HCI as an Engineering Discipline: to be or not to be!?

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***Abstract*— One of the major challenges in the emerging interdisciplinary field of human-computer interaction (HCI) is the specification of a research line that can enable the development of validated design knowledge with a predictive power for the design of interactive systems. Based on the three different elements in the design of interactive systems: (1) human being(s), (2) technical artefact(s), and (3) context of use, different academic disciplines contribute with different research paradigms to this new field: social sciences with a strong empirical and experimental approach, industrial and interaction design with a strong emphasis on artistic design, and engineering disciplines with a strong technical and formal approach. This programmatic paper presents, discusses and recommends a possible way to integrate the strengths of different research and design paradigms based on triangulation, and we argue for HCI as an engineering discipline.**

***Index Terms*— human computer interaction, design paradigm, research agenda, triangulation.**

I. Introduction

The Human-Computer Interaction (HCI) community is diverse. Academics and practitioners from science, engineering, design or art contributing to its rapid development, but communication and cooperation between the different disciplines can be challenging at times. The Association for Computing Machinery (ACM) Computer Human Interaction (CHI) conference is the largest and arguably one of the most important conferences in the field, which is organized through their Special Interest Group Computer Human Interaction (SIGCHI). At the 2005 SIGCHI membership meeting, the organization of the next conference was discussed, which ignited a shouting match between academics and practitioners [1]. This outbreak of emotions illustrates the tension between the different groups and it can be explained by taking a closer look at the paradigms under which they operate. Already about ten years ago Rogers, Bannon and Button [2] opened and discussed three major questions: (1) What is the problem in [or with] HCI? (2) What does a theoretical approach have to offer HCI? (3) How does a theory relate to practice? In this programmatic paper we will address, discuss and provide possible answers to these three questions: in the past, at present, and hopefully for the future as well (see also [3]). Before we can directly address possible answers (see chapter 5), we have to introduce definitions, terms and concepts about research paradigms (see also [4]).

A. Some Definitions

We will use the established term HCI in a wide scope. With upcoming new technology (e.g., ambient and aware systems, mobile and entertainment computing, etc.) the traditional term HCI seems to be quite limited. Furnas [5] addresses this issue by broadening the scope of HCI to ++HCI. We mainly agree with Furnas' scope of ++HCI. Therefore we will use a very broad definition of HCI throughout this paper (including adaptive and non-adaptive systems, professional, home consumer and entertainment products, etc.). HCI investigates and develops *interactive* products, systems or services that contain at least some computational power.

Science as an activity is the concerted effort to understand, or to better understand, the natural world and how the natural world operates, with observable evidence as the basis of that understanding. It is done by investigating observable phenomena, and/or through experimenting that tries to simulate observable processes under controlled conditions. The logic of modern science requires that observations or facts guarantee the validity of generalizations or theories [6]. Science is not art; because art is largely an individual's effort to communicate his or her ideas or feelings in an implicit manner via artefacts. On the contrary, science is a group effort to explicitly describe and understand reality. Science is not technology either. Although science can lead to technology, and it uses technology, it is *knowledge* by nature [7]. Following van Aken [8] we shall use the "term 'scientific' like the German 'wissenschaftlich' or the Dutch 'wetenschappelijk', meaning 'according to sound academic standards.' Thus, its meaning is not confined to the natural sciences" (p. 242). We will use the term *science* to describe *research* in the positivistic paradigm only, and we will use the term *academia* to describe the whole.

According to Merriam Webster's online dictionary[1], *'engineering'* is defined as: (1) the activities or function of an engineer; (2a) the application of science and mathematics by which the properties of matter and the sources of energy in nature are made useful to people; (2b) the design and manufacture of complex products (e.g., software engineering); and (3) calculated manipulation or direction (as of behaviour). We will mainly refer to definition part (2a) and (2b) further on. *Academic research* consists of 'science', 'engineering', and other activities according to sound academic standards.

B. How a Discipline Develops

All over the world, research communities are contributing to the growing area of HCI, based on the context in which each community is established (e.g., art, industrial design, computing science, software engineering, electrical and mechanical engineering, psychology, sociology, ethnology, etc.). The survival of these communities depends on their abilities to adapt to their environment, and to which extent the whole interdisciplinary research arena can be established as such. Nowadays, several stakeholders are requiring more interdisciplinary research than in the past [9]. HCI is by nature interdisciplinary, and started almost 30 to 50 years ago [10], [11]. What are the main theories, artefacts, and methods developed until now? What are the remaining challenges for HCI?

In this paper we try to offer a broad and ambitious view to continue a discussion about the possible academic future of HCI. We will begin by describing some aspects of how academic disciplines can evolve, which the relevant phases are, and what the possible requirements are that have to be fulfilled. In the next step we discuss the relevant paradigms and discuss how the different paradigms could be merged into a necessary new one. In the final part of this paper we present a general concept in which interdisciplinary research for HCI may benefit from a structured approach via triangulation.

Böhme, Van den Daele, Hohlfeld, Krohn and Schäfer [12] differentiate three phases of development in academic disciplines: (1) *Explorative phase*: "Methods are predominantly inductive in character, and research is determined by strategies aimed at classification... The dynamics of the field are characterized more by discovery than explanation. The fine structure of the objects of study remains largely unknown, and is handled in a manner closely paralleling cybernetics' famous 'black box'. The scientist knows the relevant input and outputs – but what goes on between remains a mystery". (2) *Paradigmatic phase*: "The onset of the paradigmatic phase is marked by the emergence of a theoretical approach which is able to organize the field. The introduction and elaboration of this approach represents a theoretical development with a definitive end. ... The theoretical dynamic of the paradigmatic phase is evidently one which can come to a conclusion – that is, can lead to mature theories which contain a fundamental, and in certain respects a conclusive, understanding of the discipline's research object". (3) *Post-paradigmatic phase*: "Where the organizing theories of scientific disciplines are clearly formulated and comprehensive, the possibilities of revolutionary changes or spectacular generalizations of their basic principles are commensurably reduced. Instead, the dynamics of theoretical development will be determined by the application of paradigmatic theories for the explanation of complex systems which can be subsumed within them" (pp. 6-9). Although Masterman

[1] See at http://www.m-w.com/dictionary/engineering

[13] could identify 21 different meanings of Kuhn's term 'paradigm', we will still use this fuzzy term to refer to the epistemological basis of a research community.

C. HCI: Looking Back

It seems obvious that the present state of affairs for the interdisciplinary field of HCI is in the explorative phase ([14], p. 45), though it may be able to move on to the paradigmatic phase in the future. This statement does not necessarily exclude the possibility that different research communities contributing to HCI are already in a paradigmatic, or even in a post-paradigmatic phase. According to Grudin [15], the origin of HCI can be located between human factors and ergonomics on the one hand [16], and software engineering on the other [17], without being merged with one of these two but cannibalising both. According to Hartson[18] HCI is "cross-disciplinary in its conduct and multidisciplinary in its roots, drawing on - synthesizing and adapting from - several other fields, including human factors (e.g., the roots for task analysis and designing for human error in HCI), ergonomics (e.g., the roots for design of devices, workstations, and work environments), cognitive psychology (e.g., the roots for user modelling), behavioural psychology and psychometrics (e.g., the roots of user performance metrics), systems engineering (e.g., the roots for much pre-design analysis), and computer science (e.g., the roots for graphical interfaces, software tools, and issues of software architecture)" (p. 103).

What were the main research topics of HCI in the past? Hunt [19] analysed 1374 papers published between 1990 and 1999 in leading international journals (e.g., ACM Transactions on Computer-Human Interaction, Behaviour and Information Technology, Human Computer Interaction, Interacting with Computers, International Journal of Human Computer Interaction, International Journal of Human Computer Studies, etc.). He categorized all papers as follows: knowledge-based systems and theory (18.6%), design theory and software engineering (13.7%), language interfaces, i.e., text, speech, hypertext, hypermedia (11.3%), computer mediated communication (8.1%), social, cultural and health implications of computers (7.3%), system testing and evaluation (6.8%), menu, icons, and graphics (5.7%); all other 16 categories accounted for less than 5%. One interesting result should be mentioned: only 0.6% of all papers fall in the category 'HCI research issues' [19]. Clemmensen [20] analysed 17 years of research published in the journal 'Human Computer Interaction'. One of his major and important results is that the number of published papers based on 'hard science' (according to Newell and Card [21], [22]) has been decreasing since 1994. "This trend cannot be explained by a decreasing number of all empirical studies, …, where the number of theoretical studies for the first time after the first two years becomes higher than the number of empirical studies. These trends support Hartson's assumptions that theoretical studies play a significant role in the literature [18]. However, the decreasing tendency in the use of laboratory experiment indicates decreasing support to Hartson's claim that much of HCI theory comes from cognitive psychology" ([20], p. 272).

Since about 1990, several stakeholders have started to become concerned about the ongoing research, and some important discussions have taken place: CHI panel 1991 on 'HCI theory on trial' ([24]); INTERCHI workshop 1993 on 'rethinking theoretical frameworks for HCI' [2]; CHI workshop 1996 on 'educating HCI practitioners: evaluating what industry needs and what academia delivers' [23]; CHI workshop 2000 on 'national and international frameworks for collaboration between HCI research and practice' [26]; CHI panel 2002 on 'CHI@20: fighting our way from marginality to power' [27]. And these discussions continue today [1]. From the CHI 2002 panel two statements are worth quoting: one from Don Norman (in [27]) "We do not contribute anything of substance: we are critics, able to say what is wrong, unable to move a product line forward. … The Design profession flourishes because they do things, they create" (p. 689); and the other from Stuart Card "The second limitation [of HCI in the past, added by author] was insufficient foundations … The Chapanis National Research Council report found most non-experimental human factors methods were not adequately validated …" (p. 690). Several papers reflect on the past performance of HCI: 'a preliminary analysis of the products of HCI research' [28]; 'toward an HCI research and practice agenda based on human needs and social responsibility' [29], 'HCI-whence and whither?' [11], 'HCI in the next millennium: supporting the world mind' [30], 'a reference task agenda for HCI' [31], and more recently 'crossing the divide' [15]. Despite all the concerns discussed, Grudin concludes: "I am optimistic about the future of scholarship and scientific communication" ([15], p. 23). On what foundation is this optimism based?

D. HCI: Looking Forward

Although it is not possible to design a new paradigm, this has to emerge from different activities with the aim to setup a research community. But it is still possible to look at the main boundaries that have to be taken into account to maximize the success for this kind of endeavour. The following main activities can be identified: (1) institutionalisation via research centres and groups in academia and industry; (2) networking via major international conferences (e.g., ACM CHI, IFIP INTERACT, HCI international, etc.); (3) establishing peer-reviewed journals, (4) proper education and teaching of enhanced models, solutions, and tools [28] [32] [33], and last but not least (5) identifying a standard or process for determining the quality of research ([15], p. 5). Activities (1) to (3) have been well done so far, but (4) and more (5) especially seem to be under-developed. Hence, we will focus on these two activities further on. We do *not* argue that in the past the HCI research community performed suboptimally; this is obviously not the case. Just in historical terms we have to move on and work hard to mature up to a level our customers (i.e., the practitioners, etc.) would like to have us, and thus the *quality* of our main deliverable: *HCI design knowledge*.

The main question still is: how is it possible to improve the maturity of our discipline? What are the minimal requirements for a HCI research agenda that have to be satisfied to enable successful research? To make an answer possible, we first have to address the following questions: what is a paradigm, what are the relevant paradigms for our scope of research, and if there are differences, how can we combine them fruitfully?

II. DESIGN PARADIGMS

All over the world, several research communities, i.e. *human-computer interaction* [34], *human factors* [35], *software engineering* [17], and *management information system* [36] are struggling with their foundations, even if they are not fully aware of this. Following Kuhn's model [37] of scientific development, it can be proposed that the inter-, cross- and multi-disciplinary research arena of HCI may be considered an arena of several distinct *communities* that coalesce around associated paradigms. *Paradigm* is defined in the Kuhnian sense of a *disciplinary matrix* that is composed of (a) shared beliefs, (b) values, (c) models, and (d) demonstrative examples that guide a 'community' of theorists and practitioners [37] [38]. Dorst [39] presents an empirical comparison of two approaches of design methodology: *reflective practice* and *rational problem solving*. To this purpose, he introduced and discussed the two most influential paradigms related to these two approaches: (a) *phenomenology* for 'design' and 'engineering' research (reflective practice) and (b) *positivism* for scientific normative research (rational problems solving). *Phenomenology* ascertains and studies the kinds of elements universally present in the phenomenon. The phenomenon is whatever is present at any time to the mind in any way. "The business of phenomenology is to draw up a catalogue of categories and prove its sufficiency and freedom from redundancies, to make out the characteristics of each category, and to show the relations of each to the others" (Harvard Lectures on Pragmatism, CP 5.43[2], 1903). Research according to the *positivistic paradigm* is based on concepts of reality that invoke scientists to look for what is, and not to speculate on what might be, while searching for true

[2] CP x.xx (volume.paragraph) = (*Collected Papers of Charles Sanders Peirce*, 8 volumes, vols. 1-6, eds. Charles Hartshorne and Paul Weiss. Cambridge, Mass.: Harvard University Press, 1931-1935)

meaning, and that the generated concepts really exist backed by empirical data.

What can we say about design and engineering activities? To which paradigm do these activities belong? One position is clearly expressed by Bayazit [40]: "an artist's practicing activities when creating a work of art or craftwork cannot be considered research" (p. 16). Dorst [39] characterizes engineering as design activities as 'thrown' into a design 'situation' ('thrown-ness' in German 'Geworfenheit', see [41]). Winograd and Flores [42] illustrate this kind of 'thrown-ness' as follows: "When chairing a meeting, you are in a situation that (I) you cannot avoid acting (doing nothing is also an action); (II) you cannot step back and reflect on your actions; (III) the effects of actions cannot be predicted; (IV) you do not have a stable representation of the situation; (V) every representation you have of the 'situation' is an interpretation; (VI) you cannot handle facts neutrally; you are creating the situation you are in". The following two main aspects characterize this kind of situation: (1) no opportunity for 'reflection' (see (I), (II), and (V)), and (2) no stable and [maybe] predictable reality (see (III), (IV), and (VI)). A design situation based on 'thrownness', is a typical context characterized by the latter two main aspects. The designer creates and synthesizes the situation while he/she is acting in it. To focus on the constructivistic and synthetic aspects of this paradigm, we will replace the term 'phenomenology' by the term 'constructivistic paradigm' from now on.

According to the *positivistic paradigm* most of the dominant activities in natural and formal sciences can be characterized as a rational problem-solving approach. This main approach can be described as ... "the search for a solution through a vast maze of possibilities (within the problem space)... Successful problem solving involves searching the maze selectively and reducing it to manageable solutions" [43]. In this paradigm, all knowledge should be described, represented and processed in an *objective manner*: independent of an undisclosed individual and personal knowledge base. The personal knowledge base (e.g., 'craft skill') is exclusively accessible to the individual him/herself, even sometimes without the opportunity for conscious reflection about the content (see e.g. the 'knowledge engineering bottleneck'; [44]). In natural sciences most formal descriptions are validated – sooner or later – via empirical observations, experiments or simulation studies. Models and theories generated under the *positivistic paradigm* are strong in abstraction and therefore prediction. This predictive power is based on abstraction of all details that are perceived as not relevant in reality at time (t_1). In the context of a particular model or theory all differences between reality at time (t_1) and reality at time (t_2) are classified as irrelevant and uncorrelated *noise*. These models and theories normally can not handle singularities and unique cases in time. Because there is no asymmetry in time (neither forward nor backward), we can also use these models and theories for *explanations*. However, if we want to change reality we have to add models and artefacts that fit into reality where details matter. *Concrescence* is the complementary activity to abstraction, done by adding all 'necessary' details that were abstracted from (see Fig. 1).

Nowadays, the positivistic paradigm seems to be the dominant characterization for a scientific research line. But how can we incorporate *design* as an academic activity? According to the aspect '*no reflection*' Winograd and Flores [42] propose to overcome problems by approaches like *reflective practise* as introduced by Schön [46]. Following Schön [46], a "practitioner approaches a practice problem as a unique case. He does not act as though he had no relevant prior experiences; on the contrary. But he attends to the peculiarities of the situation at hand" (p. 129). The practitioner confronted with a concrete design problem "seeks to discover the particular features of his problematic situation, and from their gradual discovery, designs an intervention" or action (p. 129). Schön's concept of 'reflection-in-action' can be applied to a broad range of research activities, in which the scientist is looking for a particular solution for a given set of constraints (e.g., design of an experimental set-up, a formal proof, a research plan, a technical artefact, etc.). The implicit nature of all these activities is the synthetic approach, to come up with something concrete as part of reality ('concrescence', see

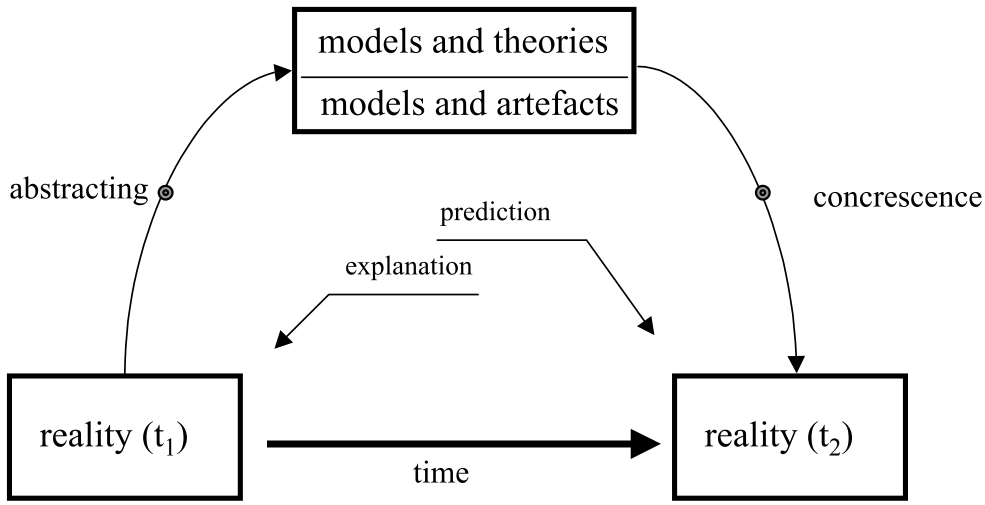

Fig. 1. A general schema for the process of academic knowledge development (adopted from [45], Fig. 2.8 and [39], Fig. 6.2)

Fig. 1). The two aspects of academic activity 'abstraction' and 'concrescence' are both necessary and complementary. If this is an appropriate description, why then does academia primarily focus on the positivistic paradigm and praise its 'abstraction'?

Given a reality at time (t_1), science observes and analyses particular phenomena, makes proper abstractions, and tries to predict similar phenomena for reality at time (t_2) (see Fig. 1). To preserve a stable view on reality [reality (t_1) = reality (t_2)], science has to operate under the following assumption, and this assumption seems to be *essential*: [{model, theory} $\notin$ reality]. Whatever a theory about the phenomenon *gravity*, for example, explains and predicts, this theory does not influence or change the phenomenon *gravity* at all! In this sense, models and theories of science (in the positivistic paradigm) are *not* part of the investigated and described reality; they are *apart* from this reality ([47], p. 12). We will use the term 'reality' (*excluding* models and theories) further on to make this distinction clear compared to the broader meaning of the term *reality* (*including* models and theories). The underlying mechanism to guarantee the fulfilment of the assumption is *reductionism via abstraction*. Any differences in empirical measurements between (t_1) and (t_2) are interpreted as just accidental factors ('noise'), which do not contradict the theory. With only knowledge, based on theories developed under the positivistic paradigm, the design of a concrete artefact is almost impossible, because the knowledge in these theories is *purified* from the changing contextual factors between reality at (t_1) and at (t_2). This lack of specific knowledge for any concrescence (e.g., craft skills) in science gives *design* and *engineering* disciplines their right to exist. Dreyfus [48] stimulated and continued a very important discussion about the importance of *intuitive expertise*, complementary to rational problem solving.

On the other side, activities under the *constructivistic* paradigm claim to influence the reality and therefore to change this reality via the developed artefacts [reality (t_1) $\neq$ reality (t_2)], and in fact they do! The design and engineering disciplines develop knowledge to make the *concretisation* successfully possible. This knowledge realized in the form of *models* and *artefacts* can be interpreted as part of the reality, and not apart from it [{model, artefact} $\in$ reality]. But how can design and engineering disciplines guarantee a *stable reality*, as desired by science? If models and artefacts are seen as part of the reality, i.e., as a subset of the reality under consideration, then any action that changes this subset changes the whole set (reality) as well. So, none of the *constructiv-*

istic disciplines can guarantee a stable reality, and they do not want to [49]. Up to now, the main conclusion is that knowledge developed in the positivistic paradigm and knowledge developed in the constructivistic paradigm is different. If the schema in Fig. 1 describes the whole process for developing knowledge, independent of a given paradigm, then the positivistic and the constructivistic knowledge can be seen as two subsets of a superset of knowledge: [{model, theory} ∪ {model, artefact} ≡ {model, theory, artefact}]. In this sense we can describe them as *complementary* [50].

Probably, the most practical value of positivistic knowledge is the specification of limits and boundaries under which constructivistic knowledge has to operate. For example, the state-of-the-art theory in thermodynamics explains and predicts that the design of a 'perpetuum mobile' is not feasible. Therefore, any attempt to design such a kind of system is assumed to be *unrealistic*. The challenge in combining both kinds of knowledge is creating artefacts (*attractors* as singularities), which fall *inside* the constrained design space provided by positivistic knowledge. This kind of *validated design* is quite challenging, because the designer has to take almost all relevant constraints and limits into account. This consequence usually is probably one of the main reasons for designers to oppose or even reject this position. But still, how can an academically sound *research* line be characterized that includes design-related activities? Let us have a closer look at existing *design-related* activities inside different academic disciplines.

First, we will shortly describe and characterize the most well-established disciplines. Disciplines such as physics, chemistry, etc. present themselves as 'natural sciences'. Theory development takes place in a strictly formal manner with a rigorous experimental validation practice. Truth is based on the conformity of empirical data with the observed 'reality' of the phenomenon under investigation. The most important bases for conclusions is *inductive logic*. Academic disciplines like mathematics present themselves as 'formal sciences'. Truth is based on logical consistency. One of the most important bases for conclusions is *deductive logic*. On the other hand, humane disciplines can be classified as 'ideal sciences'. Truth is based on *belief*: hermeneutic evidence grounded in intuition! The most important basis for conclusions is a *value system* contained in an individual knowledge base.

How is it possible that sciences based on a positivistic paradigm claim to be and presents themselves as *true* academic disciplines (compared to the rest), even if they include (and need) constructivistic and synthetic components as well? One possible explanation is the important asymmetry between both kinds of knowledge: 'positivistic' knowledge claims a more fundamental status than 'constructivistic' knowledge. 'Positivistic' knowledge has a stable predictive and explanatory power *over time* (see Fig. 1; based on the underlying idea of *absolute* and *timeless truth*, see [6]), because it is particularly designed for this purpose. But this approach pays the price of not being able to reach reality: to explain and predict, but not to *touch* and *change* reality (see also [51]). In the rest of this paper, we will develop an outline for research in the field of HCI, which tries to take the considerations and conclusions of this section into account.

III. WHAT IS HCI ABOUT?

A. Overview

HCI claims the broadest range of research activities, including all contributions from the above mentioned communities. In the context of this paper we define the field of HCI as follows: HCI is a discipline concerned with the design, evaluation and implementation of interactive systems for human use and with the study of relevant phenomena surrounding them ('context of use'). Furthermore, we define an interactive system as a work system {WS} := [{U}, {S} with ICT[3] component(s), other components]. Fol-

[3] ICT = Information and Communication Technology.

lowing Dowell and Long [7], we distinguish between a work system {WS} and a [work] domain {WD}, and the relation between these two components (see Fig. 2). The major goal for HCI to become an engineering discipline is "the design of behaviours constituting a work system" {WS} "whose actual performance (PA) conforms with some desired performance (PD)" (p. 1522).

The relationship between the work system and the [work] domain has to be investigated in the context of task and domain analysis related activities (e.g., [52] [53] [54]). One of the main issues in the relation {WS}↔{WD} is the man-machine function allocation problem [55]. A system {S} for a real-world application domain {WD} can only be developed taking {WS}↔{WD} the following into account: {S} | {WS}↔{WD}.

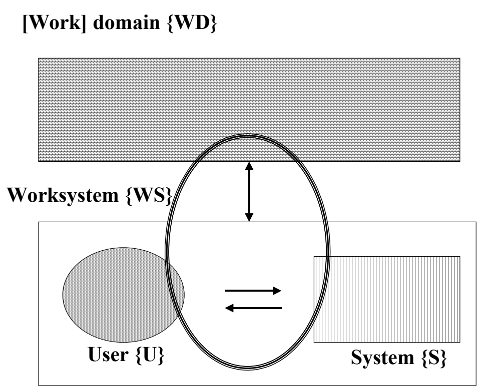

Fig. 2. The distinction between the interactive work system {WS} and the work domain {WD} (adopted from [7]).

Developing a work system without taking a [work] domain into account is risky, because later there is no guarantee that the designed work system can contribute to achieve the desired performance of the whole system. On the other hand, without technology push, the technical option space for solving real-world interactive problems would be seriously constrained (e.g., the visionary device of Vennevar Bush called MEMEX, see [56]; [57], pp. 41-42).

So far, the main conclusion is the need to investigate the relationship between 'push - based' developed technology and the requirements coming from existing or planned work domains. Which type of interaction technique is appropriate for which type of task and work domain? However, how can we develop interaction techniques without having a possible interactive task in mind? One possible answer is the development of 'generic' interaction techniques, which should be applicable to *any* task type ('generic' in the sense of 'work-domain independent'). Is for example the 'mouse' based interaction really a generic and optimal interaction technique (see the contradicting empirical results in [58]). On a more general level, what kind of research line has to be established to gain valid answers to this kind of research questions?

B. Work System

The two major sub-elements of the work system are of a completely different nature: (1) humans can be described in terms of perceptual, cognitive, acting, and emotional capabilities and limitations (user {U}); (2) the system (a technical artefact) can be described in processing power, system architecture, input/output relations, functionality, material properties, etc. (system {S}). Green, Davies and Gilmore (1996) differentiate between three different views: (1) psychological view, (2) systems view (focus on artefact), and (3) interactive view. We follow this classification to describe required design knowledge: (1) design knowledge related to the *user* {U} (e.g., trainings, tutorials, help systems, etc), (2) design knowledge related to the *system* {S}, and (3) design knowledge related to the *interaction space* {IS} [59]. The 'context of use' will be discussed in the next section about the work domain.

Attempts to integrate the two *worlds* of the user and of the system have a long tradition and still it is the most important challenge (see [60] [61]). If we conceptualise the field HCI primarily as an *engineering* discipline [7], we have to translate the research results from the social and cognitive sciences into technical dimensions that can be directly applied to solve design issues for interactive systems (e.g., [62]). This kind of *translation* is a valuable and challenging goal for the whole HCI research community.

Even today, one of the dominant HCI research lines has focussed on the design of the interface of the interactive system, but as a matter of fact, *interface designers* design the *interaction space* {IS} between the user and the system. This view will have a strong impact on the theoretical foundations of HCI: describing user-system interaction as a *dynamic* relation $\{IS\} := f[\{U\} \leftrightarrow \{S\}]_t$ taking the relation $\{WS\} \leftrightarrow \{WD\}$ into account. The *interaction framework* of Barnard and Harrison [59] is a first and valuable attempt in this direction. Any kind of *inter-action* has at least one essential component: the *synthetic* part, to end in something concrete.

Two different approaches to investigate the user can be distinguished: (approach-1) to treat a user as a human being with a *physical body* (e.g., the view of biology, psychophysics, physiology, etc.), and (approach-2) to treat a user in a particular *context of use* (e.g., design, marketing, manufacturer, psychology, etc.). Approach-1 looks at a user without taking the relation $\{WS\} \leftrightarrow \{WD\}$ into account; but this description is not completely correct. The human being is investigated in his/her *natural* environment, which can be described in physical terms. So, one could interpret {WD} as the whole world, specified and described beyond any cultural, political, economical, and social constraints (the purely physical view to nature). Approach-2 tries to incorporate the relationship $\{WS\} \leftrightarrow \{WD\}$ in a more specific manner: {WD} as a concrete (inter)action space with all related semantics regarding cultural, political, economical, and/or social dimensions (see [63] [64] [7]). To connect approach-2 to approach-1, we must look for a theoretical foundation of human activities that can take explicitly *contextual* boundaries into account (e.g., activity theory, see [65] [66]).

Green *et al.* [60] discuss in detail the pros and cons of trying to connect these two approaches. They describe three lines of possible development: “Two of these lines are ventures in developing representations of interactive situations which apply equally to both partners, the person and the system. The third line is even less theoretically ambitious, seeking only to crystallize and expose concepts which many users (even if not HCI workers) already recognize, but which have not yet been presented in an organized way” (p. 109).

C. Work Domain

In all design projects, which have to develop a fully-fledged product for a particular market segment, we are obliged to investigate the domain in which the product has to survive. A domain can cover a broad range of social activities. Following Dowell and Long [7] “a domain of application can be conceptualised as: ‘a class of affordance of a class of objects’. Accordingly, an object may be associated with a number of domains of application (‘domains’)” (p. 1524). Following Nardi [67] three main approaches to investigate a user in a context of use or task context can be distinguished: (1) *situated actions* [64], (2) *distributed cognition* [68], and (3) *activity theory* [69]. Nardi [67] concludes: “Activity theory seems to be the richest framework for studies of context in its comprehensiveness and engagement with difficult issues of consciousness, intentionality, and history” (p. 96). With a concerted effort by HCI academics to develop a systematic conceptual framework (the work domain as a well specified context for human activities), much progress could be made. We could then answer questions like, for what kind of task is a particular type of interface and interaction technique most appropriate.

Last, but not least, a very practical argument has to be discussed: the argument that a *task context* cannot be excluded from a research line to acquire validated design knowledge utilizing on empirical investigations. Any kind of a human action can be described and interpreted as a task and/or problem-solving activity [66]. For example, if we investigate human (re)actions to a controlled environment in a laboratory setting (a very artificial context), then – whatever this test subject is doing – his or her (re)actions cannot be interpreted without taking the concrete *task context* into account (often the intended part of the design of the experimental setting: the set of independent variables/ factors). The main critique against experimental settings is the often unclear relation between an artificial experimental setting and natural task contexts (the *ecological validity* discussion; see [63]). Nevertheless, even if we would follow a research line in which we try to exclude a *work domain* (in the sense of [7]), we will still not be able to exclude a *context of use* at all (in this very general sense). For example, in the case of research on pattern recognition of facial expressions, we have to assume a possible context of use although it is implicitly given. In one of the following sections about the user's *validation cycle* we will return to this issue.

IV. How to Get a Scientific Language?

Kuhn [70] differentiated between two phases in the development of an academic discipline: before and after reaching consensus. Reaching a consensus phase can take a long time (i.e., between decades up to centuries). The consensus phase is, for example, characterized by a common content in different textbooks and handbooks providing successful examples from which students can learn. Green, Davies and Gilmore [60] stated very clearly that the aim of establishing a common research line for HCI "is only feasible if a *common language* can be developed, in which relevant aspects of both the person and the system can be expressed" (p. 99). Rauterberg and Szabo [71] made a first constructive attempt to conceptualise and compare different perceptual effects (e.g., visual and auditory modality on the human side) with different technical options to produce particular perceptual impressions. Since 2003 at least one HCI glossary[4] has been available online, in which all given definitions of HCI relevant terms and concepts are provide that could be extracted from existing ISO standards [72].

A. What is a Scientific Language?

A coherent and powerful technical language based on consensus is a necessary precondition for any progress in an academic discipline [60]. Up until now, the HCI community has no well-established *corpus of descriptors*. For example, the important concept *interaction style* introduced by Shneiderman [73] is translated into an *interactive style* [57], into a *dialogue style* [74] and into a *dialogue technique* [16] (referring to ISO 9241). Neither the terms 'interaction style', 'interactive style' nor 'dialogue style' can be found in the keyword index of [75] or [76]. In Jacko and Sears [77] at least the term 'dialogue style' made it into the subject index, but not into the text. Only Baecker and Buxton [57] distinguish between nine major categories of *interaction styles*: (1) command line, (2) programming language, (3) natural language, (4) menu, (5) form filling, (6) iconic, (7) window, (8) direct manipulation, and (9) graphical interaction (p. 427). They conclude that "more effort needs to be expended on developing a taxonomy of the content of human-computer interaction" (p. 434).

Vet and Ruyter [78] developed a *concept of interaction styles* that decomposes an interaction style into three components: (1) conceptual operations, (2) interaction structure, and (3) interaction techniques. "An *interaction style* is thus defined as the execution of a conceptual operation within an interaction structure using an interaction technique" (p. 8). To be able to compare the published empirical results with the strengths and weaknesses of different technologies, a special *notation language* was proposed [71]. Only with such kind of notation language, the results of published experiments can be compared and discussed to achieve valuable conclusions for further developments.

Very successful and therefore prominent examples of such kind of a *scientific language* are all formal notations in different

[4] http://id00156.id.tue.nl/hci/

engineering communities. These formal notations based on mathematics can be seen as probably the only truly international agreed upon academic language. These types of languages are highly attractive, and sometimes are claimed to be a necessary requirement for an *academic* quality standard. Several researchers in HCI and related fields already moved in this direction [79], [80], [81], [82], [83], [84], and [54]. But there is a major pitfall or even shortcoming with formal notations: "by their very nature, mathematical metaphors can only be applied to a narrow range of problems" ([85], p. 589). Gupta [85] discusses several reasons why an academic language based on mathematics may not always, or even cannot always provide appropriate insight into a complex reality ("the widespread misappropriation of the language of mathematics in the social … sciences has to be one of the great tragedies of our time", p. 589). One of the main challenges for a research line in HCI is to figure out where (and why) formal notations are appropriate and where (and why) they are not [86]. For example, far more than any formal approach, the book of Suchman [64] has been influential on the HCI community ([87], p. 615). We do *not* wish to express or transform everything into a formal language, but we must do as much as possible, taking into account that many important ideas and relevant design knowledge can only be expressed in a *non*-formal language.

B. How to Obtain Consensus

Habermas differentiates four types of speech acts [88]: (1) *communicativa* imply the freedom for an expressed opinion itself and the freedom to express one's own opinion (everyone is allowed to take part in a communication); (2) *representativa* imply the semantics of the expressed statement and the possible subjective bias in it; (3) *constitutiva* cover the objective truth in the statement; and (4) *regulativa* enable the expression of normative aspects. Agreement, according to Habermas, can be reached via truthful expressions in a power-free communication (in German 'machtfreier Diskurs'). A *truthful* expression in a speech act is characterized by all involved parties being able to reach a potential agreement, based on their rational reason. *Rational reason* is defined as knowledge about a possible way to justify the *truth* in an objective manner, during the speech act itself ('veracity') and beyond in daily practice ('credibility'). To achieve consensus, it is important that all involved parties share and accept a similar way to *describe* and to *justify* the 'truth'. This means having at least consensus about a *validation* methodology. How to establish validation into a HCI research line (on different levels) is described and discussed in the following section.

V. Coherent Research Line

A. Does HCI Need a Coherent Framework?

We argue in this paper for a *research line*, not for a particular *framework*. What is a framework? Most introduced and described frameworks try to conceptualise one or more domains with a set of relevant dimensions. Green [80] considered a *space* in which to locate many different kinds of notations. Floyd [89] discussed an evolutionary approach for software development as a *reality construction*. Kuutti and Bannon [90] promoted *activity theory* as a unifying concept to integrate the different perspectives. Cugola *et al.* [83] discussed a framework for formalizing inconsistencies and deviations in human-centered design. Furnas [5] introduced MoRAS as a framework similar to the approach of Rasmussen, Pejtersen and Goodstein [91]. Olson and Olson [92] described a framework for *collaboration technology* with four key concepts: (1) *common ground*, (2) *coupling of work*, (3) *collaboration readiness*, and (4) *collaboration technology readiness*. De Souza *et al.* [93] promoted a semiotic approach to HCI research. For the field of *information system research* Bacon and Fitzgerald [94] identified a *need* for a systematic framework and proposed one. Ng [87] introduced a theoretical framework for understanding the relationship between *situated action* and *planned action* models in the *information retrieval contexts*. Many HCI scholars still believe in frameworks. But to which extent are these frameworks really helpful in unifying the HCI community?

B. Is a Coherent Framework Achievable?

Harris and Henderson [95] expressed their concerns regarding *generic* frameworks: “We begin by recognizing that no attempt to fit the world into a neat set of categories can succeed for long. We will always encounter inconsistent, ambiguous, messy bits that don’t fit. Standard system design which depends on making the world fit a neat set of categories will naturally have trouble” (p. 94). Given this quote, what can be done about this, and what are the main reasons for questioning the feasibility of *generic* frameworks as candidates for improving the maturity of the research community? To be clear, we do not argue against frameworks as such (whether they are specific or generic), but is focussing on one or at least only a few frameworks a good way to go? Rozanski and Haake [96] conclude that there may be too many *facets of HCI* that make a unified framework hard or even impossible to achieve. Floyd [89] and Santos, Kiris and Coyle [97] stress the fact that system design takes place in a changing environment; it changes often rapidly because of a very fast technology push. Given all these constraints we have to conclude that a unifying framework is very difficult to establish, if at all. But, we can strive for a unifying *research line*.

C. Triangulation

This section discusses the most relevant aspects of a possible research line for HCI on a high conceptual level. Inspired by the maturity model of Humphrey [98], and to start with we try to introduce a similar view. The major levels a new design discipline might go through to become mature are the (1) *initial phase*, (2) *repeatable design processes*, (3) *defined research line*, (4) *managed research activities*, and (5) *optimized theory development*. At the top level (5) a cyclic structure for self-optimizing knowledge development should be established.

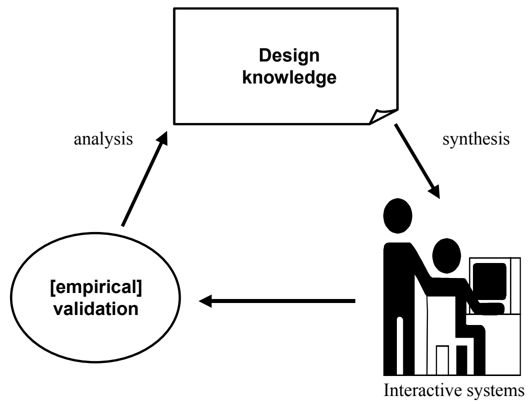

Fig. 3. The academic validation cycle; triangulation for an academic research approach with a rigorous validation component (adopted from Greenberg [33]; see also Wickens *et al.* [100], p. 387f).

To combine the *analytical* strength of empirical validation methods (e.g., observation, experiment, inquiry, etc.) with the *synthetic* strength of system design, Mackay and Fayard [99] introduced the triangle structure presented in Fig. 3 (see also Greenberg [33], and Wickens *et al.* [100]). This triangle structure conceptualises the three most important components of HCI research: (1) the collection of *design knowledge*, (2) the *interactive system* in different possible representation forms, and (3) the various options- for *usability testing* and [empirical] validation. This triangle structure is similar to the circular model of Henderson [101] in which the following steps are differentiated: (1) *design* (“creating improvements in the activity”); (2) *implementation* (“bringing the designs to life”); (3) *use* (“people’s work that is to be improved”); (4) *observation* (“encountering and capturing the activity”); and (5) *analysis* (“understanding the regularities in the activity”) (p. 262).

1. Design Knowledge

The development and collection of *design knowledge* is the primary goal of the whole research line. This validated knowledge with high predictive power should be formulated in design theories based on high-level design principles (e.g., Gram and Cockton [102]), medium-level guidelines (e.g., Mayhew [103]), and low-level implementation techniques (e.g., *metrics* according to Rauterberg [84]). Sutcliffe [25] proposes "that HCI knowledge should be theory-grounded, and development of reusable 'designer-digestible' packets will be an important contribution in the future" (p. 197). Finkelstein and Kramer [17] very clearly summarize "that we cannot expect industry to make very large big-bang changes to processes, methods and tools, at any rate without substantial evidence of the value derivable from those changes. This, accompanied again by the increased disciplinary maturity, has led to a higher 'validity' barrier which research contributions must cross. It is readily observable, that research that proposes new frameworks, methods and processes are not accepted without positive evidence that they are of use rather than simply airy and unfounded speculation" (p. 4). We fully agree with this important conclusion.

2. Interactive System

Design knowledge with sufficient predictive power enables the design expert to apply this knowledge to a concrete system design with a guaranteed outcome. In the most powerful form this design knowledge enables the designer to *calculate* the intended system characteristics in advance [49], [62], [84]. All design knowledge can be given away in the form of design theories, written down in books and articles, shown in videos, taught in education and training, etc., and sometimes demonstrated in the form of concrete artefacts as well [104]. The crucial part is taking the step from requirements to specifications before implementing. One of the promising methods for this step is MUSE [53]. So far only HCI academics with an engineering mind set are willing to invest in such kind of structured approaches [105], [106]. It seems to be the case that the introduction of a structured method like MUSE is too early, based on the fact that not enough design knowledge about the user is available in a particular context of use. Sutcliffe and McDermott [107] and Sutcliffe and Wang [108] introduced other approaches to incorporating HCI with software engineering methodologies. However, HCI as an engineering discipline will need such *structured design methods* sooner or later.

3. Empirical Validation

To validate a proposed design knowledge (e.g., design principles), *empirical* research methods are necessary. But first an abstract design principle for a particular type of *design class* has to be instantiated via concrete artefacts $\{S_1 \dots S_n\}$ before it can be tested. To make proper use of the full range of empirical research methods, proper training in these methods is required. Unfortunately, based on a lack of this kind of profound expertise, *design* oriented HCI researchers are starting to complain about the "tyranny of evaluation" [109]. So far, only educational programs in social sciences have provided this kind of training in empirical research methods. For non-social scientists several good textbooks are already available [110], [111], [112], [113]. Throughout a thorough empirical validation activity, the shortcomings of a particular design instantiation can be discovered, and can often be directly turned into a solution ([114], 105ff), [115].

D. Industrial Relationship

The research field of HCI has raised much attention and interest from industry. "The human-computer interface is critical to the success of products in the market place..." ([116], p. 794). ICT companies have a growing need for ICT professionals with an increasing expertise in HCI [23], [117]. Industry is looking for highly skilled *interaction designers* who can contribute to commercial success based on their profound design expertise (Norman in [27]). Moreover, industry is mainly interested in utilizing design knowledge that can directly lead to successful product design (see Fig. 4, commercial optimization cycle). It is a plausible, but still is an insufficiently proven assumption, that usability *immediately* contributes to commercial success [118]. Bias and Mayhew [119] present and discuss several projects in which a cost-justifying usability approach was successfully applied. They collected and discussed a couple of *cost-justifying* arguments to convince project managers in industry to invest in usability engineering activities. The commercial optimization cycle (see Fig. 4) is primarily money driven, while the academic validation cycle should deliver design knowledge of high quality.

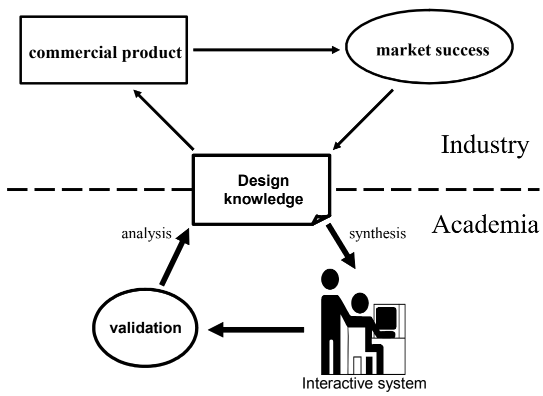

Fig. 4. Linking the academic validation cycle (below) to the commercial optimization cycle (above).

Given the high pressure and urgent demands from industry, the whole HCI field has primarily offered and delivered 'usability testing methods', instead of 'validated design knowledge' including a structured design methodology. 'Discount usability' and 'usability testing' seems to be an outsourcing strategy for selling scientific validation process methodology, instead of developing and delivering the desired design knowledge. This statement is maybe over-critical, but it points to the core of the problem. Up to now, books have contained many relevant, design-related ideas, hints and tips, sometimes called guidelines (e.g., [74]) or even design principles (e.g., [102]). However, what is still missing is a basis for a corpus of design knowledge that contains thorough empirically validated results. As Gaines [30] points out, "I will conclude that we are still at a very early stage in the development of HCI that the major impact of the technology on society is yet to come and that to understand the design issues involved we will need much greater overt understanding..." (p. 19). Industry would perceive the outcome of the academic HCI research cycle as sufficiently mature if HCI research would primarily operate on an engineering paradigm.

E. 'System' Validation Cycle

One of the open questions is the appropriate substitute of a *real system* with *something* else that is much faster to create, but still retains the most relevant features for further validation. Hix and Hartson [120] emphasize a two-step approach: (1) *conceptual design* and (2) [initial] *scenario design*. "*Conceptual design* is higher level and has to do with synthesizing objects and operations. *Detailed design* has to do with activities such as determining the wording of messages, labels, and menu choices, as well as the appearance of objects on the screen, navigation among screens, and much more" (p. 132). A scenario design can be worked out in the form of a set of screens, story boards, or even video clips [121].

Particular research questions can only be investigated if a complete interactive system is available. Fortunately, a lot of relevant questions can be already answered with *lighter* substitutes than the real system (e.g., prototype, 'Wizard of Oz' simulation, formal specification, concept; see Fig. 5). However, these substitutes can only replace the real system if they are – in general – fully validated beforehand, and all their methodological constraints are well investigated and known. If we have to rely on *cheap* replacements or *light* substitutes, then we have to make sure that the results gathered with these substitutes are not biased, at least not *uncontrollably* biased with the chance for proper corrections afterwards. Very little has been done so far to validate these substitutes compared to real systems (a positive exception is [122]). This issue is a very important, but highly underestimated, research contribution [123], [124]. This research contribution will lead to a properly validated design methodology beyond Lim and Long [53].

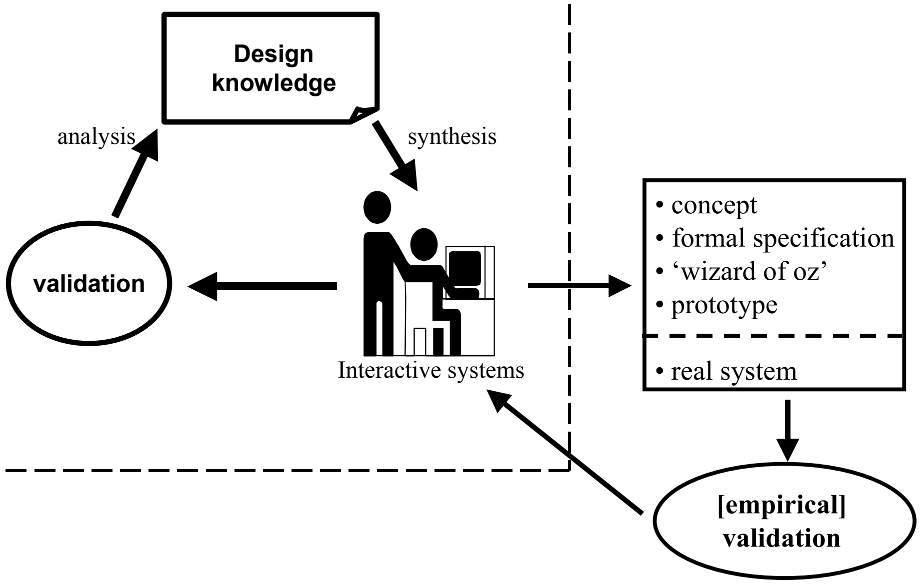

Fig. 5. System validation cycle with different ways to substitute a *real* system with a *lighter* replacement.

F. 'User' Validation Cycle

Up until now, the HCI research arena has demonstrated a strong eclecticism in its approach to methodology. Methods from psychology, social sciences, ethnography, etc. have been adopted to solve some of the immediate problems without taking into account the ontological consequences. Books and articles about a particular *HCI methodology* give an introduction and overview about possible methodological adaptations [125], [126], [127], [128].

The preference for *empirical* validation methods (compared to *formal* validation methods) is based on the fact that user's behaviour, confronted with a new system, is very difficult to predict. If a *user* substitute (e.g., user model, user simulation, etc.; see Fig. 6) instead of a representative sample of real end users is used for validation, these substitutes have to be validated beforehand as well. These validated *user* dummies are already partially delivered by the following research contributions: cognitive and mental modelling [129], human factors [130], artificial intelligence [131] and humanoid robotics [132]. Research in this direction led to tools like AMME [133], HOMER [134], or IMPRINT [135].

We entirely agree with Landauer [128] that the professional use "of good research methods is a pressing and immediate practical concern, not just a step toward a firmer scientific base" (p. 204). Monk [126] differentiates between the following four polarities: (1) 'naturalistic observation' versus 'rigorous experiments', (2) 'field' versus 'laboratory research', (3) 'scientist as participants' versus 'scientist as observer', and (4) 'few' versus 'many test subjects'. One important point he makes, is "the importance of developing *predictive models and theories* which can suggest and explain empirical results" (p. 136). As Long [136] put it: "Greater effectiveness of interactions, practices and research will derive from the validation of new HCI knowledge, and the specification of relations between HCI research and the design of human-computer interactions, such that research is 'fit-for-design-purpose'" (p. 241). Only with a rigorous validation methodology will the development of design knowledge lead to stable theories with sufficient predictive power for the design of new interactive systems. These high quality design theories will enable interaction designers to specify the intended system characteristics beforehand [137].

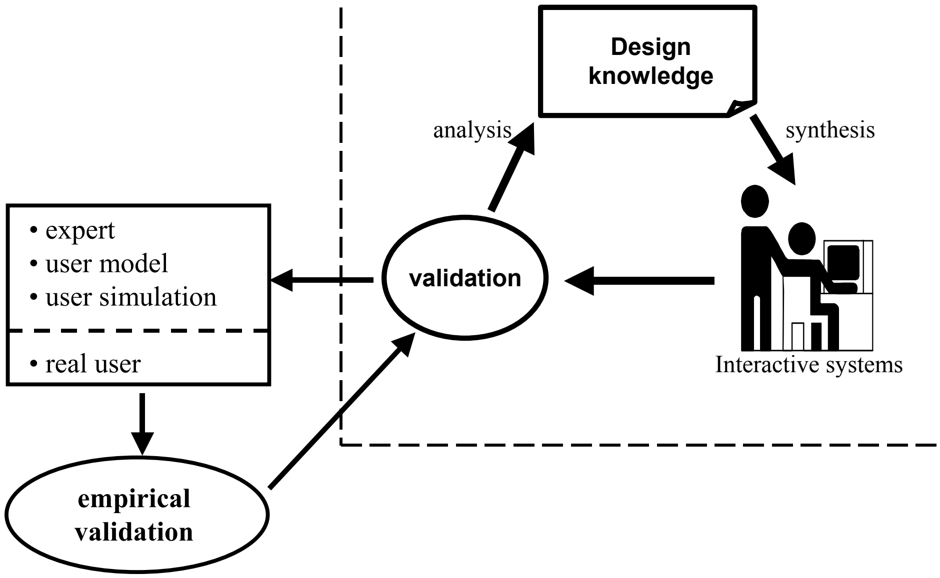

Fig. 6. Empirical validation cycle for different ways to substitute real users with 'user' dummies.

VI. Conclusions

Evaluating the role of theory in HCI (and taking design seriously) means evaluating the usefulness and usability of applying a theory to the design of interactive artefacts. "A deeper understanding of how representations are created and how they contribute to the solution of problems will become an essential component in the future theory of design" ([43], p. 78). Hence, what does HCI really need to have a serious chance of becoming a mature engineering discipline? In addition to Long [138], here are some very basic answers:

- A new theoretical focus to investigate the *interaction space* based on specified problems: The interaction space between a human and a technical artefact is difficult to conceptualise, and it is difficult to find the proper set of parameters. On the one hand, we have to deal with the human being, primarily described and specified in qualitative dimensions, and on the other, we have to design a technical artefact, described and specified with quantitative parameters.
- A *coherent taxonomy* with a powerful corpus of descriptors and terminology: In developing a coherent taxonomy includes the development of a coherent theory as well, and vice versa. A scientific terminology without a theoretical context is neither possible nor desirable.
- A *rigorous validation* method to prove the design knowledge to achieve progress (see Fig. 3): The academic community of HCI would benefit from agreeing upon an objective and rigorous manner of validation; the 'wild' growth of unvalidated statements could converge to a couple of stable theoretical nuclei. One necessary pre-condition seems to be the specification of 'relevant' problems in relation with the state of the art (documented via publications, and mainly via patents for technical artefacts).

The needs of HCI are growing as the power and complexity of interactive systems continue to grow, and "we will be unwise to neglect any approach to meeting them" ([139], p. 160). If the HCI research area wants to survive as a scientific discipline, at least the following three conditions have to be fulfilled (to move from the *explorative* to the *paradigmatic phase*):

- The specification of most relevant elements ('research objects'; including 'problem definitions').
- The development of a coherent scientific language (for achieving *consensus*).
- Establishing a research line to develop design knowledge in a validated manner with predictive power (based on triangulation).

"A number of dramatic human-computer interaction design successes, ..., have already occurred as a direct result of systematic research − as contrasted with intelligent creativity alone" ([128], p. 224). On the one hand, 'systematic research' and, on the other, 'intelligent creativity alone' seems to be *contradictory*, but we have argued that they are *complementary*. Historically speaking, HCI research and development has been 'spectacularly' successful, and has indeed fundamentally changed interactive computing [116]. It is important to appreciate that decades of research are involved in creating and making interactive technologies ready for widespread use. Using methods researched and validated in other scientific fields allows HCI to move quickly to robust, valid results that are applicable to the more applied area of design. But to improve the maturity of HCI research and to guarantee the long-term survival of this research field, we have to do more. Let us summarize the two major messages of this paper: (1) HCI research should move from 'art' to 'science' and (2) from 'evaluation' to 'calculation'. Based on recent results of Bartneck and Rauterberg [4], the engineering discipline should take the lead to bridge the gap between artistic design and science. If HCI research can operate on a sound scientific engineering paradigm, we can successfully move into this direction.

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