CHI@20: Fighting Our Way from Marginality to Power

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ABSTRACT
The Special Interest Group on Computer Human Interaction (SIGCHI) has had a successful history of 20 years of growth in its numbers and influence. To help guide the continued evolution of the academic discipline and professional community, we invite several senior members to offer their visions for what the field of CHI actually accomplished over the past several decades, and what do we still need to accomplish? What do we need to do differently/ better/smarter? What haven't we tried because the technology, the money or the will wasn't there in the past, but perhaps is now?

The CHI field is more than just technology. We understand that our work can have a profound effect on individuals, families, neighborhoods, corporations, and countries. We know that we can influence education, commerce, healthcare, and government. How can we contribute to bridging the digital divides in developed and developing countries? What agendas can we offer for the academic, research, industrial, and civic spheres for the next 20 years? How can we be more ambitious? How can we truly serve human needs?

Keywords
Human-Computer Interaction, usability engineering, theory, practice, professional, education, future vision

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POSITION STATEMENTS

BEN SHNEIDERMAN: ASPIRATIONS FOR GENERATIVE THEORIES

SIGCHI and the HCI community have been a productive cauldron of innovation for information and communications technologies during the past twenty years. Our products have had widespread impact including graphical user interfaces, the World-Wide Web, online communities, instant messaging, collaboration tools, information visualization, and multimedia. We should proudly celebrate our successes and energetically tell the story of our contributions beyond our community.

For the next twenty years we need to grow the community of industrial practitioners, often known as usability engineers, and make them more effective participants in commercial development. We will do this by raising their skills and embedding design knowledge in advanced software tools that yield rapid and high quality products. We will integrate usability engineering into effective business models and reliable design processes.

In parallel, we need to expand the science of human-computer interaction and deepen the foundations of our academic discipline. We must recognize that nothing is so practical as a good theory and that theory thrives when challenged by practice. Our goals should include development of predictive, explanatory, and generative theories that systematically support the next generation of innovations. Predictive theories give quantitative estimates of human performance with input devices (e.g. Fitts' Law), menu traversal, or visual scanning. Explanatory models sharpen our understanding of successful products and can guide future designs, such as Don Norman's seven stages, Clark's common ground, or my direct manipulation.

Generative theories will open up new knowledge domains and guide us to innovative applications. Just as quantum theory in physics led to photocells, transistors, and lasers, I predict that a deeper understanding of how to support human relationships will lead to an outpouring of innovation.

Remarkable successes will come if we raise our goals to promote trust, support creativity, and amplify motivation. We need to go beyond collaboration to address conflict resolution, further than usability to embrace universal usability, and above utility to engage passion. By addressing the central concerns of our time we will add substantial value to commercial developments and gain widespread respect in scientific communities. We will then be recognized and valued as the cauldron of innovation.

MITCHELL WALDROP: LICKLIDER'S TRANSCENDENT VISION AND HIS UNFINISHED BUSINESS

Historically speaking--and here I think my talk will serve as a counterpoint to Don Norman's--there was at least one instance in which the CHI sensibility was responsible for enormous creative ferment. This is the story of psychologist and human factors pioneer J.C.R. Licklider and his greatest creation: the ARPA community.

"Lick," as he was known, is the main character in my new book, *The Dream Machine: J.C.R. Licklider and the Revolution That Made Computing Personal*. In my talk I will sketch his story in some detail. But for now, suffice it to say that in 1962, when the DoD's Advanced Research Projects Agency (ARPA) hired him to organize a new research program on command and control, he arrived at the Pentagon with a transcendent vision of how to proceed. He believed that humans and computers were destined to form a "symbiosis," each preeminent in its own sphere—rote algorithms for computers, creative heuristics for humans--but together far more powerful than either could be separately. And in the effort to implement that vision, he and his hand-picked successors at ARPA forged a community of researchers that included MIT's Project MAC, home of the first on-line community; Douglas Engelbart's team at SRI, which pioneered the mouse, on-screen windows, hypertext, and much of the rest of the modern user interface; the nationwide effort to build the Arpanet, which was the forerunner of the Internet; and ultimately the young Turks at Xerox PARC, who by the mid-1970s had turned the ARPA vision into the modern desktop environment: stand-alone, bit-mapped personal computer, ethernet, laser printer, windows-icon-mouse GUI, and all the rest. Indeed, the ARPA community originated most of the modern computing itself.

As always, however, there is unfinished business. I will mention two items drawn from Lick's own concerns.

1) Lick eventually came to see programs as a new form of external cognition--analogous to writing, mathematical notation, diagrams, graphs, and the like, but with one critical difference: programs weren't just static symbols on a page. They were dynamic. They could execute. They were like equations that could solve themselves. After he left ARPA in 1964, he accordingly spent most of his research time in a largely fruitless quest for a "dynamical modeling" language: some form of graphical, on-screen notation that make the construction of computer models as easy and as intuitive as drawing a sketch. Today, for all our progress in computer simulation, scientific visualization, and the like, we aren't much closer. (Alan Kay, who tried to achieve it with his invention of object-oriented programming, calls such intuitive software development the "great undropped shoe" of computing.)

2) Likewise, for all our progress in search engines, data-mining, information visualization, et cetera, computers still don't give their users much help in getting all that data past what Lick called "the desk-brain barrier." That is, collecting a large stack of material is comparatively easy. But assimilating it--figuring out the significance of this fact or that fact, recognizing relationships, putting information into a larger context, understanding what's going ON—is hard. "Sense-making," as it's sometimes called, is what detectives do, not to mention scientists, journalists, intelligence analysts, and lawyers researching a case. It's pervasive in every form of what we're pleased to call knowledge work. And yet, for the most part, the tools aren't there.

DON NORMAN: WHY HCI IS STILL A SECOND-CLASS CITIZEN

CHI fails because it is too narrow in focus. CHI fails because its practitioners are badly trained by the universities, by professors who do not understand the product cycle of industry -- and often, who are scornful of what they do know.

When companies hit hard times, who do they lay off first? The HCI crew. My email is overflowing with mail from colleagues looking for work; "My (UI) company just closed its doors." "My group was laid off." My this, my that.

Sure, the research community flourishes, but the impact upon industry is minimal. Does HCI flourish at Microsoft and IBM? yes, it does. Does it ever initiate a new product? Does it control budgets? Does it make a large visible impact upon management? No. HCI in these companies – and these are where some of our best people work -- has been of secondary importance. At Xerox, we are of tertiary importance. At Apple, which once had the largest impact of any UI group I know about, the entire UI crew was fired when Steve Jobs took over.

Folks -- we are a secondary profession.

The problem: We do not contribute anything of substance: we are critics, able to say what is wrong, unable to move a product line forward. We add some value, but we are thought of as a cost center -- we add cost to product development. If we want respect and impact, two important things must be added to our skill sets -- and one important philosophy:

Skill one: Design

The Design profession flourishes because they do things, they create. Usability languishes because good usability is invisible. We must become designers. Otherwise, we are invisible resources, and although we think we are indispensable, the world of business knows this to be false.

Skill two: Business

We need to learn to speak the language of business. The four Ps of marketing, the financial language of ROI and NPV

(Return on Investment and Net Present Value – the time value of money). Marketing owns this field. They use all the techniques we know are faulty (focus groups, questionnaires), and with these techniques plus their business skills, they have conquered. We need to regain the space. But this also means we need tools that give good-enough answers in hours, not the days, weeks, and months we now take. Right now our educational training is centered in departments of Psychology and Computer Science. I think we would be better served if we moved to Departments of Marketing (in the business school) and Departments of Design.

The Important Philosophy

Usability is not the most important part of a product. Making a successful product should take precedence. This means that it delivers value and that it is aesthetically pleasing, cost efficient, easy to manufacture, and understandable. Do excess features make the product harder to use? Yes. Do they sell products? Yes. Should we therefore encourage excess features? Probably. We will never succeed as a profession until we learn perspective, until we put our own discipline at the service of those developing products that earn revenue. We need to be thought of as team players, as revenue producers. Now, we are often thought of as usability bigots -- usability above all - and as costly luxuries.

I see a valuable profession being reduced to insignificance within industry -- and the fault lies with us. We need to change our training, our products, and our attitudes.

STUART C. CARD: DEEPEN FOUNDATIONS AND EMPHASIZE DESIGN

As a start, you have to realize that making a discipline that studies people and machines is hard to do successfully. Previous attempts, such as industrial engineering, applied psychology, human factors, and ergonomics have had successes, but also their limitations. To understand HCI, you have to realize that it was organized to overcome two of the limitations of the human factors approach: The first human factors limitation was an orientation to evaluation rather than design. A focus on evaluation means you give up most of the ways of making a difference, do not actually produce a component of the system, and hence practitioners have little actual power, sort low in the pecking order and are expendable in hard times. The second limitation was insufficient foundations and academic institutionalization. The Chapanis National Research Council report found most non-experimental human factors methods were not adequately validated and were nowhere taught.

Now HCI has been pretty successful at institutionalization, with programs in prestigious universities, its own journals, textbooks, growing conferences, and an accumulating set of methods. Furthermore, HCI, starting with the SIGCHI Curriculum Report, has sought to combine in the same discipline training for system design and building with analysis and evaluation. In the main, we should feel pretty good about it.

At the same time, I have some serious worries for the future. This rise in the dependence of HCI on usability labs is basically a regression to one of the limitations of human factors we were trying to overcome. Don't get me wrong, testing is necessary, but design is where the action is. You will just never get great systems out of usability testing; you would never get to the GUI interface by usability testing on DOS. Repeated experience shows that depending exclusively on usability testing just saddles the HCI person with the weak hand.

Instead, we need to equip the HCI person with power tools for design. For me, that implies supplying HCI with supporting science in the form of predictive theories. Predictive theories are not merely frameworks. Predictive theories are things (which one person can tell to another) that can predict a situational or design consequence. Predictive theories *are* generative theories. They are ways of characterizing and hence organizing and constraining the design space. They are ways of *understanding what you are designing*. Let me illustrate the sort of theories I mean. Take the Fitts's Law Theory of the mouse. Of course, you can do the little calculation it includes. But the significance of it are its largely qualitative uses in design: it tells you that only a couple things matter for mouse pointing (a real surprise), that distance can be compensated by bigger targets, that particular muscle bandwidths are the key to the whole thing. Bill Moggeridge of IDEO and I once used the theory to design a device superior to the mouse in just a few days—a device that was also superior to designs his group had worked on for weeks before learning the theory—and we knew the design was superior before doing any user testing. Other examples: Window working set theory solves the mystery of why windows sometimes have such high overheads and under what conditions they do. Model Human Processor calculations are a way of setting system response time goals. Information scent theory is helping us design tools for testing Web usability and maybe accelerated Web browsers. Once you've got one of these things in an area that matters, my experience is that it's not too hard to get product designs out of them (*pace*, Norman). Of course, it's good to do some usability testing as a check. But even if you do, you've already gotten HCI into the design.

In the HCI discipline, we've been making efforts to expand understanding of contextual design and to encourage the formation of a robust HCI interactive design component within industrial design. But we have fallen away from deepening our foundations with enough supporting science. For HCI to be a successful discipline, for it even to survive in universities, it has to have content with intellectual power. This doesn't mean, of course, that every HCI practitioner has to go out and make a theory. Doctors don't have time to sequence the human genome. That's someone else's job. But they can profit from the drugs made from that knowledge. The critical part is that the knowledge generated in the supporting science should take a form that ultimately is useful for something—a whole topic in itself. Similarly, such theories might be embedded in design tools, methodologies, tables of data, or software toolkits—power tools for the HCI design professional. The point I want to make is that just as one shouldn't settle for a discipline less than we can have, one also shouldn't commit the superdiscipline fallacy of thinking that everything is a failure unless we manage to produce an HCI discipline that is more perfect than any engineering discipline ever created. My aspiration is for something somewhere in the lower half of the engineering disciplines, maybe civil engineering. Some good predictive theories, some tools, some tabulated data, some templates of solutions that work (like "codes of practice"), a way to handle imprecisely-known parameters or approximate theories (e.g., "safety factors"), a design methodology, a collection of designs that more or less worked, clear advancement in the field over the years, a collection of classic disasters, constant fighting to get practitioners to pay attention to the new methods, constant harping by the practitioners that the new methods aren't good enough for "real work", stern come-to-Jesus sermons on our shortcomings by white-bearded characters not unlike Don Norman, renewed dedication by innovators and researchers to having an effect on practice: That, my friends, is what the promised land will look like if we get there.

Looking at this list you can see we've got a start. But we'll never get there without more hard work on the foundations. We need a Decade of the CHI Theory. We need a decade in which we put in place a set of theories and theory-based tools to cover a significant part HCI. We need to create a set of papers, texts, tools, tutorials, and summer schools that put in place the materials to teach and use these theories. But if we get organized, when we meet together ten years from now, Licklider, a great believer in predictive theory, might be proud of us.

MARILYN TREMAINE: THE CASE FOR FUNDAMENTAL RESEARCH IN HCI

SIGCHI grew from a small SIG of 68 members in 1982 to the second largest SIG in ACM in 2002. It has over 6000 members and is still gaining in membership while other SIGs are seeing declining membership. The same is true for the CHI conference. We continue to see growth in the number of papers submitted and people attending. We see this occurring when most other computer-based conferences are seeing declining submissions and attendance. This sounds like success. Can it be true? SIGCHI declares itself as the premiere HCI society in the world and few would argue differently.

My argument is that the success is tenuous and that SIGCHI is at a crossroad where serious thinking about its future has to take place if SIGCHI is to continue in its role as the premiere professional society for HCI. Basking in our laurels and not paying attention to important issues that are yet unresolved could make us into a dull ineffectual group with humdrum meetings.

To survive, any field has to grow and develop new knowledge. It has to develop its paradigms and then carry out research that expands these paradigms and leads to dramatic paradigm shifts. Can anyone state what the current CHI paradigm is? Can anyone list the seminal papers that define the field?

I worry that CHI is becoming more like a professional society of plumbers or conference planners -- lacking intellectual depth. Networking is a great part of the conference and people crowd into rooms to hear the latest new interface development. The conferences are very exciting. People make new contacts and learn techniques from others. The receptions are filled with animated conversation and attendees love the panels and plenary talks. This is wonderful, but I think we need more.

The growth of the field's paradigm should be in its papers, but a look at the topic areas of the presentations suggests that the papers are following industry trends rather than leading them. The papers in the conference need to build the field and establish its long-term research credentials, not chase today's hot products.

Without the depth, we cannot solve the serious problems facing HCI, such as how to do design. Without the depth, the field will lose its academic stature and become a collection of practitioners who will start to complain that they are second-class citizens.

The lack of research depth in CHI comes from the huge influx of practitioners who are not interested in long-range research. Their work is important, but our common future will be richer if we cultivate both sides. Research is relevant to solving tomorrow's problems. How do we get the right balance between practice and research?

I think that the CHI conference should separate clever product innovations from fundamental research, with distinct review committees for each. This should eventually promote stronger research and make research papers more attractive, even to practitioners. We can do this by forming three or four mini-conferences within CHI, each with its own papers, panels, short papers and posters. It is still important for the communities to communicate with each other, perhaps on the last day of the conference with a finale featuring the "best of" each mini-conference. I see some glimpses of this happening.

New theories could accommodate exciting developments in multi-modal interfaces. We need to know how to design interfaces that integrate speech, gesture, gaze, thought, and facial expression. Basic research on information visualization could guide designers in choices of colors, shapes, dimensions, and highlighting for rapid presentation of high volumes of relevant information. There is so much to do.