his own pioneering inventions, but he did not need to be a genius in order to be a strong leader. By contrast, guys like Albert Einstein or Richard Feynman are deservedly known as geniuses, but they didn't possess Edison's innoventive powers, in the sense that *they left no practical inventions*, even less innovations, as a legacy. I think few historians – or you – would describe either Einstein or Feynman as "leaders", even though they were tremendously clever, far-sighted, and an inspiration to all those with whom they worked and taught. Undoubtedly, Edison was possessed of strong conceptualizing abilities. However, it was in a seat-of-the-pants, intuitive sort of way. As we have seen, he bypassed much theory – even scorned it – and rather, he concentrated on turning his ideas into tangible things, *products for which nobody had yet expressed the slightest interest*, with unabashed confidence and every expectation of eventually demonstrating their practical value. He was much less of a thinker than a doer; which, by my use of the words, means he was less an Inventor and more of an Innovator. Tesla, on the other hand, was a brilliant all-rounder, being both: he was an

Innoventor. Unfortunately, in later life the torrent of his cerebral kT noise overwhelmed his neural determinism. Paul Brokaw and I happen to know someone in Switzerland who has fallen into same pitiable condition during the past few years. With the noise comes a fierce persecution complex.

History provides abundant lessons of champions like Edison and Tesla who forged entire new industries out of their singular vision, but whose gifts were largely unappreciated by their contemporaries. Thus, even though of obvious value today, there was *no clamor from the public* at large for Gutenberg's printing press, Bell's telephone, Daguerre's photography, Fleming's triode tube, Crooke's "cathode-rays", Armstrong's superhet receiver, and much else. We have evidence of how such major contributions go unheralded, by our being generally unclear about who invented tape recording; who discovered bipolar transistor action (no, it wasn't Shockley, Bardeen or Brattain); who labored for 20 years to perfect xerographic copying; who designed the first digital watch, the first pocket calculator, the first microwave oven; who were the visionaries behind the Compact Disc system; and countless similar examples.

Each of these innovations was the outcome of a vision, *a stubborn conviction*, often on the part of *just one person*, that some idea or another possessed intrinsic value and would *generate entirely new markets*, rather than merely serve the here-and-now, door-knocking measurable market. Furthermore, we should not overlook the fact that behind each of these innovations were *the disappointments of countless dead-ends*. The old jingle "If at first you don't succeed, try, try, try again" sounds naïve and even juvenile; but this is a mantra that every innovator knows to be at the heart of progress. I advise my team to expect to go down a dozen or more dead-ends in the course of attempting to implement every new approach to some function.

By 1945, the domain of electrical devices was already quite mature, but *electronics* was still a new word, the science fiction writer Arthur C. Clarke envisaged a totally new way of deploying electronic technologies – a *global network of satellites* in geosynchronous orbits. The first publication of the idea was in *Wireless World* magazine[100].

[100] *Extraterrestrial Relays*, in Wireless World, October 1945. Incidentally, this was the same journal that I started to buy at news-stands for two shillings an issue, in 1947. It was far more professional than the only other two magazines of the time, *Practical Wireless* and *Radio Experimenter*. Unlike them, it was not a "How To" vehicle, but a forward-looking presentation of major events in the world of radio and television technology. (It did, however, present propagation forecasts for radio hams). One column used to especially intrigue me: ostensibly authored by one person under the pseudonym "Cathode Ray" (but I suspect now this was many contributors in rotation) it took a simple topic – the sort of thing that seemed too simple to be worthy of any further discussion – and turned it, and the reader, on its head for long enough to provide a totally new perspective. This is a didactic *modus operandi* that nowadays I particularly favor myself.

When he made these suggestions, few would have imagined them to be practical, and fewer could have foreseen the *crucial importance of communications satellites* to every aspect of staying in touch in late-twentieth-century life on Earth. It was the sort of major initiative that you'd think some business tycoon, with a reasonable knowledge of radio-technique, might have proposed, teamed and funded. *But it took an imaginative Sci-Fi writer to do it.* This was two years before the first demonstration of the bipolar transistor by *Becker and Shrive*, lowly engineers at the Bell Labs, where those other better-known fellows also worked. This couple applied the ***What If?*** paradigm to a sliver of germanium which was expected to operate as a JFET, and they discovered a whole new kind of transistor, whose existence had, however, been previously predicted by the advanced semiconductor theory of William Shockley.

CHAPTER 13 DETERMINATION AND TENACITY

Some ideas are totally crazy. No, really. I've met my fair share of "mad-inventors", some of whom believe (seriously*!*) that perpetual motion is within grasp. Some have spent a lifetime in pursuit of the impossible. But this is an unreliable stereotype; many mad-clever ideas were probably brushed aside because of the inadequacies of the technology of the time. *Galileo and da Vinci provide excellent examples of visionaries whose ideas outran the technical means for realization in their lifetime*. It was noted earlier that Edison's objective was "to innovate devices that could satisfy real needs and thereby come into widespread popular use". Necessarily this was based on a *strong sense of what those needs were -* ***or would be!*** This constant appeal to the *forward vision* is the essence of leadership. It is surely a simple matter of definition that leadership requires *leading*, not following; *anticipating*, not merely responding.

Two examples of anticipatory, leadership-inspired innovations, for which absolutely no market existed, are perhaps worth quoting here. The first relates to the invention of the **laser**[101]**,** reported in the October 1993 issue of *Physics Today* in an article by Nicolaas Bloembergen, who first reminds us of the ubiquity of the laser:

> The widespread commercial applications of lasers include their use in fiber optic communication systems, surgery and medicine, printing, bar-code readers, recording and playback of compact discs, surveying and alignment instruments, and many techniques for processing materials. Laser processing runs the gamut from sculpting corneas by means of excimer laser pulses, to the heat treatment, drilling, cutting and welding of heavy metal in the automotive and shipbuilding industries by CO_2 lasers with continuous-wave outputs exceeding 10kW..... Lasers have revolutionized spectroscopy, and have given birth to the new field of nonlinear optics. They are used extensively in many scientific disciplines, including chemistry, biology, astrophysics, geophysics and environmental sciences...

It is unlikely that all of these applications – these "devices that satisfy real needs" – were foreseen by the inventors. So, what *did* motivate them? Bloembergen goes on:

> [The] physicists who did the early work were... intrigued by *basic questions* of the interaction of molecules and magnetic spins with microwave and millimeter-wave radiation. Could atoms or molecules be used to generate such radiation, they asked themselves, and would this lead to better spectroscopic resolution? [My emphasis]

Thus, the motivation in the case of the laser seems to have arisen simply from the more limited desire to find a way to advance an existing technique (spectroscopy) by providing one specific improvement (higher resolution). That might sound like incrementalism. Still, while we can't be sure, it's doubtful that the laser was *the result of a survey* of numerous other physicists as to what *they* perceived would be useful. Rather it appears to have been a *spontaneous invention* by "back-room" physicists *who knew what would be of value to fellow physicists, based on their own experience.*

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[101] An article entitled *"The Shock of the Not Quite New"* in The Economist of June 18th, 1994 noted that "lawyers at Bell Labs were initially unwilling to even apply for a patent of their invention, believing it had no possible relevance to the telephone industry". This brief article is well worth reading. It includes several other illustrated examples of innovations which went unrecognized until much later, including the steam engine, the telephone, radio, and the computer.

Cannot we do the same sort of thing? Are we not today aware of small advances that, even though not yet expressed by our customers, are nevertheless expected to be of value? Certainly, the development of new integrated circuits ahead of market demand cannot be compared to such a monumental leap as the invention of laser. Still, there is no reason why the same *spirit of leadership* cannot be present even in this humble endeavor. Furthermore, the essential idea of *innovating out of a broad knowledge of the possibilities and utilities of one's technologies* applies equally well in both cases.

My second example of another 'big' idea is the invention of **magnetic resonance imaging (MRI),** whose full potential in medicine is only just beginning to be realized/ Indeed, it is thought by some that MRI will soon surpass X-rays in importance in diagnosis. MRI came out of nuclear magnetic resonance (NMR) techniques which were originally developed to investigate nuclear properties. In *Science* (10 December 1993), George Pake is quoted as having this to say about the sources of the ideas:

> Magnetic resonance imaging could arise only out of the *nondirected* research, not focused on ultimate applications, that gave rise to what we know today as NMR. The key was a series of *basic quests* to understand the magnetic moments of nuclear spins; to understand how these nuclear magnets interact in liquids, crystals and molecules and to elucidate the structure of molecules of chemical interest. Out of these *basic quests* came the knowledge that enabled a vision of an imaging technique. Without the *basic research*, magnetic resonance imaging was unimaginable. [My emphases]

I am not suggesting, of course, that our primary mission as individual designers is to conduct *fundamental research*, or that a manufacturing corporation like Analog Devices should undertake such. But even in our industry, we cannot allow the quest for *ground-level advances* in our competitiveness to be ignored. This requires that we constantly reflect on the utility of this or that new technology or product concept; it says that we must routinely – and almost instinctively – devise original circuit functions and topologies; that we must exploit both conventional and advanced silicon processes in unconventional ways; that we develop time-saving testing techniques, *before* their need has been articulated by the project engineers. In short, we must consciously and relentlessly search for *novel ways of exploiting our technologies* to produce "devices that satisfy real needs".

In a modern corporation such as Analog Devices, the pressures to meet even the *known market demands and deadlines* are unremitting. They seem to constantly consume all available resources. Nevertheless, *some of our time must be spent in 'nondirected research'* if we are always striving toward leadership. Most of us will admit that we spend a significant fraction of our time at our workplace[102] thinking about all kinds of topics that are *not immediately relevant* to the projects for which we have immediate responsibility, and it's only because these efforts cannot be related to 'approved and funded' projects that they fail to show up on reports of resource utilization.

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[102] As well as at home; but that's our affair. For the ardent innovator, the workplace is everyplace.

Technology Development

In every microelectronics company, innovention is essential across a broad front, notably in the relentless development of IC products, but also in *advanced fabrication processes*. At Analog Devices, the early development of *bonded wafer* techniques to manufacture dielectrically isolated substrates is a good example from recent history. This was a step taken *independently and proactively* by key people in the process development team. To the best of my knowledge, it was not predicated on some "Voice of the Customer" exercise (the design community being the customers, in this case). I recall that Jody Lapham had to make his own bonded wafers in the early days, which among other things required him to turn his *practical prowess* to the development of equipment to image the bonded layer in search of voids[103].

It was doubtless the conviction that bonded wafers represented a break-through, promising *a step-function advance in IC technology*, that spurred this development. None of we 'customers' had specifically clamored for bonded wafers *per se*; but our technical need was for much lower collector capacitances and freedom from latch-up effects. The eventual result of this *anticipatory research* was an outstanding new IC technology (XFCB) which unquestionably enjoys a world-class leadership position. More recently, we have acquired a second-generation XFCB process, with higher f_Ts and smaller geometries, on which already several new products have been proven, working essentially perfectly at first silicon – which reflects innovention in another group of people, those providing the crucial *device modeling equations and characterization data*. Without such, simulation studies are practically a waste of time.

These new processes also provide an innovative capacitor technology and retain the *thin-film silicon-chromium resistors* that have been a distinctly innovative aspect of Analog's approach to IC design and fabrication for more than twenty years. The perfection of these ultra-thin resistors (at 25 Angstroms, their thickness is only one-two-hundredth the wavelength of yellow light, and corresponds to just a few atomic layers) was another hard-won battle, undertaken because of the *dogged conviction* on the part of a handful of believers that the benefits were worth fighting for.

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[103] Few not immediately engaged in technology development think much about the myriad sub-tasks that must be undertaken in reaching some new high point in one's overall complex of capabilities. For example, we might have to design complete pieces of test equipment, because nothing is available on the market to perform some function. About 25% of my time used to be spent in this mode in earlier days. Other frustrating time-sinks creep unavoidably into one's schedule. When accounting for time spent on each project, how is one to categorize the dozen or so avenues which must be explored before the right one is finally found? As stated elsewhere, the idea that technology development proceeds in a sequence of linear, progressive steps is almost always an illusion. A successful corporate structure must fully acknowledge that.

Sometimes, innovation involves the bringing together of many loosely-related processes into a more potent whole, such as *laser-trimming of thin-film resistors at the wafer level*[104]. This required an innovative synergy, combining significantly different circuit-design approaches, specialized layout techniques and novel test methods (involving the use of clever on-line measurement techniques to decide what to trim, and by how much). In this arena, too, much innovention was needed, as might be expected.

Further, special attention was needed as to the precise formulation of the *resistor composition*, in order to achieve the crucially-important low temperature coefficient of resistance (TCR), which is only a few parts per million per degree Celsius; the *control of the laser power pulse duration* and *spot size* to minimize the post-trim drift of this TCR, and thus maintain *very accurate matching* of networks over temperature. It was also necessary to understand the importance of controlling the *oxide thickness* to prevent phase-cancellation by reflected laser energy. With the help of Paddy Quinlan, a nice old professor at the University of Cork in Ireland, *new mathematical tools*, needed to explore potential distributions in arbitrarily-bounded regions, were developed; potent *synchronous demodulation techniques* were developed to trim analog multipliers; and so on. These, and many other advances by numerous individual contributors, coalesced to bring ***the science of laser-wafer trimming*** to a high art.

Those Voices Again

In view of all the foregoing, should we view the modern call to "Listen to the Voice of the Customer" as a *significant departure* from the ways through which we found success in the 1970s? Or, is VOC *merely a reformulation* of what we've been doing all along, but now with the benefit of Harvard's Seal of Approval? Obviously, listening to the customer makes eminently good sense. Faced with the need to respond to an emerging market requirement about which we may know little, it is valuable to solicit would-be customers about their specific needs. But surely, if, from Day One, we had been practicing good *gate-keeping skills*, thereby accumulating a large body of *relevant knowledge about our industry*, the criticality of the customer interview would be substantially lessened. Furthermore, we could then address our customers as *equal partners,* with advice to offer proactively, and *with solutions already at hand*.

The "formal" VOC technique begins with an essentially neutral, objective interview, using two representatives, one of whom fields a series of previously-formulated questions (often invariant from customer to customer) while the other takes notes as the customer's team freely responds. The value of the VOC process is presumed to lie in its efficacy in extracting key nuggets of marketing intelligence, free of bias or suggestion. However, *this is illusory*, because many customers are quite *unable to imagine* a better way of solving their system problems. They may be

[104] Dan Sheingold reminded me of this, in reviewing an early draft of this essay, and suggested LWT as an example of what might be called collaborative innovation.

doggedly fixated on what they *firmly believe to be their need* (stronger gas mantles) and completely fail to appreciate that there may be a better *forward-looking* alternative (electricity).

Furthermore, customers will often decide to withhold critical information, for several reasons. They may have simply become tired of spending ("wasting") their time with an endless stream of marketing people signing in at their lobbies. Sometimes, it will be because the individuals being interviewed had been told by their supervisor not to reveal the intimate details of some project. At other times, they may have already made up their mind that National Maxilinear of Texas, Inc. is going to be the vendor, because of *all the good ideas* which that company (proactively) presented, and because of the *leadership image* they projected, at their last on-site seminar.

As effective market researchers (whether that is one's title or just the way we think about our customer interactions) we need to exercise considerable psychology, going well beyond the merely technical and mercantile issues. We have to recognize that our customer is, above all, an emotional unit. Further, we need to keep our thoughts about how best to conduct a VOC interview constantly under review, recognizing that it is *merely another tool*, not a formula for success, and like all tools it is easily misused. We each have developed certain optimal ways of getting to the bottom of some matter, and we each pursue *the process of discovery* in our individually-appropriate way, following an individually-appropriate sequence.

During a customer interview, copious note-taking is useful; I've often had to chastise myself for failing to jot down some detail that I recall was indeed discussed, because *it seemed so painfully obvious at the time*. We need to strike a balance between the distraction of generating a record of the proceedings to accurately report our findings to others, against the desire and opportunity to *probe more deeply* into often difficult and sensitive areas, and *personally learn* something of *fundamental value* from the interview.

As is always true, if we adopt a simplistic and slavish adherence to *a single set of rules* while failing to see the *principles* they encapsulate, we will make miserable progress. Here's Ray Stata again:

> The essence of VOC is listening carefully to the customer and abstracting from his commentary insights about latent requirements that are not immediately evident, drawing on our background of understanding to stimulate creative interpretations of what's being said.[105]

That is a sentiment we can all agree to. *My criticisms of VOC are not intended as a rallying-cry to abandon customer interviews.* I am only saying that we must conduct them in a manner that works best for us individually. They are one of *numerous gatekeeping activities* with which all key contributors in an innovation-based company – not just those formally designated as 'market strategists' – must be involved. Clearly, it would be presumptuous and foolhardy to imagine that we can lead out of our own superior knowledge in every situation. Nevertheless, *if we place*

[105] Ibid.

an excessive dependence on the customers' inputs to fuel our innovation, we have little or no advantage over our competitors, who can just as easily work though the same VOC procedures and presumably arrive at an equally "potent assessment" of a particular opportunity.

Furthermore, rigid adherence to the "blank page" approach could lead our customers to believe that *we presently know little or nothing about their requirements*, and that consequently, we are unlikely to be in any position to offer novel or cost-effective solutions. Surely, **the VOC process deserves a broader interpretation**. At its best, it can be a valuable tool in *responding* to the *short-term* needs of our customers. However, even in the most optimal scenario, *it will inevitably lag the market* and will probably miss many of the deeper *long-term needs*. Though very important*, it should never be relied on as the primary path to leadership*.

As innoventors, this requires our *total independence competence* in our field.

A Control-Theory Analogy

We might liken innovention to a typical control system.

The objective is to cause a certain desired end-state in the response (the output) to some demanded state (the input). In designing such a system, we can utilize either *feedback*, or a predictive (anticipatory) *feed-forward* approach to reach the end-state.

VOC is essentially like the feedback system. Its basic objective is to minimize the error between the set point – the customer's specifications for a new product – and the final state of output – the product(s) we eventually generate in response to this demand. Signals at various points along the path (products under development, new concepts in our portfolio and the like), and the nature of the path (business and technical strategies) also determine the state of the control system, which, ideally, would involve *continuously going back to sense and update* the customer's most recent needs.

The output of this system also has some *noise* (that is, spurious or incorrect technical knowledge, fragmentary awareness of a possible competitor's plans, resource collisions, the uncertainty of local, national or even global economies, and so on), requiring that decisions about optimal actions are always based on imprecise or corrupted information (*fuzzy data*, to use a popular phrase). In fact, beyond such vagaries, this 'market-responding' system has *a great deal of noise* in it – meaning an elevated dependence on *filtering* (= human, unreliable, non-deterministic judgment) in dealing with its indications.

The seductive promise of feedback systems is that they can achieve *zero error* between their 'set-point' and the output. Translated: we can exactly meet the customer's precisely-articulated needs. However, it is well known that the *inertia that is inherent in any control system*, mainly due to the presence of *delay elements* in the loop, can lead to long settling times, or even – for a nonlinear system (and what is the design process if not *that?*) – no stable solution at all. Also, feedback systems are less successful in coping with inputs (= market demands) that are rapidly changing, due to this very *inertia*. Sometimes, when a sudden large change is needed, a development program will exhibit something akin to slew-rate limitations: that is, there's a long ramp-up time before linear operation ensues again, during which the whole process is held up. An example of this might be when a new package style may "suddenly" and "unexpectedly" be needed.

I suggest that *the leadership approach to innovention* must instead be modeled along the lines of *an open-loop system*. Such systems can be made *extremely fast and effective*, at some expense in final accuracy, perhaps, which is now bounded not only by the quality of the input data (in this scenario, based on a trust of one's key technologists, and their broad, rather than specific, *knowledge* of the market) but also by the accuracy of the implementing system (*knowledge* about how to optimally achieve the final state, that is, *practical engineering knowledge*). Noise (= uncertainty in the outcome) is still there, but it now emerges out of an appeal to data that is gathered over *an extended period of time*. including: fundamental physical limitations of devices and technologies; durable principles of design; long familiarity with a wide variety of customer needs; well-established public standards which will lead a large number of different customers to raise similar demands; etc. Due to the long averaging time, and the absence of an *"input comparator"* the noise is filtered more completely before it enters the system, causing smaller errors.

If we are to pursue leadership, we may, on occasions, need to abandon our reliance on low-bandwidth, possibly oscillatory *closed-loop market-responding* approaches to product genesis, in favor of a *direct, feedforward response* based on comprehensive, mature, personal and sure-footed *knowledge of one's potential markets*, and on the use of technologies and design skills which can be quickly re-deployed to meet emergent market demands *in an anticipatory – rather than a reactionary – manner*. Open-loop (so-called *predictive*) systems are known for their inherent stability and for their efficacy in tracking rapidly-changing inputs[106]. In our analogy, this might mean that we're at the ready – with, say, that "special package" or test routines – long before the product is nearing release, because *it's need was anticipated*, knowing about our customers' systems and their needs. In another scenario, it may mean that *we're ready with the design of our next product* at about the same time that the current one is being released.

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[106] It is rather a pity that the expression *"Yeah, he's inclined to run open-loop!"* has taken on a pejorative connotation.

So how soon *should* one introduce follow-up products, in an 'open-loop' fashion (before the demand is obvious) so as to stay on the competitive curve, knowing that these will inevitably cause some erosion in the revenues generated by our earlier products? Clear opportunities for follow-up action may be neglected, because of the (justifiable) concern that some product "released only last quarter" might be obsoleted too soon. Unfortunately, *in pursuing leadership in innovation, one doesn't have much choice: we must often make those decisions without waiting for the feedback from the customer.* They should be based on a sound *knowledge* of the often quicksilver trends and in thoughtful anticipation of future market needs. Rather than waiting for that coveted million-piece order to be delivered on your doorstep before undertaking the development of some new product, you ***just do it !***

Clearly, a judicious balance of both approaches (leading and following, anticipating and responding) is needed. My concern here is that many current philosophies and management styles, intent on improving the success rate of new products over the short term and minimizing investment risk, point *away* from the traditional emphasis on *leadership-based innovention* and they drastically dilute the freedoms enjoyed by the recognized, and still-active, product pioneers and champions of earlier times.

Are those times still relevant? That's not clear. Nevertheless, it seems to me that it is generally preferable to do business based on long-term *hard-won internal strengths* than to depend too much on going 'out there' on expensive, time-consuming marketing forays, to pry out of the often ill-informed customer all the critical information needed to face challenging business decisions. There is no denying the importance of listening to as many customers as time (and the travel budget) allows; now and then, one may pick up invaluable knowledge about some new trend or direction. New systems are often extremely arcane and we rarely have all the knowledge we need. But it's slim pickings. *Leadership based on a kind of unique wisdom, and on mastery of one's domain, gained by "filtering with a long averaging time" is clearly preferable*.

Product-Selection Methods

It would be singularly unwise to suppose that all of our future concepts will come either out of team-focused VOC exercises or from tolerance of the singular vision. For standard products, much of the challenge is to constantly be on the watch for competitive threats, and to have *an arsenal of next-generation solutions* always on hand. These products need to incorporate a high level of *'predictive innovention,'* motivated by a keen awareness of what the customer will probably need in *two to five years from now*, and of the emerging technologies that will become available in one's own factory, and also, of course, in our competitors' factories. This requires a strong instinct for future product *value and enduring utility*.

In my control theory analogy, I suggested that the development of standard products was more likely to benefit from the 'feed-forward' approach. It will be the **Voices-of-Many-Customers** that are here important, and the **Voices-of-Many-Competitors**, as indirectly articulated in their ads, their data sheets and their application notes. On the other hand, *special-purpose ICs* – the ASICs and USICs, the systems-oriented products – are more likely to benefit by careful listening to the voice of the customer, sometimes just one key customer, maybe two or three, as well as to the **Voices of Committees** (those writing standards and codes, recommending certain practices), and the **Voices of Consultants** (people hired to advise us about completely new and totally unfamiliar fields) and finally, because of the invariably-low margins of most high-volume product, it is almost a matter of definition that one needs to listen most carefully to the **Voice of Caution** before launching into any new development.

The basic challenge in this case is *first, to learn* about some unfamiliar new function, or set of functions, or a whole new system, well enough to understand the customer's terminology; *second, to learn at a much deeper level*, so that one can confidently offer an innovative solution, out of an independently-strong sense of what is needed, and share this learning with the rest of the team; *third, to strive toward a higher level of system integration*, if such is judged to be of value to the customer, or is seen as a competitive advantage; *fourth, to achieve the lowest solution-cost*, through careful choice of technology and exactly-adequate design (since one is competing with existing well-known costs and/or other bidders); *fifth, to get to product release fast*, on a very visible schedule; and *sixth, to ramp up quickly to very high delivery volumes*. In the development of special-purpose ICs, the customer is, of course, the primary reference, the 'set-point' in what must be the innovention feedback system.

At times, after weighing up an opportunity to bid on some RFQ, we find ourselves saying: "Why on earth are we doing this? Why are we committing to a very severe schedule, to a plan that requires manufacturing to suddenly produce 250,000 – or a million – pieces a month of some IC that we don't even know how to design yet, let alone test? Why are we agreeing to sell this little marvel for a couple of bucks when we know the margins on high-performance SLICs are so much higher?"

The answers to these sobering questions aren't easy, but boil down to one unavoidable fact: *We are part of a maturing, highly-competitive industry, in which these challenges are becoming the norms. Like it or not, leadership is at stake on every front.*

CHAPTER 14 TQM, DFM, TCM AND OTHER TLAs

Product quality is always important, but it's especially so in modern microelectronics, as competitive pressures mount. Thus, development time and cost to market are likewise under pressure. Expectations for today's commercial-grade parts now often exceed those required only by *the severest military and space applications a few years ago*, but at far lower cost. During the past few years, bookstores have been flooded with best-sellers crowing about the importance of 'Excellence' in modern corporations, and how to foster a culture in which excellence is second nature. It's one of those words that are seized upon by eager journalists who believe they can put yet another spin on a popular theme. *But excellence alone is not enough to ensure success*:

> Excellence... will give [companies] a competitive edge only until the end of the decade. After that, it becomes a necessary price of entry. If you do not have the components of excellence … you don't even get to play the game.

So says Joel Arthur Barker[107]. In 1995, we talked less of excellence and more about quality. *But quality for quality's sake also doesn't make much sense*. It wouldn't help to have impeccable design, perfect mask-making, 100% yields, zero delivered ppm's and infinite MTBF's unless the products to which these glowing attributes apply have *relevance* in the contemporary marketplace, and are introduced in *a timely* fashion, and at *the right price*. These are the essential prerequisites of this business. Sure, the need for a strong focus on quality is self-evident, widely appreciated, and has received a great deal of attention in recent times. This emphasis, commonly referred to as **Total Quality Management (TQM)** is obviously of fundamental importance and must be relentlessly pursued. One of the many sub-goals of TQM (all of which have three-letter acronyms that are very effective in numbing the mind to the importance of their underlying concepts) is **Design for Manufacturability (DFM).**

However, corporations are discovering that highly-experienced designers do not respond favorably to the *formalism* of TQM. This is probably because we feel slightly insulted that anybody would assume we are prone to overlook the obvious necessity for such things as, say, design for manufacture. We feel we've been quietly applying our best efforts to achieve design robustness for years[108]. We are bemused by the apparent 'discovery of quality' as a new idea. *The notion that one can legislate quality* by the institution of formal procedures, such as check lists of potential mistakes and omissions, is somewhat naive.

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[107] Joel Arthur Barker, *"Paradigms: The Business of Discovering the Future"*, 1994. I highly recommend this book, which it appears was previously published in 1992 under the title *"Future Edge"*. I guess by that time anything with the word 'Future' in its title was becoming passé—so perhaps it enjoyed only lackluster sales; by contrast, 'Paradigms' is a very marketable word today (1994).

[108] And, it might be added, we've been expected to carry out something illustriously called three-sigma design with *very limited* statistical data about our technologies. This shows some signs of getting better.

Many of these rituals, observed with something approaching religious intensity, seem overly-regimented and even ludicrous. It is this '*institutionalizing the obvious*', and the evangelical emphasis of *method* with a diminished regard for *content*, that many of us initially found to be irritating and counterproductive in earlier embodiments of rigid TQM methodology.

There are signs that the erstwhile zeal is gradually diminishing, to be replaced by a more liberal and moderate interpretation of the principles. On this, Ray Stata says:

> We have made a huge mistake in the way we introduced TQM to the engineering community and have unnecessarily put people off, for the reasons you mention. The best way to think about TQM is as a system to accelerate learning at all levels and in all functions. The methods are merely tools to be used when and where appropriate.[109]

There's also the old-fashioned matter of *pride of workmanship* at stake here. Skilled designers believe they have an innate sense of what is manufacturable and what is not, and they exercise constant vigilance over the whole process of finding an optimal solution *with manufacturability, robustness and quality fully in mind*. It is futile to attempt to mechanize the design process, if this means applying a succession of *bounds* on what methods can and cannot, should and should not, be used. *Enabling* design, for example by providing powerful synthesis tools, is quite a different matter.

Fragile innovative concepts cannot thrive in a critical, limiting atmosphere. For these ideas to thrive and grow, *trust is essential*. Design quality is never the result of completing check lists. It is even conceivable that by instituting a formal mechanism for 'checking designs' one could impair the essential need for personal vigilance, replacing it with the absurd expectation that mistakes will assuredly be trapped by the checking procedures.

Clearly, there is no disagreement with the notion that *design checking* is important. It can catch errors which might easily be overlooked and thereby allow designers to *benefit from hard-won experience and the prior occurrence of fatal errors and known bugs*. But this monitoring function needs to be integrated into one's workplace, and be active, in a background mode, continuously, throughout the entire development period, not at the last minute.

In the long run, design assurance will be more automated, using our workstations and company-specific 'experience data bases'. For now, this will perforce have to be limited to what can be done with present-day computers, which already provide easy (if not always instant) access to massive amounts of knowledge about our business. To fully support all the **TQM**, **DFM** and **TCM** ('Time and Cost to Market') initiatives, we'll need to build powerful automated monitoring agents to catch design anomalies.

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[109] Ibid.

These could be anything from rudimentary pieces of code to reduce a keyboard-intensive task to a single keystroke, right up to major software and network developments and the pioneering use of *machine-initiated tele-connections* of all sorts to link together geographically disparate groups. In some future world, the quiet time that our machines have when we go home at night will be used to review our day's work: in the morning, there'll be a *private report* of our oversights, indiscretions and omissions awaiting us. It has been most aptly said that "to err is human", but it is also true that *to err is humiliating*, and particularly so in public. On the other hand, having one's errors pointed out in private can be enriching. And errors are not entirely negative entities. This is reflected in Ray Stata's words:

> ... a defect is a treasure in the sense that lessons can be learned from our mistakes. One of my goals is to change our culture from 'Who's to blame?' to 'What's to blame?', to emphasize that many of the correctable mistakes in the Company have more to do with the processes and the ways in which people interact than they have to do with the capabilities and good intentions of people.[110]

Such a position is absolutely admirable. Unfortunately, it remains true that individuals, rather than teams or machines, make mistakes, and we *individually* need help. Perhaps some day, machine-based counseling will be available. I'm not thinking here about the use (or abuse) of reincarnations of such programs as Weizenbaum's *'Eliza'*, to secretly provide employees with on-line therapy to help them cope with their domestic trials. Rather, I am proposing – in all seriousness – that we can reasonably expect *our workstations to help us* to be more effective in coping with the myriad details of modern design and the complexity of modern projects, and yes, even in being more effective in helping us to catch our mistakes. It's largely a question of degree; many design-oriented programs already have extensive warnings about very simple things, like unconnected pin-symbols in schematic capture, or incorrect syntax in a wide variety programs.

If such vigilance were exhibited by a human working alongside us, we would probably (a), find it extremely irritating after a while, in spite of the well-meaning way in which the advice was offered; and (b), we would be embarrassed that our foolishness or ineptitude was becoming publicly obvious, particularly if our helper were a junior hand. In fact, I find *the idea of being advised by a machine*, in the privacy of my own personal cyberspace, quite palatable. We are all of limited intellect and all have very limited time. We all make mistakes on occasions. Getting our computer to take on a larger load of keeping watch over us seems quite acceptable. These machine-issued reprimands might include an occasional warning for having overlooked an important design practice, or ignoring a manufacturing standard, or for taking some needlessly risky step, or for committing some clear violation of a basic principle.

Though the full realization of this pipe dream (some, no doubt, would call it a raving nightmare) is far from being practicable today, there are nevertheless many 'intelligent' ways in

[110] Ibid.

which *continuous, automated oversight can be built into design tools*. In circuit simulators, simple 'warning lights', that advise of improper circuit conditions or modes, are already possible; other desirable features will require substantial advances in computer operating systems before they can be realized. *Improvements of this sort will automatically ensure and enhance product quality.*

One that I particularly feel to be both tractable and of great value would be a simulation overlay for automatically investigating the susceptibility of a monolithic circuit to electrostatic discharge (ESD). This is almost *a 'pure quality'* issue, inasmuch as it has little to do with the *core design* of a product. A simple procedure could be written which would automatically subject every combination of pins to the prescribed 'jolt energy', and then issue a damage report[111].

To the extent that TQM requires a systematic approach to the process of learning at all levels and in all circumstances, and thereby improving product timeliness and quality, the development of such *'aids for the poor mortal'* is manifestly a sensible initiative. But it should never be construed as anything more than *another tool*, in this case, one for revealing critical areas in need of a certain kind of attention. But again, *perfect quality in poorly-conceived products will never make a winning combination.*

The latest theme, by the way, is the 'Process-Centered Organization', a term coined by "one of the world's leading business thinkers", Michael Hammer[112], who also co-authored 'Re-engineering the Corporation'. It appears he has only just discovered that what really counts in an organization is having everybody understand that they are part of a larger process, rather than unit functions, and that by being more clearly aware of the way in which they can influence parts of the overall processes both up-stream and down-stream, they are more likely to be effective contributors to the welfare of the corporation. That is, considerable autonomy must be afforded the individual in a healthy organization.

I suppose there are some companies in the world that don't know this yet, but I'd be astonished to learn that this was a rare insight.

111 Since writing the original essay, we now have this capability.
112 His latest work, called *Beyond Re-engineering*, is published by Harper Business Books, 1996.