

Human-computer interaction as science

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ABSTRACT

Human-computer interaction (HCI) has had a long and troublesome relationship to the role of ‘science’. HCI’s status as an academic object in terms of coherence and adequacy is often in question—leading to desires for establishing a true scientific discipline. In this paper I explore formative cognitive science influences on HCI, through the impact of early work on the design of input devices. The paper discusses a core idea that I argue has animated much HCI research since: the notion of scientific design spaces. In evaluating this concept, I disassemble the broader ‘picture of science’ in HCI and its role in constructing a disciplinary order for the increasingly diverse and overlapping research communities that contribute in some way to what we call ‘HCI’. In concluding I explore notions of rigour and debates around how we might reassess HCI’s disciplinarity.

Author Keywords

Science; disciplinarity; cognitive science.

ACM Classification Keywords

H.5.m. Information interfaces and presentation (e.g., HCI): Miscellaneous.

INTRODUCTION

Human-computer interaction (HCI) is represented by a large and growing research community concerned with the study and design of interactive technologies. It rapidly emerged from the research labs of the 1970s, matching the lifetime of the Århus Decennial conferences. Perhaps characteristic of all ‘new’ research communities, anxieties have been expressed over its status as an academic object from the very beginning (indeed, as an early career researcher I myself have often felt a similar confusion about what HCI is as an academic object). Many of these anxieties centre around disciplinary shape and how that

shape relates to ‘science’—the topic of this paper.

Discussion about the role of science both *in* and *of* HCI can be traced to various formative exchanges in the early 1980s between Newell, Card, Carroll and Campbell around the deployment of cognitive psychology for designing user interfaces, and the prospects of developing “a science of human-computer interaction” [43, 12, 44]. Since then there have been sporadic expressions—a tendency if you will—towards cultivating some element of ‘scientific disciplinarity’ for HCI. This may be seen in the form of panels and workshops on matters like scientific replication [58, 59] or interaction science [32] that have surfaced at the ACM CHI conference in the last few years. Most recently Liu et al. [36] and Kostakos [35] have argued that HCI is a poor scientific discipline when measured against other *bone fide* examples (such as those of the natural sciences or disciplines with ‘science’ in their title). In this analysis HCI is found devoid of central motor themes that are taken as a signature of thoroughbred scientific disciplines, thus representing a presumed failure of the HCI programme. Echoing the calls of Greenberg and Thimbleby in 1992 [27], work is thus required to make HCI “more scientific” [35].

In exploring these complex debates, this paper addresses a range of cognate concerns in HCI: ‘science’, ‘disciplinarity’ and ‘design’. The argument I present in this paper contends that the status anxiety over HCI as an academic object has its origins in the early formulation of HCI’s research practice. This practice blended the application of cognitivist orientations to scientific reasoning with Simon’s view of design [56], in order to establish a particular research idea—what I refer to as the *scientific design space*. This guides both *what* human-computer interactions are, and *how* we investigate them. This idea, I argue, has configured how many HCI researchers relate to interactive artefacts in their work practices and thus shaping HCI’s disciplinary circumstances and discussions.

It is not the intention of this paper to suggest that cognitivist scientific reasoning is the *only* orientation to reasoning present in HCI research. It is also not within the scope of this paper to fully map out the landscape of different forms of reasoning in HCI (e.g., the ‘designerly’), nor to evaluate the claims of different approaches compared with their achievements. Neither is it the intention to imply that disciplinary anxiety is solvable. Instead, this paper focusses upon cognitivist scientific reasoning and its expression

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through the scientific design space, arguing that this has been an important and persistent force in the broader logics of significant portions of HCI's programme. This is despite repeated suggestions of transformative intellectual changes that HCI as a whole may have gone through.

This paper firstly unpacks the debates that I broadly subsume into questions over the status of HCI as an academic object and its relationship to 'science'; through this I detail two important anxieties that have animated this debate. I then relate these to a core concept—the scientific design space—that I argue emerged in HCI's formative years at the confluence of cognitive science and interface engineering challenges. In this I revisit HCI's relationship to cognitive science, and the introduction of a particular 'picture of science', tracking subsequent influence of this on attempts at crafting HCI's disciplinary architecture. Finally, the paper's discussion returns to evaluate these concepts.

THE STATUS OF HUMAN-COMPUTER INTERACTION

An external view of HCI's disciplinary status could assume that it is secure. For instance, in CHI 2007's opening address, Stuart Feldman (then president of the ACM) described the HCI research as "absolutely adherent to the classic scientific method" [1]. But the picture from *within* HCI seems radically different. By reviewing broad discussions around HCI's disciplinarity in this section, I intend to sketch a background for subsequently addressing the specifics of 'science' in HCI.

Questions over HCI's disciplinarity emerged early in its development. In 1987 ergonomist and HCI pioneer Brian Shackel asked during his INTERACT conference keynote whether "HCI was a discipline, or merely a meeting between other disciplines" [15]; a couple of years later, Long, Dowell, Carroll and others discussed what kind of discipline HCI might be described as [38, 11]. Although Carroll characterises this and his exchanges with Newell and Card as the "theory crisis" of the mid-1980s [11 p. 4], one only need glance at a standard textbook to notice that HCI seems still to be routinely presented by an ambiguous constellation of overlapping disciplinary descriptors (e.g., interaction design, user experience, etc.). The term itself is also problematised, and HCI can be taken to perhaps subsume or compose these various related descriptors. For example, one position adopted by a key textbook—*Interaction Design: Beyond Human-Computer Interaction* [51]—formulates HCI as a contributing academic discipline to a broader field of interaction design.

Here I sketch out two features of HCI's disciplinary anxieties: incoherence and inadequacy. Later on I argue that invocations of 'science' are often attempts to remedy these perceived problems in HCI.

Incoherence

In essence, the core of the incoherence problem lies in the idea that HCI seemingly has few 'secured' propositions that researchers generally agree upon (except, perhaps Fitts's Law?), and no obvious shared commitment to a certain set of problems or ways of approaching them [36].

Debate about this incoherence problem has two poles of discussion: descriptions of 'how things are' (which often include explanations), and prescriptions of 'how things should be'. A key way of describing present incoherence to HCI is the accumulation of diversity in its approaches over time. As Rogers states, "HCI keeps recasting its net ever wider, which has the effect of subsuming [other splinter fields such as CSCW and ubicomp]" [50 p. 3]. As a matter of this accumulation, HCI researchers have tended to 'bolt on' new approaches repeatedly. This accumulation has involved accommodating new epistemological perspectives and the disciplinary objects that come with them. This is perhaps best illustrated by theory and theorising in HCI, which has been accorded importance given its association as a signature of established disciplines. Beginning with a particular cohort of theories drawn from cognitive science and psychology, 'theory' in HCI has increasingly been adopted from diverse origin disciplines [50, 29]. This has introduced a great diversity of technical senses in which 'theory' is meant (see [50 pp. 16-17] for a descriptive account). Question marks are raised over what qualifies, what a theory is useful for, how they are to be organised, what the relationships between them actually are, how new theories should (or can) be developed, amalgamated, divided, or simply decided as 'good' or 'bad', relevant or out of scope. As Bederson and Shneiderman admit prior to an attempt to define theory in information visualisation and HCI, "a brief investigation of the language of theories reveals its confusion and inconsistent application across disciplines, countries, and cultures" [2].

With theory also comes attendant epistemological commitments (which may or may not be honoured, of course) and other associated objects. For instance, these are the methods (e.g., experimental design, experience sampling, anthropological ethnography) and corresponding instruments for administering these methods (e.g., NASA TLX, social network analysis metrics [23]).

This "remarkable expansion" [50 p. xi] tends to be taken sometimes as indications of success (in the form of rich emerging discipline) but perhaps more significantly as a signifier of problems, such that—as Carroll states—"an ironic downside of the inclusive multidisciplinarity of HCI is fragmentation" [10]. Similar views are expressed perhaps most commonly within the program committees of the SIG CHI conference, and in other public fora e.g., *Interactions* magazine [29]. An absence of uniformity of theory, method and instrument is unsettling when compared to formal accounts of how disciplines *should be*, particularly the disciplines with coveted scientific status. The choice thus

seems stark; Rogers raises the question of prescribing *disciplinary order*: i.e., whether to “stem the tide and impose some order and rules or let the field continue to expand in an unruly fashion” [50 p. 2]. The dichotomy presented is now a familiar one where the opposite of disciplinary prescription is “unruly”.

What of the response to this in HCI? One possibility is to redescribe HCI in such a way that creates some semblance of order. This includes attempts to rationalise the existing range of work that occupies the HCI space, perhaps most visibly represented in discussions around ‘turns’ [50] ‘waves’ [5] and ‘paradigms’ [30]. For instance, Rogers offers four key turns: design (early 1990s), culture (late 2000s), ‘the wild’ (mid 1990s) and embodiment (early 2000s) [50].

Another way is to prescribe standardisation; and where standardisation comes, so do calls for ‘scientific’ ways of establishing order. These calls are largely intended, I think, to strengthen HCI’s disciplinary coherence. For example, Whittaker et al. argue that HCI’s “radical invention model” works against “the development of a ‘science’ of HCI”, with their proposed solution being the development of standardised sets of “reference tasks” to support “cumulative research” [57]. This view is consonant with programmatic statements arguing for developing a more science-like approach in HCI through practices like routine replication [27, 26, 58, 59, 31] or other prescribed forms of evaluation [45]. Relatedly, Liu et al. have also argued for the need of prescriptive standards of order via the development of what they term as shared “motor themes” [36].

Inadequacy

The second and interrelated expression of anxiety is that of HCI’s (intellectual) *inadequacy* when positioned as an academic discipline against a roster of other, better established disciplines. In 2002 panel, positions on this were represented in a panel of key HCI figures (Shneiderman, Card, Norman, Tremaine and Waldrop) discussing 20 years of the CHI conference [55]. Shneiderman argued the importance of “gain[ing] widespread respect in the scientific communities”, Norman commented on HCI as a “second-class citizen”, while Card reflected on his aspirations for HCI to graduate to “something somewhere in the lower half of the engineering disciplines, maybe civil engineering”. The situation seems to be unchanged since 2002: in a foreword to a recent (2012) handbook for HCI, Shneiderman reflects that “HCI researchers and professionals fought to gain recognition, and often still have to justify HCI’s value with academic colleagues or corporate managers” [54].

These concerns are political ones—they are motivated by the ways in which HCI is seen to be judged by others (academics, research funders, etc.). As part of this it seems standard practice to perform disciplinary comparisons

between HCI and disciplines that are formally labelled as ‘sciences’, including physics (which remains the favoured model of scientific purity in positivistic philosophy of science), chemistry or biology [50 p. xii, 31]. More specifically, when considering the prospects of HCI’s position amidst ‘the disciplines’, this debate is often configured as a disciplinary contrast between the ‘hard’ and ‘soft’ sciences, and correspondingly, between the rigorous and the less disciplined, between the quantitative and the qualitative, between maturity and immaturity, and between the ideal-scientific and the striving-scientific. Bederson and Shneiderman present a paradigmatic expression of this in an attempt to explain the inadequacies: “Mature scientific domains, such as physics and chemistry, are more likely to have rigorous quantitative laws and formulas, whereas newer disciplines, such as sociology, or psychology, are more likely to have qualitative frameworks and models” [2] (also see Card’s comments on this [55]). Such comparisons, we might note, are of course performed under the assumption of an essential disciplinary comparability, in spite of the questions over the relevance of this endeavour—a point I return to later.

THE SCIENTIFIC DESIGN SPACE AND HCI

In order to understand the twin anxieties of incoherence and inadequacy, I think it helps to return to HCI’s formation. By examining how some key features of present-day HCI emerged from the research labs of North America in particular, this section discusses the intellectual origins of an orienting concept: the *scientific design space*. As part of this I am interested in the relationship to both the ‘picture of science’ in HCI as well as ideas of architecting HCI as a (possibly) scientific discipline. In closing, this section then turns to assess some of the intellectual foundations of this concept, based in Simon’s perspective of design and science.

Designing the mouse

Prior to broad recognition of HCI as a distinctive, nameable research activity—and naturally prior to debate about its status even as a ‘discipline’—the design of the human-computer interface was primarily approached by pioneering research labs as a *construction problem* involving the provision of some possible control of a computer system. Many early fundamental interaction techniques were initially conceived of in this way (e.g., direct manipulation, the mouse, windowing environments [42, 41])—i.e., as primordially technological engineering endeavours that aimed to produce task-functional and efficient user interfaces. Verification of the usability of such interfaces tended to be applied only after design decisions had been made and implemented [10 p. 2]. Challenges were mounted to this software engineering focussed approach by nascent HCI. From this emerged a pairing between design work and cognitive scientific work [9]. Norman described this as “cognitive engineering” [46], although he retained a separation between the different roles of design and

cognitive science. Yet, as we will see, the situation of this pairing in its formation was ambiguous in terms of whether it also is a conflation of the two.

The practical foundations of the scientific design space (i.e., a science *of* design [9], not the scientific *study of* design [14]) erupted from research within Xerox PARC, perhaps illustrated most emblematically by Stuart Card and his pioneering work on the computer mouse. In the book *Designing Interactions* [41]—documenting the history of the design of interactive systems and devices using first-hand accounts from its key players—Moggridge states of the computer mouse: “Stu [Card] was assigned to help with the experiments that allowed [mouse developers Doug Englebart and Bill English] to understand the underlying science of input devices”. Card’s aim was, in his own words, to develop a “supporting science” that would undergird the design activity of emerging interface technologies like the mouse or the desktop metaphor based graphical user interface. While earlier human factors and software engineering influenced work in HCI had been concerned with the use of ergonomic theory, its role in relation to design was generally *verificationist* [9, 10 p. 2], i.e., not used in predictive ways that serviced design. Basic atheoretic trial-and-error engineering approaches were typically being used at the time: in optimising the mouse’s design Card states that “the usual kind of A-versus-B experiments between devices” were no longer sufficient since “the problem with them was that [English, the designer] did not really know why one [mouse design] was better than the other” [41 p. 44]).

Card instead saw a role for “hardening” the “soft sciences of the human-computer interface” [43] through *applying* cognitive science as a way of explaining why interfaces failed or succeeded and thus predictively *guiding* design work. Cognitive science, offering a representational theory of the mind, could be deployed in order to construct a correct “theory of the mouse” [41 p. 44] based on theories of interoperating mental structures that were being developed in cognitive science research. Although initially drawing upon the non-cognitive behaviourist model of Fitts’s Law and Langoff’s work on this, Card’s work integrated this into a fuller assembly of cognitive units (e.g., perceptual processors, motor processors, memory stores, etc.). Cognitive psychology, with its mappings between human action and cognitive units, offered explanations for how Card might formally rationalise a ‘design space’ of the mouse so as to guide the mouse designers’ work along the right pathway according to the predictions of cognitive science. In this sense, cognitive science was employed to ‘tame’ the apparent ‘irrationality’ of design work.

At times Card’s novel application of cognitive psychology challenged assumptions about what was actually important for design decisions being made for input devices like the

mouse. For example, using this approach Card found that the differences between mouse designs was more to do with hand-eye coordination than the device itself. In addition to its explanatory power, Card found that applying cognitive scientific concepts to conceptualise and shape the design space could also be generative; it could offer different design possibilities through prediction. For example, it was found that by designing a mouse-like input device that incorporated “putting fingers together you can maybe double the bandwidth [of input precision]”. This resulted in some clear advice for designers: to “put your transducer in an area that is covered by a large volume of motor cortex” [41 p. 45]—i.e., direct the design *towards* the capacities of the cognitive motor processor and its relationship to the rest of the cognitive subsystem. From this example it becomes clear how the cognitivist orientation was applied not only to the user, but also in shaping the idea of a *scientific* approach to the design space itself. Card, Mackinlay and Robertson later summarised this approach thus: “Human performance studies provide the means by which design points in the space can be tested. We can use this basic approach as a means for systematizing knowledge about human interface technology, including the integration of theoretical, human performance, and artifact design efforts.” [6].

The scientific design space approach and HCI’s disciplinary architecture

The scientific design space of Card and others offers a consistent and strong visionary prescription for how research in HCI can proceed. That Card and colleagues have had a large influence on much of HCI’s development in uncontroversial, but I argue that some aspects of this influence on key forms of reasoning in HCI have been overlooked. In this section, I explore its role in the broader endeavour of describing and prescribing HCI’s disciplinarity. In this sense, I am interested in how the scientific design space offers solutions to some of the disciplinary anxieties of HCI.

In 1983’s *The Psychology of Human-Computer Interaction*, Newell, Card and Moran sought to develop “a scientific foundation for an applied psychology concerned with the human users of interactive computer systems” [8]. Card would later point to the development of the mouse as an “ideal model of how the supporting science could work” [41 p. 45]. Although this hinted at a separation between design space exploration and cognitive science, the nature of that relationship remained largely unspecified. Yet, drawing out the implications of this “ideal model” meant extending the scientific design space of the mouse to a scientific design space approach for human-computer interactions more generally. The first step was to consider input devices as a whole [6], but linked to a broader prescriptive programme of “discovering the structure of the design space and its consequences” [7].

TIME		ACTION	MEMORY	THEORY
(sec)	(common units)			
10 ⁹	(decades)	Technology System	Culture Development	Social and Organizational
10 ⁸	(years)			
10 ⁷	(months)	Design Task	Education Education	
10 ⁶	(weeks)			
10 ⁵	(days)	Task	Skill	Bounded Rationality
10 ⁴	(hours)	Task	Skill	
10 ³	(ten mins)	Task	LTM	
10 ²	(minutes)	Task	LTM	
10	(ten secs)	Unit task	LTM	Psychological
1	(secs)	Operator	STM	
10 ⁻¹	(tenths)	Cycle time	Buffers	
10 ⁻²	(centiseocs)	Signal	Integration	Neural and
10 ⁻³	(milliseocs)	Pulse	Summation	Biochemical

Figure 1: Architecting the discipline—positioning HCI within a scientific order (figure reproduced from [43]).

A clearer scientific disciplinarity to HCI was also developed subsequent statements. In 1985 Newell and Card presented a vision for the role of psychological science in HCI. This work offered a descriptive account of a layered model in which temporal bands (between decades and milliseconds) map to different action units, associated memory and cognitive capacities, and the relevant theoretical bands which apply [43] (see Figure 1). Within this schema HCI’s phenomena of interest sit largely within the psychological and bounded rationality bands, happening to coincide precisely with the concerns of cognitive science of the time and ordering the rest of the space in terms of various related sciences.

The significance of this layered model should not be underestimated. It offers a unified, reductive organisation that is similar to positivistic philosophy of science where higher order sciences are reducible to lower ones (ultimately physics) [20]. In this scheme, it is social and organisation science, various ‘levels’ of the cognitive sciences, neurosciences, and biological sciences, that fill in this order. Critically, this model for HCI is also cumulative: thus, latter developments such as Information Foraging Theory [48]—an influential theory that explains the information search and collection behaviours of web users (for example)—fit within the ‘rational’ band, yet build upon lower, more foundational bands from which information foraging “gains its power and generality from a mathematical formalization of foraging and from a computational theory, ACT-R, of the mind” [32].

As a guiding notion of hierarchical disciplinary order, this work seems to have been influential in sparking attempts at descriptions of HCI. Firstly, Carroll, while in disagreement with Newell and Card’s presentation of the role of psychology in HCI [12], elsewhere concurs to some extent with this organisation in terms of “*level of description*”. Thus, perception and motor skill are typically thought of as

‘lower’ levels of organization than cognition; cognition, in turn, is a lower level of organization than social interaction.” [10].

Shneiderman also adopts this hierarchical descriptive model of relationships between more fundamental and ‘higher’ sciences. For instance he distinguishes between “micro-HCI”, where researchers “design and build innovative interfaces and deliver validated guidelines for use across the range of desktop, Web, mobile, and ubiquitous devices”, and “macro-HCI”, which deals with “expanding areas, such as affective experience, aesthetics, motivation, social participation, trust, empathy, responsibility, and privacy” [53]. Macro and micro HCI have “healthy overlaps” yet have different “metrics and evaluation methods”.

Finally, Rogers presents a description of HCI that seems oriented by a similar hierarchical sensibility. In *HCI Theory: Classical, Modern and Contemporary*, HCI’s (scientific?) disciplinary structure is a logical, hierarchical arrangement of “paradigms, theories, models, frameworks and approaches” that vary in scale, “level of rigor, abstraction and purpose” [50 p. 4]. In this scheme paradigms are the large scale “shared assumptions, concepts, values and practices” of a research community, while a theory indicates an empirically “well-substantiated” explanatory device that resides within a particular perspective or theoretical tradition (and is presumably associated with a particular paradigm). Beneath this sit models, which are predictive tools for designers, of which Fitts’ Law is the most familiar.

Unpacking the idea of the scientific design space

Returning again to the core idea of the scientific exploration of the design space, here I want to unpack its orienting ideas. In doing so, I think we can better understand the lines of reasoning being deployed in its pursuit.

The central idea of design spaces and their systematic and empirical investigation as a scientific matter seems to have a strong resonance with the work of Herbert Simon. Simon was frequent collaborator with Newell, whom Card had been a student of. Simon’s influential book, *The Sciences of the Artificial* [56], is important for this concept in that that not only lays out a programme for a scientific approach to design but also discusses this within the context of Simon’s prior work around solution-searching within bounded rationality (e.g., ‘satisficing’).

In essence Simon argues that the phenomena of the design activity itself demands a scientific approach of its very own: a new “science of design” [56 ch. 5], but one that is unshackled from the tendency to employ methods from the natural sciences. Simon’s conception of this new science of design shares a link to the formulations of Card, Newell and others that I described earlier. Crucially, both these approaches conceptualise *design as an optimisation problem*. As an optimisation problem, the design of “the artificial” (here meaning human-constructed objects such as

interactive digital artefacts) may be rendered as dimensions, enumerated, rationalised, and essentially made docile. This step gives rise to the application of a spatial metaphor of design: i.e., the idea of a design *space* that is “parametrically described” and populated with “device designs as points” [7]. Card positions cognitive science as a scientific way to “structure the design space so that the movement through that design space was much more rapid” [41 p. 45]. Simon offers a more generalised version of this: design spaces not only of artefacts but also economic, organisational and social systems. This view is broad and encompassing, and it is perhaps a conclusion drawn from a fundamentally cognitive conception of the mind. Hence Simon states that “the proper study of mankind is the science of design” [56 p. 138]. For Simon a scientific approach to designing such things involves constructing all design problems as *computational spaces*—ones that are amenable to formalisation and therefore computational search, optimisation and so on. And it is this notion which I argue undergirds the design space concept as it has found its way into forming a scientific approach to HCI research.

Returning to the disciplinarity question once again, the scientific design space idea in its broader construction from Simon seems to offer both security for the status of HCI (in terms of academic adequacy and coherence), and also answers some of the questions presented earlier regarding the status of HCI as a discipline—whether engineering, craft or science [38]. The idea effectively responds by reformulating design problems in ways that let them be “reduced to declarative logic” [16 p. 176], meaning that “design [becomes] a system with regular, discoverable laws” [24 p. 26]. We will return to this topic of ‘design’ in the closing of the discussion section.

DISCUSSION

Having firstly described HCI’s disciplinary anxieties, and then covered formative concepts around scientific design spaces, the discussion now broadly explores the relations between them. I will initially do this through picking apart the intellectual coherence of the design space approach. First I argue that this idea has been very important in HCI research, and moreover, still is particularly in the evaluation of novel input devices. Then I wish to discuss the conceptual problems of the ‘scientific design space’, and in doing so must necessarily also turn to tackle the more general issue of the role of ‘science’ in HCI. Finally, I turn to review the introduction of ‘designerly’ perspectives in HCI and examine how this relates to scientific design spaces.

The adoption of scientific design spaces in HCI

Via Card and others, it seems that Simon’s way of conceptualising the design of artefacts has been a significant contributor to HCI’s ‘DNA’. Yet this impact of Simon on HCI is only occasionally acknowledged [9].

This influence was initially made possible through the importance of cognitive science in HCI’s formation. Conceptually the prospect of transforming design problems into reductive computational spaces (to be addressed by scientific methods) was facilitated by the central implications of a ‘strong’ cognitive science position. Specifically this position offers an isomorphism between the computer and the human (i.e., as a cognitive object, with various input / output modalities). There are two key connotations that follow: firstly that the human is computational, or can be described with computational concepts; and secondly that the designer and therefore design itself is computational. This underlying idea in HCI has enabled the adoption process of the design space model.

The practical expression of these ideas may be readily found in the research outputs of many HCI conference venues like CHI and UIST, but also in those of related communities such as Ubicomp. Following Card and colleagues, it has become the approach of choice for work that evaluates novel input or output devices, but also for innovative interaction techniques for established interface forms (e.g., GUIs, touch and gestural interfaces, etc.). The ‘ideal expression’ of the scientific design space is often conducted under the broader glossed label of psychology. It characteristically involves engaging in task-oriented interface evaluations via a hypothesis-driven experimental format—being often classed as ‘usability evaluation’ [26]. In building hypotheses and delivering their results, classic features of cognitive science theory are recruited, for example cognitive objects like memory, task load and so on. This might also include methods where the rationale is grounded in cognitive science reasoning, such as ‘think aloud’ techniques. Hypothesis testing enables an organised and systematic traversal of the design space particular to the class of device and interface under investigation [6, 7]. As part of this, cumulative, replicable, generalisable and theoretically-informed findings are delivered both to HCI as a whole but also potentially back to cognitive science as instances of applied research. It is also possible that attempts to form novel cognitive theory specific to interactive systems may result, for example Information Foraging theory presents one well-known instance of this (also note its relationship to the ACT-R cognitive architecture [48]).

As an adoption phenomenon in HCI, this approach seems very well-established. But complaints have emerged about the *trappings* of this approach being prioritised at the expense of “rigorous science” [26]. For instance, Greenberg and Buxton articulate this trend as one of “weak science” in HCI that is more concerned with the production of one-off “existence proofs” of designs than systematic rigour that can “put bounds” on the wider design space under test [26]. In comparison, Card, Mackinley and Robertson emphasise the importance of uncovering design space structure [7], and through this taking different device designs as inputs or “points in a parametrically described design space” [7]—in

other words, “systematizing” [6]. In contrast, the prioritisation of existence proofs has meant theory-free meandering explorations of an *unspecified* design space with little clear epistemic justification for taking a design space approach in the first place—or to put it another way, the methods of this approach end up deciding the problems to be tackled [26]. The scientific design space is something of a ‘muscle memory’ for HCI’s relationship to interactive devices and systems.

I must note, of course, that there are broader issues at play here in HCI beyond the intellectual framework of design spaces. For instance, publishing cultures that reward quantity and the ‘least publishable unit’ will also tend to tolerate existence proofs. This is likely encouraged or at least further enabled by the idea of ‘scientific’ cumulative progress in HCI.

Design spaces and scientific method

Now that I have discussed the adoption of the scientific design space mode of HCI research, I wish to tackle the very idea itself in two ways. In this section here I will argue that this mode of work borrows from the natural sciences in spite of Simon’s original articulation. In the section after this I will link the discussion to much more general issues around the notion of ‘science’ itself in HCI’s discourse.

The first point is that curiously, even given his call for genuinely new sciences of the artificial that were specifically *not* derivative of the natural sciences, the position Simon outlines nevertheless relies upon this strategy anyway. It seems that this is a product of the way in which Simon conceives of and specifies design as an activity—and how this could help knowledge about design move on from what he saw as “intellectually soft, intuitive, informal, and cook-booky” ways of conceiving of it at the time [56 p. 112].

As Ehn expresses it, Simon performs something of a ‘trick’ which “*poses the problem* of design of the artificial in such a way that we can apply the methods of [formal] logic, mathematics, statistics, etc., just as we do in the natural sciences” (original emphasis) [16 p. 175]. In other words, because of Simon’s conceptualisation of design activities in terms of spatial, computable design spaces that are essentially reduced to search problems, the deck becomes stacked in such a way that ‘textbook’ understandings of the methods of the natural sciences just happen to turn out to be the relevant choice. And, following Simon, we find the science of the design space in HCI also relies upon *perceived* methods of the natural sciences. Interestingly, this perception is itself second-hand: it is actually that of cognitive science’s version of the methods of the natural sciences not the actual practices per se.

The second point here is the lack of clarity in this notion around design and science as activities (I shall come to unpack ‘science’ and ‘design’ in the sections following this one). Specifically we could draw a distinction between

three forms: 1. design *as a* (cognitive) scientific activity; 2. the application of (cognitive) science *in* design—i.e., retaining some notional separation between these activities; 3. the development of scientific understandings *of* design itself (see [14 ch. 7]).

The question for the scientific design space in HCI is based around some blurring between the first two forms. In some ways the initial programme of Card’s recounted in *Designing Interactions* appears different to Simon’s, yet in others seems to have a similar end result. At first glance Card seems to position his work as offering a “supporting science” that does not replace design (i.e., the application of cognitive science *to* design). He reminds us that it still requires “good designers to actually do the design” [41 p. 45]. Yet, I think the picture is somewhat more complex than this. In later work, such as morphological design space analysis [7], extant designs become ‘inputs’ to a design space which itself generates the parameters along which “good designers” must work. In this way designers must engage with the predictive authority of the (cognitive) scientific shaping of the design space—so as to “alter the designer’s tools for thought” [44]. (Card, Mackinley and Robertson’s story of the “headmouse” user is also worth reflecting upon in this regard [7 p. 120].)

HCI’s ‘picture of science’

At the core of the design space notion—I believe—is a desire to bring some aspect of (or perhaps all of) HCI closer towards a scientific disciplinarity. Doing so offers the promise of addressing anxieties over incoherence and inadequacy. Yet in order to better understand this I argue that we must start to unpack what is meant by ‘science’ and how it is used in HCI’s discourse. I also wish to bracket off ‘science’ and ‘science talk’ more generally and look at *what is done* with it. After examining this, I then move on to contrast these deployments with understandings of scientific practice from philosophy of science and empirical studies of scientists’ work—contrasts that reveal a problematic dissonance between the two.

Perhaps the first debates over what kind of science might be relevant to HCI can be found in the Newell, Card, Carroll and Campbell exchanges of the early 1980s [43, 12, 44]. Here, Carroll and Campbell took issue over Newell and Card’s characterisation of “hard science” (“quantitative or otherwise technical”) and “soft, qualitative science” [43] in their arguments about the possible role of science (cognitive psychology) in HCI. As Carroll and Campbell disputed, this was a “stereotype of hard science” and psychology itself; a false dichotomy around science was created by Newell and Card in order to support a positivist programme [12].

Since then, discourse on ‘science’ in HCI research broadly has featured a web of conflicting deployments of the term. These deployments can offer both descriptions of HCI as scientific and prescriptions about ensuring that HCI is scientific (in some way). It is possible to offer two

contrasting poles of this so as to illustrate the difficulties. At one end ‘science’ can be deployed in its loosest meaning as a synonym of rigour. For instance, I would suggest that Carroll often uses this formulation (see [11]); it is an invocation that seems to avoid scientism or positivist assumptions. At the other end of the scale we see ‘science’ being used to denote a specific set of ‘scientific qualities’ that are seen as gold standards of being scientific. Elsewhere I have summarised what is typically meant here in terms of three linked concepts [49]: 1. *accumulation*—science’s work is that of cumulative progress (e.g., [44, 57]); 2. *replication*—science’s work gains rigour from being reproducible by ‘anyone’ [28]; and 3. *generalisation*—science’s cumulative work builds toward transcendent knowledge. As part of this, various other descriptions of what it means to be “more scientific” [35] are pointed to: an adherence to the scientific method, empiricism and the “generation of testable hypotheses” [32]. In this sense ‘science’ is produced as a description of a general knowledge production procedure that offers standardisation and guarantees against ‘bias’. In this account ‘science’ represents a self-correcting incremental process of knowledge accumulation: “the developing science will gradually replace poor approximations with better ones” [34].

There are of course many *other* ways of talking about ‘science’ which have found their way into HCI [49]. The most obvious would be in vernacular usage—where ‘being scientific’ is used in place of ‘being professional’, ‘being reasonable’, ‘being careful’ or ‘being scholarly’ and so on. Often these forms may be political, for example, using ‘science’ as a rhetorical strategy to assert epistemic or moral authority (‘science’ as good work or a transcendent truth). It could also be used as a way of categorising research as scientific and non-scientific. Or it can be an aspirational label that requests peer recognition—for example, computer science rather than informatics [49]. It seems, then, that the overall ‘picture of science’ in HCI is confused.

I wish to now to examine this picture with that presented by empirical studies of the natural sciences. The first general point that I address to the broad HCI description / prescription of ‘science’ is the difference between *formal accounts* of science (e.g., scientific papers) and the *material practices* that are carried out to produce them. In ethnomethodological accounts of scientific practice the relationship between the two, and their essential inseparability, is explored. Livingston, for instance, describes the *ad hoc* and profoundly local achievement of performing everyday practical laboratory tasks (e.g., determining chemical soil composition). He makes the observation that “From an external point of view, the procedures themselves determined the results. From the point of view of those working in the lab, our joint experiences helped us find what those procedures were in practice and how we could make them work.” [37 p. 155].

Other studies of scientific work in astronomy, physics, chemistry and biology practice unpack the ‘lived order’ of these practices, describing how natural phenomena must be socially ‘shaped’ and transformed into scientific “Galilean objects” for presentation in academic papers and so on [21, 39].

Following this, I think that HCI discourse around ‘science’ broadly tends to make a common mistake in expecting formal accounts and material practices to be interchangeable, leading to various confusions (elsewhere this necessary dissonance has even lead to accusations of fraud [40]). For instance, the replication of results as a necessity in principle for HCI has been described as “a cornerstone of scientific progress” [59]. This notion trades on the idea that formal accounts should provide an “adequate instruction manual for replication work” [28]. Yet when we turn to studies of the natural sciences, from which the principle is ostensibly extracted, we find that firstly most scientific results do not get replicated and secondly that where it is used, replication is a *specifically motivated*, pragmatic action for particular contested, relevant cases [13].

Not only is there the potential for confusion between formal accounts and material practices, but studies of the natural sciences also question the notion of the homogeneous way in which ‘science’ broadly is conceptualised, further problematising the term as we find it in HCI. This use tends to presuppose the existence of a coherent specific set of nameable enumerable procedures that make up ‘the scientific method’; procedures that are also held to be representative of a standardised approach to science in general. Philosophy of the sciences instead argues that there are sciences, *plural*, each sui generis, with no uniform approach or necessary promise of ultimate unity [20]. The notion of ‘the scientific method’ is itself questionable. Drawing attention to problems of an induction-based model of scientific progress, Feyerabend has instead argued that there exists “anarchistic methodology” and counterinduction in scientific practice that is not visible in formal accounts [18]. This is *not* that ‘anything goes’ methodologically, but rather that adherence to formal accounts of method alone cannot explain how science progresses. As Bittner puts it, the natural sciences have “tended to acquire arcane bodies of technique and information in which a person of average talent can partake only after an arduous and protracted preparation of the kind ordinarily associated with making a life commitment to a vocation” [3]. Hence it becomes potentially distorting to compress such a diverse set of practices into a singular but unspecifiable method of ‘science’ so as to draw out principles for being “more scientific” [35].

HCI’s relationship to design

In this section I want to discuss another key element of the design space besides its ‘scientific’ sensibilities—‘design’. Of course, design has always been a concern for HCI

research. As Card, Newell and Moran stated, “design is where the action is in the human-computer interface” [8 p. 11]. The question is *what kind of design*—the deployment of the term ‘design’ is itself somewhat like ‘science’, i.e., a potential source of great confusion [61]. Regardless of the conceptual challenges I raised, Card and colleagues nevertheless very clearly articulated a strong sense of design following Simon. Yet I have argued this adoption at the same time provides an intellectual foundation to some of HCI’s recurrent concerns about developing a scientific disciplinarity. The formalised rigour of the scientific design space has meant this *particular conceptualisation of design* has flourished in HCI; this conceptualisation is grounded in background(ed) and unreflective scientific framing devices. It is for this reason that many novel interactive systems and technologies are still evaluated as inputs to the design space model.

Since at least the late 1990s and certainly the early 2000s, however, HCI has seen the development of its own subcommunity of researchers concerned with what I will gloss here as ‘designerly’ perspectives (a gloss that I will later problematise). As they appear in HCI, designerly perspectives work with a range of terms like ‘design research’, ‘research through design’, etc.; to highlight a few examples I discuss here: [17, 60, 61, 19, 62, 22]. This relatively small but distinct subcommunity is one of the few places in HCI that actually foregrounds Simon’s conception of design—which is suggested by some to be a “conservative account” [17]. In this account, Simon effectively democratises and deskills design by arguing that “[e]veryone designs who devises courses of action aimed at changing existing situations into preferred ones” [56 p. 111]. This logically flows from his bounded rationality view of design as search.

Yet within the context of this designerly perspective, it has been argued that traditions along the lines of the scientific design space model tend to mask wider discussion about the nature of the design activity itself; as Fallman argues, “Design is thus a well-established and widespread approach in HCI research, but one which tends to become concealed under conservative covers of theory dependence, fieldwork data, user testing, and rigorous evaluations” [17]. In short, the absence of designerly alternatives to Simon has meant design is “at best limiting and at worst flawed” in its usage in HCI [60].

Yet, following the pattern of eclectic importations of new literatures to HCI, these debates around designerly perspectives typically (perhaps necessarily) offer a somewhat simplified presentation. As a point of contrast, Johansson-Sköldberg detects five interrelated but different discourses of “designerly thinking”: as creation of artefacts (Simon), as reflexive practice (Schön), as problem-solving (Buchanan), as sense-making (Cross), or as meaning-creation (Krippendorff) [33]. Within HCI accounts of design, the literature tends to brush over such nuances,

although there have been distinctions made between design practice and critical design [61].

Further, as they are expressed within HCI, designerly perspectives have *similar rehearsals* of arguments around matters of disciplinary order, anxieties [19], and the relevance of scientific disciplinarity to design as an activity. For example, Zimmerman, Stolterman and Forlizzi specifically call for research through design to “develop protocols, descriptions, and guidelines for its processes, procedures, and activities” and find its own sense of “reliability, repeatability, and validity” [62]. Gaver characterises this as designerly approaches succumbing to HCI’s tendency towards scientism [22]. It seems that the debate around these designerly perspectives is thus no less susceptible to HCI’s orienting conversations around (scientific) disciplinarity that I have discussed in this paper.

However, perhaps because of the struggle for recognition in HCI, designerly perspectives do tend to consistently emphasise the importance of assessing and valuing designerly research correctly—the argument being that the products of this work do not always fit with how HCI values research. This is highlighted by Gaver (“appropriate ways to pursue our research on its own terms” [22]), Wolf et al. (“its own form of rigor” [60]), Fallman and Stolterman (“rigor and relevance have to be defined and measured in relation to what the intention and outcome of the activity is” [19]), and Zimmerman, Stolterman and Forlizzi (“a research approach that has its own logic and rigor” [62]). Yet this argument about rigour *has not been generalised to HCI as a whole* and remains stuck within the frame of the designerly perspective attempting to gain legitimacy within the HCI community—in concluding I want argue for that generalisation.

CONCLUSION

In this paper I have sought to examine disciplinary anxieties in HCI through picking apart its ongoing relationship to ‘science’. This has meant identifying the idea of the scientific design space—an approach conceiving of designed artefacts as scientific objects, influenced by formative early applications of cognitive science to input devices. This significant approach to design seems to have subsequently configured much HCI research discourse, leading to discussions around scientific qualities of accumulation, replication, and generalisation. Yet, as I have tried to show, matters of science and scientific disciplinarity in this perspective are somewhat problematic conceptually and far from settled. Further, in spite of announcements over HCI’s various ‘turns’ and successive ‘waves’ [5] of development, or even ‘paradigms’ [30], I have contested that some of the key assumptions of HCI have been quite resilient to such apparent changes, even with the introduction of designerly perspectives to HCI that challenge Simon’s conceptualisation of design—indeed, there we also find similar debates played out around (design) science and (design) disciplinarity.

In concluding I wish to suggest two implications. Firstly, that HCI researchers should—with some caveats—stop worrying about ‘being scientific’ or engaging in ‘science talk’ and instead concern themselves with working in terms of *appropriate forms of rigour*. Secondly, that HCI should—again, with some caveats—stop worrying about disciplinary order or ‘being a discipline’ and instead engage with the idea of *being interdisciplinary* and all the potential difficulties and reconfigurations that requires.

From science to rigour

The first point turns on ‘science’ in HCI. To summarise, the paper has presented the role that ‘science’ plays in *descriptions of HCI*, e.g., accounts of HCI research as having scientific qualities. Secondly the paper has highlighted the use of ‘science’ in building *prescriptions for HCI research*, e.g., programmes by which HCI research can be conducted as a scientific discipline of design. These descriptions and prescriptions pertain to HCI research practice. Yet, in both cases I have tried to show that these are problematic when we consider how these articulations compare with what the model—the natural sciences—looks like *as a set of lived practices*. I have argued that the model being invoked by HCI—i.e., formal accounts—and the everyday material practices of natural scientists do not match up (for good reason), meaning that the case for employing said formal accounts of ‘science’ as a descriptor of or prescription for HCI seems weak and potentially confusing. At the most charitable we might say that ‘science’ *could* be used as a synonym for rigour, albeit a highly loaded one.

In abandoning the formal-scientific, I want to emphasise the notion of *appropriate rigour*. This is the idea that rigour in HCI must be commensurate with the specific intellectual origins of the work; e.g., this may be (cognitive, social, etc.) psychology, anthropology, software engineering, or, more recently, the designerly disciplines. This runs counter to the desire for hierarchical disciplinary orderings, standardisation, or other forms of positivist reductionism in HCI that I have discussed in this paper. Firstly, such orderings will necessarily foreground contradictory accounts of rigour, and secondly, invocations of ‘science’ will tend to replace focus on seeking the relevant frame of rigour. Instead, appropriate rigour is achieved “not through the methods by which data is collected, but through the ways in which the data can be kept true [...] during the analysis” [52].

To highlight the notion of appropriate rigour I point to the “damaged merchandise” controversy of the late 1990s, where the reliability and validity of well known usability methods in prominent studies were critiqued [25]. Perhaps one of the reasons why this critique produced a significant response [47] was that it took the studies to task using the intellectual framework that had been implicitly ‘bought into’ (cognitive psychology). As Greenberg and Buxton argue, “the choice of evaluation methodology—if any—

must arise from and be appropriate for the actual problem or research question under consideration” [26].

There are other purposes with which ‘science’ terms may be put to in HCI that might be necessary—albeit as a double-edged sword. For example, in *political* purposes, such as rhetorical, or persuasive uses, ‘science’ may have importance for communicating how HCI fits within research funding structures that adhere to the normative descriptive / prescriptive forms that this paper has questioned. The danger here, of course, is the apparent cynicism of this approach.

From discipline to interdiscipline

The second concluding point turns rethinking disciplinarity in HCI and concerns about constructing a rationalised disciplinary order. While the importance of multidisciplinarity has long been identified in HCI (e.g., [10]), what I argue for here is underlined by Rogers’s characterisation of HCI (for good or bad) as an “eclectic interdiscipline” [50 p. xi]. In other words, the difficulties of assembling HCI into some disciplinary order may be the natural state, the key characteristic of HCI. Reflecting upon this, Blackwell has suggested that HCI could be best conceived of as a *catalytic interdiscipline* between disciplines, rather than a discipline that engages in the development and maintenance of a stable body of knowledge [4]. If HCI is to be a rigorous interdiscipline then it will require working more explicitly at the interface of disciplines. We will need *more* reviews of and reflections upon the landscape of different forms of reasoning in HCI and through this better ways of managing how potentially competing disciplinary perspectives meet together. This paper has touched only one part of the landscape, but there are many more.

At the same time it should be noted that there are dangers here too: being an interdiscipline can mean that HCI research diffuses into contributing disciplines and ‘credit’ is never recognised for HCI. This suggests that in addition to reconceptualising HCI as an interdiscipline, we must think of new and perhaps radical ways to characterise HCI as it is presented to the outside world.

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REFERENCES

1. Bartneck, C. 2008. What Is Good? – A Comparison Between The Quality Criteria Used In Design And Science. In *Proc. CHI '08 Extended Abstracts*. ACM, New York, NY, USA, 2485-2492.
2. Bedersen, B., Shneiderman, B. 2003. *The Craft of Information Visualization*, chapter 8, p. 350, Elsevier, 2003.
3. Bittner, E. 1973. Objectivity and realism in sociology. In Psathas, G. (ed.), *Phenomenological Sociology*. Chichester: Wiley, 109-25.
4. Blackwell, A. F. 2015. HCI as an Inter-Discipline. In *Proc. CHI '15 Extended Abstracts*. ACM, New York, NY, USA, 503-516.
5. Bødker, S. 2006. When second wave HCI meets third wave challenges. In *Proc. of NordiCHI '06*. ACM Press, 2006.
6. Card, S. K., Mackinlay, J. D., Robertson, G. G. 1990. The design space of input devices. In *Proc. CHI '90*. ACM, New York, NY, USA, 117-124.
7. Card, S. K., Mackinlay, J. D., Robertson, G. G. 1991. A morphological analysis of the design space of input devices. *ACM Trans. Inf. Syst.* 9, 2 (April 1991), 99-122.
8. Card, S. K., Newell, A., Moran, T. P. 1983. *The Psychology of Human-Computer Interaction*. L. Erlbaum Assoc. Inc., Hillsdale, NJ, USA.
9. Carroll, J. M. 1997. Human-computer interaction: Psychology as a science of design. *International Journal of Human-Computer Studies*, Volume 46, Issue 4, April 1997, Pages 501-522.
10. Carroll, J. M. (ed.) 2003. *HCI Models, Theories, and Frameworks: Towards a Multidisciplinary Science*, Elsevier, 2003.
11. Carroll, J. M. 2010. Conceptualizing a possible discipline of human-computer interaction. *Interact. Comput.* 22, 1 (January 2010), 3-12.
12. Carroll, J. M., Campbell, R. L. 1986. Softening up Hard Science: reply to Newell and Card. *Human-Computer Interaction*, 2(3):227-249, Taylor and Francis, 1986.
13. Collins, H. M. 1975. The seven sexes: A study in the sociology of a phenomenon, or the replication of experiments in physics. *Sociology*, 9(2):205-224, 1975.
14. Cross, N. 2007. *Designerly ways of knowing*. Birkhäuser GmbH, Oct. 2007.
15. Dix, A. 2010. Human-computer interaction: A stable discipline, a nascent science, and the growth of the long tail. *Interact. Comput.* 22, 1 (January 2010), 13-27.
16. Ehn, P. 1990. *Work-Oriented Design of Computer Artifacts*. L. Erlbaum Assoc. Inc., Hillsdale, NJ, USA.
17. Fallman, D. 2003. Design-oriented human-computer interaction. In *Proc. CHI '03*. ACM, New York, NY, USA, 225-232.
18. Fallman, D., Stolterman, E. 2010. Establishing criteria of rigour and relevance in interaction design research. *Digital Creativity* 21, 4, Routledge, 2010, 265-272.
19. Feyerabend, P. K. 2010. *Against Method*. 4th ed., New York, NY: Verso Books.
20. Fodor, J. A. 1974. Special sciences (or: The disunity of science as a working hypothesis). *Synthese* 28, 2, Springer, 1974, 97-115.
21. Garfinkel, H., Lynch, M., Livingston, E. 1981. The Work of a Discovering Science Construed with Materials from the Optically Discovered Pulsar. *Philosophy of the Social Sciences* 11 (June 1981), Sage, 131-158.
22. Gaver, W. 2012. What should we expect from research through design?. In *Proc. CHI '12*. ACM, New York, NY, USA, 937-946.
23. Golbeck, J. 2013. *Analyzing the Social Web*. Elsevier, 2013.
24. Goodman, E. 2013. *Delivering Design: Performance and Materiality in Professional Interaction Design*. PhD thesis, University of California, Berkeley, 2013.
25. Gray, W. D., Salzman, M. C. 1998. Damaged merchandise? A review of experiments that compare usability evaluation methods. *Hum.-Comput. Interact.* 13, 3 (September 1998), 203-261.
26. Greenberg, S., Buxton, B. 2008. Usability evaluation considered harmful (some of the time). In *Proc. CHI '08*. ACM, New York, NY, USA, 2008.
27. Greenberg, S., Thimbleby, H. 1992. The weak science of human-computer interaction. In *Proc. CHI '92 Research Symposium on HCI*, Monterey, California, May.
28. Greiffenhagen, C., Reeves, S. 2013. Is replication important for HCI? In *Workshop on replication in HCI (RepliCHI)*, CHI '13.
29. Grudin, J. 2015. Theory weary. *interactions*, blogpost (posted 14/03/14), retrieved 15th June, 2015, from: <http://interactions.acm.org/blog/view/theory-weary>
30. Harrison, S. Tatar, D., Sengers, P. 2007. The Three Paradigms of HCI. In *Proc. alt.chi, CHI '07*, San Jose, CA, May 2007.
31. Hornbæk, K., Sander, S. S., Bargas-Avila, J. A., Simonsen, J. G. 2014. Is once enough?: on the extent and content of replications in human-computer interaction. In *Proc. CHI '14*. ACM, New York, NY, USA, 3523-3532.
32. Howes, A., Cowan, B. R., Janssen, C. P., Cox, A. L., Cairns, P., Hornof, A. J., Payne, S. J., Pirolli, P. 2014.

- Interaction Science Spotlight, CHI '14*. ACM, New York, NY, USA, 2014.
33. Johansson-Sköldberg, U., Woodilla, J., Çetinkaya, M. 2013. Design Thinking: Past, Present and Possible Futures. *Creativity and Innovation Management*. 22, 2 (June 2013), 121–146.
 34. John, B. E., Newell, A. 1989. Cumulating the science of HCI: From S-R compatibility to transcription typing. In *Proc. CHI '89*. ACM, New York, NY, USA, 109-114.
 35. Kostakos, V. The big hole in HCI research. *Interactions* 22, 2 (February 2015), 48–51.
 36. Liu, Y., Goncalves, J., Ferreira, D., Xiao, B., Hosio, S., Kostakos, V. 2014. CHI 1994-2013: mapping two decades of intellectual progress through co-word analysis. In *Proc. CHI '14*. ACM, New York, NY, USA, 2014.
 37. Livingston, E. 2008. *Ethnographies of Reason*. Ashgate, 2008.
 38. Long, J., Dowell, J. 1989. Conceptions of the discipline of HCI: craft, applied science, and engineering. In *Proc. 5th conference of the BCS-HCI Group on People and computers V*, Sutcliffe, A., Macaulay, L. (eds.). Cambridge University Press, 9-32.
 39. Martin, A., Lynch, M. 2009. Counting Things and People: The Practices and Politics of Counting. *Social Problems* 56, 2 (May 2009), 243-266.
 40. Medawar, P. B. 1963. Is the scientific paper a fraud? *The Listener* 70 (12 September), 377–378.
 41. Moggridge, B. 2006. *Designing Interactions*. MIT Press, Oct. 2006.
 42. Myers, B. A. 1998. A brief history of human-computer interaction technology. *interactions* 5, 2 (March 1998), 44-54.
 43. Newell, A., Card, S. K. 1985. The prospects for psychological science in human-computer interaction. *Hum.-Comput. Interact.* 1, 3 (September 1985), 209-242.
 44. Newell, A., Card, S. 1986. Straightening Out Softening Up: Response to Carroll and Campbell. *Hum.-Comput. Interact.* 2, 3 (1986), 251-267.
 45. Newman, W. M. 1997. Better or just different? On the benefits of designing interactive systems in terms of critical parameters. In *Proc. DIS '97*. ACM, New York, NY, USA, 239-245.
 46. Norman, D. A. 1986. Cognitive engineering. In D. A. Norman & S. W. Draper (Eds.), *User centered system design: New perspectives on human-computer interaction*. Hillsdale, NJ: Erlbaum Associates.
 47. Olson, G. M., Moran, T. P. 1998. Commentary on “Damaged merchandise?”. *Hum.-Comput. Interact.* 13, 3 (September 1998), 263-323.
 48. Pirolli, P. 2007. *Information foraging theory: Adaptive interaction with information*, vol. 2. Oxford University Press, USA, 2007.
 49. Reeves, S. 2015. Locating the “Big Hole” in HCI Research. *Interactions* 22, 4 (July 2015).
 50. Rogers, Y. 2012. HCI Theory: Classical, Modern and Contemporary, *Synthesis Lectures on Human-Centered Informatics* 5, 2, Morgan & Claypool, 2012, 1-129.
 51. Rogers, Y., Sharp, H., Preece, J. 2011. *Interaction Design: Beyond Human—Computer Interaction*, 3rd Edition. Wiley, 2011.
 52. Rooksby, J. 2014. Can plans and situated actions be replicated? In *Proc. CSCW '14*. ACM, New York, NY, USA, 603-614.
 53. Shneiderman, B. 2011. Claiming success, charting the future: micro-HCI and macro-HCI. *Interactions* 18, 5 (September 2011), 10-11.
 54. Shneiderman, B. 2012. The Expanding Impact of Human-Computer Interaction. Foreword to *The Handbook of Human-Computer Interaction*, Jacko, J. A. (ed), CRC Press, May 2012.
 55. Shneiderman, B., Card, S., Norman, D. A., Tremaine, M., and Waldrop, M. M. 2002. CHI@20: fighting our way from marginality to power. In *Proc. CHI '02 Extended Abstracts*. ACM, New York, NY, USA, 688-691.
 56. Simon, H. A. 1996. *Sciences of the Artificial*. 3rd ed., MIT Press, Sept. 1996.
 57. Whittaker, S., Terveen, L., Nardi, B. A. 2000. Let’s stop pushing the envelope and start addressing it: a reference task agenda for HCI. *Hum.-Comput. Interact.* 15, 2 (September 2000), 75-106.
 58. Wilson, M. L., Mackay, W. 2011. RepliCHI—We do not value replication of HCI research: discuss (Panel). In *Proc. CHI '11 Extended Abstracts*. ACM, New York, NY, USA.
 59. Wilson, M. L., Resnick, P., Coyle, D., Chi, E. H. 2013. RepliCHI: The workshop. In *CHI '13 Extended Abstracts*. ACM, New York, NY, USA, 3159-3162.
 60. Wolf, T. V., Rode, J. A., Sussman, J., Kellogg, W. A. 2006. Dispelling “design” as the black art of CHI. In *Proc. CHI '06*. ACM, New York, NY, USA, 521-530.
 61. Zimmerman, J., Forlizzi, J., Evenson, S. 2007. Research through design as a method for interaction design research in HCI. In *Proc. CHI '07*. ACM, New York, NY, USA, 493-502.
 62. Zimmerman, J., Stolterman, E., Forlizzi, J. 2010. An analysis and critique of Research through Design: towards a formalization of a research approach. In *Proc. DIS '10*. ACM, New York, NY, USA, 310-319.