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## Research in Child-Computer Interaction: Provocations and envisioning future directions

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## ABSTRACT

In the 21st century the academic field of Child-Computer Interaction (CCI) arose alongside burgeoning interactive technology and digital media industries that targeted children. We believe that the field of CCIU is at an important point in its development, analogous to when a child becomes a teen. Over the last few years we have each had many informal conversations with other CCI researchers in which we discuss issues such as, what is our responsibility as researchers beyond academe? What values underlie our conceptions of a "good" childhood and the role of interactive technology in it? And, how do we ensure that our field continues to grow and evolve in ways that are consistent with our responsibilities and values? To address these and other complex questions that have been drawing our attention we came together to reflect, discuss and create a position paper for our community, in which we outline some of the issues we see facing our community at this time. To inform our deliberations with opinions beyond our own we conducted an informal consultation with 25 members of the CCI community. Our responders spanned junior to senior researchers, represented diverse geographies and included industry practitioners. These diverse responses provided further content for our reflections, and helped us see perspectives beyond our own. The result of this informal process is this speculative paper in which we propose a series of seven provocations that aim to disrupt some of the normative assumptions held in our field. Our goal in doing this is to open up dialogue in our community about these issues and promote consideration of the alternative visions we present for where we might focus our attention and efforts. We see our contribution not as truth or a definitive statement of a vision for the field, but rather as our opinion about some of the complex issues we face and that we think should be considered through dialogue as we move into the next phase of our development as an academic and scholarly community. We believe that it is urgent and critical for our field that we take up these questions, explore diverse perspectives, and critically work towards decisions and actions that will define our identity and the value of our contributions as we move forward into the next 20 years of research in CCI.

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## Contents

1. Introduction.....	2
2. Purpose .....	2
3. Process .....	2
4. Seven provocations and visions for the field.....	3
4.1. Why we think we do CCI research: Assumptions and realities of research prototypes .....	3
4.2. Knowledge translation: Broken promises and innovative possibilities.....	4
4.3. The allure and misconceptions about the role of theory in CCI research.....	6
4.4. Addressing rigour and measurement in an interdisciplinary field.....	8
4.5. Challenging participatory design benefits and ways to leverage PD methods moving forward .....	9
4.6. The problem with how we "do ethics" and how to reconceptualize what is needed .....	10
4.7. The call for inclusivity, diversity and social justice: Doing it right .....	11
5. Conclusion .....	11

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Declaration of competing interest.....	12
Acknowledgements .....	12
Appendix. CCI vision questionnaire .....	12
References .....	13

## 1. Introduction

The field of Child-Computer Interaction (CCI) has reached adolescence. Over the last 20 years or so of our careers we<sup>1</sup> have noticed that the field has matured from a young and largely technology-driven field, characterized by exploratory research such as proof-of-concepts and case studies, to one that has settled into well established structures and methodologies such as more theory-driven approaches to research, attempts to evaluate with rigorous lab and field studies, the development of more functionally complex systems, advances in participatory and co-design methods for including children in research, attention to ethical issues, and consideration of achieving impact beyond academe. As technology reaches into every nook and cranny of children's lives, the field is stretching to accommodate this breadth, and in doing so welcomes new researchers from diverse fields. As we move forward, there are concerns about the loss of our foundations in child development; about the value of studies dominated by novelty effects over long term impact; the duration and impact of our participatory work with and for children; and questions about overall progress – how are children beyond our labs benefiting from academic research? Are we impacting learning materials, toys, other forms of entertainment and social media for children beyond our labs? Are we making a difference?

We believe that we are at a critical point in the development of the field. As new researchers join our ranks and some of our founders retire, the time is ripe to take a step back and look at our accomplishments and turn our conscious attention to the actions and decisions we now take that determine where our field will go. Towards this aim, we planned a special interest group (SIG) (Antle et al., 2020), which was cancelled due to Covid-19. The first two authors of the SIG decided to continue support the continuation of this reflexive work and created this opinion paper. In this paper, we put forward a set of provocations and a nascent vision for the next 20 years of research in CCI. Rather than laying out a specific agenda, we put forward this paper as a way to initiate dialogue in our community. We outline key thematic areas we see as critical to the next years of research in CCI, we summarize these themes as “provocations” (conjectural statements designed to elicit discourse), followed by alternative visions for future directions that we think might be valuable to pursue in light of our provocations. The themes are not meant to be exclusive. Our themes cover a range of research activities and target a broad swath of roles involved in CCI research activities in order to reach a broad audience. In presenting our reflections and opinions in this paper, we aim to disrupt some of the normative assumptions we think many of us hold, and also create awareness of the roles we as researchers play in determining the impact the field of CCI may have on children's lives more broadly.

## 2. Purpose

This project was initiated by myself (first author), in response to taking stock of my career in children's digital media; contemplating the value of my past contributions to the field of CCI and

envisioning contributions I might be able to make moving forward. Together with the second author, I decided to open up this reflective and envisioning process to garner opinions of others in CCI around the same kinds of questions. The final opinions in this paper are ours (authors) and while they may have been informed by interactions with others, they are not meant to be reflective of any others' opinions per se. The purpose of this project differs from other, related review efforts in several important ways. First, systematic reviews look retrospectively at what has been done in the field (e.g. Antle & Wise, 2013; Börjesson, Barendregt, Eriksson, & Torgersson, 2015; Giannakos, Papamitsiou, Markopoulos, Read, & Hourcade, 2020; Hourcade, 2015; Jensen & Skov, 2005; Kavas et al., 2020; Mechelen, Baykal, Dindler, Eriksson, & Iversen, 2020; Read & Bekker, 2011). As such they provide reflections on established trajectories of research, but their primary aim is not to provide new directions or challenge the status quo. This project, while grounded in retrospective experiences, sought to be forward-looking, provocative, and perhaps even disruptive or controversial as a means to initiate dialogue that includes diverse perspectives within the CCI community. Second, systematic reviews summarize published literature that represents a knowledge base that has been constructed jointly by a community of researchers. What is published is not the same as what we might have wished to publish, what we thought was important about our methods or findings, or even what we might have wished to research in the first place had we been able. Our reflections and the questionnaires we deployed to others to inform our reflections focused on eliciting thoughts and opinions of individuals based not on published works, but on our/their experiences and opinions formulated as members of our research community. And lastly, systematic reviews constitute research that aims to objectively characterize the literature. However, in our project we embraced subjective experiences about our research in the context in which it is both conducted and embedded more broadly. As such we make no claims about objectivity or rigour and do not position this work as scientific or empirical. It is a piece of normative writing designed to engage our community intellectually and responsibly in dialogue and debate as a means to proactively and collaboratively shape our future.

## 3. Process

This project began as I was reflecting on current and possible future visions for the field of CCI across a range of research activities and topics. These activities and topics were developed into themes for a proposal for a special interest group (SIG) (Antle et al., 2020). After the SIG was cancelled due to COVID-19, I proposed to the second author that we work together to continue reflecting on these themes in order to create a position paper. Fortunately, he said yes! As part of this process we re-read many of CCI review papers to remind ourselves of the work others had done taking stock of research and prior research agendas that had been proposed. We also sent out questionnaires based on our themes in Antle and Frauenberger (2020) over email to 50 participants. We invited researchers and practitioners, both senior and junior, from different cultures and geographies, with representation from industry, who we felt had made significant contributions to CCI through their work. As part of our consent process, we promised respondents anonymity, hoping this might encourage them to voice opinions privately that they

<sup>1</sup> In this paper we use the first person voice of the two authors, using “I” or “my” to reflect statements that are specific to the first author, “second author” to refer to statements specific to the second author and “we” or “our” for joint statements.

might not be comfortable doing publicly. Our response rate during Covid-19 was 50%, which we believe indicates interest in and support for this project in our community. The questions introduced 6 themes and asked questions designed to probe participants' thoughts and opinions. We assigned each participant 2 to 3 themes and asked them to write a paragraph about their thoughts on the different thematic areas. The themes included: the perceived value and reach of research in CCI and the experience of working in an interdisciplinary field; the uses of theory; perspectives on methodological rigour; expectations and realities around impact and knowledge translation; and pressing ethical issues and assumptions about the nature of a "good" childhood. In the sixth theme we laid out 4 emerging application areas: children and nature/sustainability; children, big data and surveillance; artificial intelligence (AI); and families and mental health. We asked what participants thought were pressing questions and challenges to conducting research in those areas. We do not report on this response data directly nor attempt to summarize it, but instead use it to inform our reflections and dialogue together.

Once we had received participants' responses, we worked individually and met jointly to discuss our developing thoughts about our own and others' ideas. We reflected on normative assumptions, unique perspectives, responses that expanded on some of our own concerns and forward-looking ideas. As such we interpreted questionnaire responses not objectively, but through the lenses of our own experiences and opinions as researchers who have been foundational to CCI (i.e. have published and been cited extensively in the field since we were junior researchers). We created clusters of ideas where we thought the field was not meeting often-stated goals; where we wanted to disrupt normative assumptions; and where we wanted to point out disconnections between means and ends. We clustered these into seven themes, some similar to our prior themes, some new. For each of the seven themes, we also envisioned and/or elucidated on others' ideas for examples of ways of addressing these challenges moving forward. Some of these ideas represent a 180° turn on normative assumptions or previous approaches. Where we were familiar with work that demonstrated these novel approaches, we cite them. When we held disparate opinions, we noted that these often represented different perspectives or facets of a theme and so we include both of our ideas or suggestions for alternatives for these cases.

In sum, we present our opinions by theme, in the form of provocative deconstructions alongside future visions. Our aim is to open up a dialogue in our community about where we have been, and to provide some future visions about where we might go. A fundamental assumption of our work here is the acknowledgement of the role we as a community have in creating any or all of these futures. We do not propose that the opinions presented here form an exhaustive list of issues we think our community must tackle moving forward, but it is a start and we encourage you to engage in your own reflections and dialogue.

#### 4. Seven provocations and visions for the field

##### 4.1. Why we think we do CCI research: Assumptions and realities of research prototypes

This is an exciting time to conduct CCI research from an intellectual perspective. As technology becomes ubiquitous in children's lives, a deeper understanding of children's relationships with technology and how these can be designed to positively influence children's development is an intellectually challenging and potentially high-impact research area. The more difficult question is how to move from the intellectual appeal to benefiting children's lives.

As a junior researcher, fresh from and disillusioned with the children's media industry, I (the first author) assumed that my role as a researcher in the field of CCI would be to create interactive technologies that were thoroughly grounded in theories of child development (vs the technology or novelty driven systems I had worked on in industry) in order to create effective research prototypes that could eventually be commercialized and/or distributed into products (through my own efforts or through partnerships with companies, agencies and the like who would utilize my work). An assumption was that the end game was to impact the creation of products (and services) that would be widely available to schools, parents, or children and as a result improve children's lives. Perhaps this view was naïve and certainly it is not shared by all researchers in CCI. However, in our questionnaire results and in many discussions we (both authors) have had over the years with other researchers – especially junior ones – we have seen that this assumption is commonly held – and that a primary reason that we do CCI research is to create research prototypes that will eventually become the interactive technologies that multitudes of children use. In this way, we think we do research to help create a better future for children.

Let us look at reality. It is extremely rare for a successful commercial product or even a free but widely available product for children to emerge from academe. Why is this? There may be many reasons for this, some of which differentiate CCI from the larger field of human-computer interaction (HCI). For example, for a research prototype to make the move to a commercial product there needs to be a viable business model where it is clear what problem the product is solving, and who will pay to have that problem solved. In the realm of interactive technologies for children, it is often unclear who might want to pay to prevent or improve outcomes for children in terms of a range of areas that we work with including but not limited to: support for learning disabilities, language literacy, technological literacy, social emotional learning, mental health, communication skills, and collaboration skills. Take mental health for example: worldwide, or country by country, or even province by province (state by state), most governments and mental health organizations do not take a preventative approach to mental health in general, and in particular not with children. And even if they did it is unclear how best to reach children – through clinics, schools or parents or through some other means? Many countries also lack processes that would support decision making around uptake of interactive technologies for mental health education based on academic research. Additionally, many countries do not have unified decision-making processes around technology purchasing. For example, in Canada there is no federal department that makes choices about interactive technology in schools. The decision about which products will be used in schools is made either at an individual school level or at a school board level, thus representing a huge barrier in terms of sales and marketing a product into schools – which has to proceed one by one and thus is not viable.

Rather than looking for a large institution which has the means to pay for the product en masse, an alternative approach involves working with a distribution partner to develop a minimal viable product from a research prototype. However in our experience, there is a Catch 22 here. It is not always clear from the beginning if a research prototype is going to be effective. Most industry partners are only interested in products that have been shown to be effective. And yet many partners prefer to work closely with researchers from the beginning of a project rather than coming at the end, when the intellectual property has been largely developed. These constraints make any kind of innovation almost impossible. Layered onto these challenges is the lack of general understanding among researchers about how to create and work

with partner organizations while still adhering to scholarly expectations and workloads. For example, it is time consuming to build the kinds of academic-industry-other partnerships needed to move from research to commercialization. Tenure clocks and grant criteria often require results faster than the years it may take to develop fruitful relationships.

Another obstacle to widespread adoption of a product created from research is that many of the interactive technologies we research involve novel hardware devices which are not necessarily widely used by the general public. For example, the entire field of tangible user interfaces is not one which could be easily scaled and made accessible to the general public without the requirement for them to purchase specialized hardware. The platforms with the greatest public uptake are smart devices and tablets and yet much of the research in CCI does not involve apps (which still have to be developed with a host of additional considerations to be released on iTunes or Google Play). And lastly, a major and significant obstacle is the fact that a researcher who decides to try and take a product to market either commercially or through free distribution is likely not knowledgeable about the requirements of such a process – there is a significant learning curve – and even with university support for such endeavours, the researcher must somehow juggle what is a long term and massive undertaking with continuance of all of their responsibilities as a researcher. We have each had our own experiences with trying to turn research prototypes into widely available products and can attest to the monumental task of trying to juggle these two objectives simultaneously. Thus, we argue that the conditions are rarely right to take a research prototype along the path to widespread adoption. Yet the assumption that we do our research to get “better” products into children’s hands is a normative and foundational assumption in our field.

We have seen exceptions where research prototypes have been turned into commercial products or those made widely available. For example, in the USA the Scratch programming environment, backed by the visibility, reputation, and financial might of MIT, has seen ongoing and broad uptake. In the UK, the BBC micro: bit project led by researchers at UCL, has been widely distributed into British schools as a tool to learn programming. Both of these cases also match major government initiatives to bring programming to schools. There are other examples, but we argue these are the exception.

*Provocation:* Research involving the creation of prototypes rarely results directly (or indirectly) in “better” products for children, and thus should not be the primary reason we do research in CCI.

*Envisioning a Future:* What then are the reasons we make research prototypes? How can this research add value to children’s lives? The reality of the challenges with taking research to market leads us to propose a reconceptualization of this goal of our research.

We propose that there are alternative ways that the creation of research prototypes adds value. One way our research creating prototypes may add value is to push the envelope for technologies that are not easily developed commercially. Some products will never be created commercially because it is not commercially viable to do so. For example, products targeted at small or difficult to reach populations, such as children with disabilities, health conditions, and those that are neurologically diverse. Yet it is possible to develop, evaluate and distribute research products into some of these under-served communities through our research institutions and community. This is particularly true for software accessible through widely available technologies (e.g. web, apps). For example, the second author freely distributes software intended for children diagnosed with autism, which is downloaded hundreds of times a year by people from all over the world, with no advertising involved.

Another way our research can add value is when we are able to conduct research that is too innovative, too novel, or risky for industry. Perhaps it is unclear if outcomes would be positive, especially in the short window of time that most businesses look at for product development and delivery. Perhaps there is a need to better understand ethical or socio-technological impacts of specific technologies. While the prototypes we create may never be taken up by industry or widely adopted by children, we can create exemplars of specific interactive technologies that we, as a field, think are worth pursuing. Either because they may have substantial benefits or because they enable us to ask ethical-social questions we think are important. For example, if we can show evidence of benefit through rigorous evaluations (see theme 3 below), then we can advocate for innovations that may be risky, novel or that others simply have not envisioned coming from a more economic world-view. Conversely, if we show possible harm, we can then advocate for care and reconsideration of further development. We address advocacy further in theme 2 below.

And lastly, our prototypes serve as concrete communication tools, not only for what technologies we think should, or should not be pursued but also for the values they convey both within and beyond academe. A widely held value in CCI is that we want to shape a positive future for children. To achieve this, we must think hard about the research questions we ask, because the technologies we design and build demonstrate the uses of interactive technologies that we think are valuable. Our prototypes reflect what we want the future to look like for the kinds of children we work with. It behooves us to ask hard questions not just about the future we want for children, but what kinds of tools these children will need to address looming political, social, and environment issues such as how to create more democratic societies, how to address the digital divide and marginalization, how to promote social justice and a fairer society, and how to address climate change. As such, our research can provide computational alternatives (see Yoo et al., 2020) to widely available commercial technologies, enabling discussion and exemplars for a variety of options and visions that generate higher-quality critical assessments of existing technologies, and as a result may better serve children.

#### 4.2. Knowledge translation: Broken promises and innovative possibilities

One challenge for any scientific field is translating its findings into practice so they can impact millions of people instead of just a few participants in a research study. Knowledge translation (known as KT) can be defined as an approach to conducting scientific research in order to make the results applicable to the target population.<sup>2</sup> In the broader field of HCI, the goal of KT is to facilitate the uptake of theoretical findings from research into design practices (Colusso, Jones, Munson, & Hsieh, 2019), although we would broaden this definition to include scientific knowledge as well as technical innovation that can be used to inform not just design practice and resulting products, but also the development of services, training, policy and public dialogue. KT may impact industry practitioners but we suggest it is also valuable to pursue with organizations related to children including government, non-government and interactional agencies, not-for-profits, other funding agencies and the public at large. The KT process involves both research related activities (which may be basic, applied, or clinical research) and non-research related activities (dissemination, implementation, design) (Colusso et al.,

<sup>2</sup> See the ACM Interactions Sept–Oct 2020 article, “HCI and UX as Translational Research” for a more thorough discussion of KT.



2019). In CCI like HCI, a second normative assumption is that part of the value of our CCI research is the promise that the outputs of our scholarship (e.g. research findings, design frameworks, ethical guidance) will somehow be translated into forms that impact those designing and implementing interactive technologies, services and policies related to children. That is, as a community, we believe we can make a difference in the world of children through KT of our research.

However, it is rare to find instances of findings or recommendations from our empirical results impacting the development of products, services, or policies with broad impacts. In part, this lack of evidence may be a result of the lack of established tools that we can use to measure our short-term, long-term or cumulative impacts through KT. Currently we do not know how to measure individual or cumulative KT against our goals of impact. Another challenge is the lack of widespread knowledge about the processes that are required to activate the outputs of scholarship that might enable this translation to occur in the first place. There are numerous obstacles to KT. For example, writing for scientific or design publications is vastly different from creating accessible communication targeted to industry (Kawas et al., 2021) or even consumers. Where might we get this training? How do we circulate our white papers and reports to ensure they reach the right audience? How do we advocate for policy change based on our research? How do we enter into a broader public debate around topics we may have expertise in? What are the direct incentives to do it? To date, these challenges have not been systematically addressed by our field, nor by our parent field of HCI (Colusso et al., 2019). While 61% of research papers in ACM IDC (Conference on Interaction Design for Children) and ijCCI (International Journal of Child-Computer Interaction) from 2011–2019 contribute empirical knowledge and 23% contribute a novel artefact, only 1% contribute to reflection and discussion (Kawas et al., 2020), which might include knowledge sharing about KT. In addition, there are few exemplars and it is largely unknown how to foster the kinds of partnerships required for successful KT. And lastly, addressing the challenges of KT takes time, training, trial and error, and dedication outside of the main criteria most of us are rewarded for in our jobs, despite grant applications that ask for demonstrations of KT. While KT may happen without our efforts (or even knowledge), it is too important to leave it to chance.

One of the prime outcomes of CCI research is design guidance. Yet within our field, incremental design research or application of that guidance with partner organizations that builds on, and/or validates these frameworks is largely missing. Within our field, as with the broader field of HCI, there is little evidence that our design guidance is taken up by other researchers, industry, or community organizations. Again, this may be a measurement issue. Academics may translate knowledge through industry hosted social media posts, however these tend to focus on educational technologies and it is difficult to assess the impact. And although in the USA both Sesame Workshop and PBS Kids have attended ACM IDC conferences and taken away guidance and ideas (and there are the other examples of this worldwide), these are exceptions and tend to be driven by those large media organizations rather than by researchers. In most of these successful cases of KT, what is still missing is the bi-directional feedback from those agencies back to researchers about how they used the outputs of scholarship, what value or impact KT had for their organization and how future outputs could be made more accessible and useable. In many cases, such feedback may not be possible due to nondisclosure or intellectual property policies within the organizations that make use of our research. In other cases, organizations may access free materials (e.g. the second author's publicly available book (Hourcade, 2015)), but it is impossible to know how this information is being used.

It is very likely that there are ways that we have impact that are unknown to us. One of these ways is likely the kind of value-sensitive training we may provide to students and/or junior colleagues. CCI is a field characterized by strong values including a desire to provide benefit, inclusivity, ensure lack of harm and address issues of social justice. It is likely that many students trained in this environment will take these values into their future careers beyond academe (e.g. industry, government, community organizations). How that plays out for them is unknown. We have had discussions with former students and like many of us, they have experienced frustration, a lack of agency, and a lack of autonomy to make a difference. While we may expose students to our values we do not, as a field, train them or help them develop strategies or tools to advocate increased awareness or change regarding of many of the issues we care about.

*Provocation:* The many forms of KT outputs of our research, including design frameworks, empirical findings, and ethical guidance, rarely make an impact beyond academe.

*Envisioning a Future:* Again, we ask ourselves: why are we doing the work we do? What is the value of our research beyond helping us publish, receive academic recognition, get tenure, and ensure the constancy of our jobs and safety of our careers?

We propose that there are several ways we, as a community, need to work together to create resources, tools and practices that will enable us to have meaningful KT. First, we need methods and frameworks with exemplars that demonstrate how we can measure the cumulative impacts of our research over the long term. We need to understand how to collect data about impacts that may take different forms (e.g. inform products, practices, services, training or contribute to public dialogue and policy). This would enable us to think about how to achieve these impacts as we plan and conduct our research, not as afterthoughts. Another way to ensure broader impact of our research is to provide practitioners with accessible summaries that synthesize multiple studies, for example Anthony, Hiniker, and Kientz (2018). Since the challenge of KT extends to much academic research, we need to look beyond HCI, perhaps to the biomedical sciences or cross-disciplinary initiatives,<sup>3</sup> for exemplars of KT best practices. We also need to acknowledge the role that others play in translation of knowledge. For example, industry-based researchers and science communicators have much to offer, and we recommend recruiting them to conduct tutorials at ACM IDC conferences.

Second, we also need exemplars of how we can use the outputs of CCI research (e.g. knowledge about design, technologies, ethical issues) as a way to engage in broad dialogue outside academe. While we most often assume KT involves the uptake of knowledge created from research into industry-based design practices, there are other forms and audiences for KT we may want to consider. For example, as pointed out above, research prototypes alongside plain language reports and presentations can make ideas concrete for non-academic stakeholders. We may want to recruit researchers from within or beyond CCI who are willing, trained, and experienced to show us how we can promote and/or make social change through our research so that our research is better woven into the fabric of local communities. Perhaps we can draw on advocacy work in the broader HCI community to better understand the approaches and processes we can use to do this work (e.g., HCI research in civics (Boehner & DiSalvo, 2016), action research with refugees (Fisher, Yefimova, & Yafi, 2016)). Here again, we may leverage our diverse backgrounds and form partnerships with other researchers who

<sup>3</sup> For example, see <https://re.ukri.org/knowledge-exchange/knowledge-exchange-framework/>.

have worked for or with industry or community groups or other institutions where we want our work to have impact. They will help us speak the right language and find processes conducive to KT.

Third, another under-explored avenue for KT and impact is weighing in on policy – nationally and internationally. For example, the American Academy of Pediatrics was consulted recently about the optimal amount of screen time for children. Why was the CCI community not consulted? Why do we not advocate? We propose that it is imperative that senior international researchers in CCI work together to establish an organization that can inform policy at both national and international levels. One approach to making information broadly available is scoping reviews, which could provide a synthesis of research on a variety of emerging topics involving children, similar to those done in mental health (Arksey & O'Malley, 2005). CCI researchers could work together to create a variety of scoping reviews that make evidence accessible for policy formation (Munn, Peters, Stern, Tufanaru, McArthur, & Aromataris, 2018).<sup>4</sup> If we view technological innovation through a political lens, and we think we must, then we have a responsibility to advocate politically both through our research and as a community of experts. Another step may be to establish and create awareness of an international IDC expert panel that national and international agencies can turn to. Even more importantly we need to move beyond being experts and find ways to enter political and public debates on issues of importance, such as, what tools do future generations need to address systemic and existential social, political, and environment crises we are now facing? Acting collectively in such a way may also have a greater impact than having uncoordinated individual efforts.

Another potential collective effort is to better engage with the media during IDC and related conferences. Having a media chair to communicate with local media outlets, provide passes to the conference (e.g., an emphasis could be made on demos), and arrange for interviews with authors could help increase the impact of the conference and provide some positive publicity for the local hosts. IDC conferences could also be venues for TED-like talks where local audiences could be invited, which could be recorded and posted online to have another form of reaching wider audiences similar to *CHI Lites*<sup>5</sup> created recently by the ACM CHI conference.

Finally, to have real impact beyond academia, we propose that we need to form more interdisciplinary collaborations in which we work with others beyond our boundaries. To do this we need to better understand how to form alliances and partnerships with shared goals, how to leverage government funding and incentives that support long-term collaborations for pure, applied, and action research projects that may result in KT within the ecologies of our different cultures and countries. Virtual workshops, which may be easier to attend non-academic stakeholders, are one way begin to form collaborations and co-develop tools and practices (e.g. Iversen, Smith, & Dindler, 2018). We also need avenues that can train us how to communicate with cross-disciplinary audiences and use processes that support effective collaborations both with other academic disciplines (e.g. learning sciences, medicine, social work, psychology) as well as external organizations. We need resources, such as blogs and forums, that share and highlight KT successes and failures. For example, the Interaction Design & Children Toolkit helps researchers translate CCI research findings into actionable guidance targeted at industry-based designers (Chen, Nayak, Wong, Kawas, & Kientz, 2020),

which could then be posted on industry-based social media sites (e.g. on Children and Media Professionals Facebook group,<sup>6</sup> Joan Ganz Cooney Centre blog,<sup>7</sup> UK Parents For a Digital Future<sup>8</sup> blog). In short, we need to take the time to develop practices, collaborations, and resources that support us to conduct successful KT if we are to meet our goals around impact.

#### 4.3. The allure and misconceptions about the role of theory in CCI research

As the CCI community has matured, there has been a push to include theoretical foundations in our research in order to ground our designs in theory and/or to produce or extend existing theory through empirical work. These forms of theory-intensive research may be basic research, designed to push theory forward, or translational research designed to create theoretical knowledge that can inform design practice. Of course, not all research needs to be grounded in a theoretical foundation nor produce theory as an output. However, in CCI many researchers have been taken in by the allure of theory and then found themselves grappling with how to include theory in their work. Some of the main ways theory has been used within HCI include: to inspire new areas where interactive technology may change, improve or make new forms of interaction possible; as a foundation, to inform either conceptually or using specific concrete mechanisms (more rare) the design of interventions and systems; as a justification for design decisions made during the development of a prototype; and/or as a lens in order to look at specific aspects of interaction or outcomes during evaluations, either formatively or summatively. The application of theory can range from descriptions and explanations at a biological level, at an individual level and at the small group level (most common in CCI), extending right up to the level of social systems. At its best, this kind of application of theory involves taking current theoretical descriptions or explanations and applying them at a level that can be used to make decisions in the research or design process (Antle & Wise, 2013). However, there are relatively few cases where this is being done at a level of specificity and with the rigour of the theory's parent discipline(s) – that is, in ways that would make the research acceptable to those disciplines. Instead we have many cases of research publications in which theory appears to have been added because authors feel it is expected. Such approaches can result in loose application of theory and in using the same theories over and over – often outdated (e.g., Piaget without consideration of post-Piagetian updates) or using high-level “mbrella” theories (e.g. Vygotsky's sociocultural theory, Papert's constructionism), without the theory having a direct effect on the research design. In these cases, researchers may be uncertain or unable to use theories in more meaningful ways rather than seemingly using them to legitimize their research through association with theory – ticking off the box, so to speak.

Conversely, we have seen theories used to ground entire sub-fields of CCI, however it is unclear to what benefit. For example, we have seen Papert's constructionism receive considerable attention for over 40 years in research with children, technology, and education. Papert's vision was of children arriving at “powerful ideas” by working on projects (constructing items) that arose out of their strong interests, with the guidance of adults, and access to computers as powerful and flexible tools to develop these projects, which then would result in enhanced performance in mathematics, science, and so forth. However, over this time

<sup>4</sup> For an example of a scoping review for policy, see <https://www.gov.uk/government/publications/specific-learning-difficulties-current-understanding-support-systems-and-technology-led-interventions>.

<sup>5</sup> <https://sigchi.org/chi-lites-about/>.

<sup>6</sup> <https://www.facebook.com/groups/ChildrenandMediaProfessionals>.

<sup>7</sup> <https://joanganzcooneycenter.org/blog>.

<sup>8</sup> <https://blogs.lse.ac.uk/parenting4digitalfuture>.

there has been very little evidence that this theoretical perspective instantiated in technological-mediated activities produced superior learning processes or long-lasting positive outcomes in areas other than the skills involved in carrying out constructionist activities (e.g., programming) when applied to general populations of children when compared to other approaches. Where is the evidence? Many more recent projects involving programming activities for children have been justified in terms of providing more STEM learning opportunities, through the concept of computational thinking, or in terms of empowerment, but not as a means to get to “powerful ideas”. Other approaches, which have some compatibility with constructionism but are much more widely adopted in the learning sciences, such as active learning, have significantly more evidence to support them (Wiemann, 2014).

Along these lines it is important to ask ourselves if the theories we have been using have served us well or how they have served us and how they have not. In some cases, the aforementioned push for using theories comes from the learning sciences (and education research). For example, in the USA proposals for research in the learning sciences typically must include a theoretical foundation. And yet, many CCI researchers who work with educators have found many effective classroom learning practices are often theory agnostic, instead derived from experience and based on what works in the classroom. As such, it is important for authors to be explicit about their reasons for using theory and for reviewers to expect explanations about why (or why not) researchers are using theory in their research, what the role of theory is in the research, what constitutes evidence of benefit, what evidence exists (if any) with regard to the use of theory, and if and under what conditions the theory may be validated or extended.

*Provocation:* The CCI Community has used theory largely as an umbrella to legitimize their research rather than as a means to investigate theoretical mechanisms underlying child-computer interaction in order to contribute to theory validation and generation.

*Envisioning a Future:* We urge full disclosure when using or not using theory rather than insisting on a singular approach to theory use. What work is the theory doing? What are the limits of theory? Why is theory not appropriate as a grounding mechanism?

When we seek to ground our design(s) in one or more theoretical frameworks we must do so with an attention to the specificity and depth found in that theory's parent field (assuming it is not HCI!). And when we justify design decisions based on theory, we must concurrently reason about how (and what) to evaluate in order to validate or extend the theoretical mechanisms and models we used. Or, if we are using theory as a lens to investigate interaction (rather than inform design), we must specify, gather evidence for and write about theoretical concepts and constructs precisely and rigorously. As a result, in addition to contributing the field of HCI we may also be able to contribute through theory-building (deductive or inductive) to two-way dialogue with the theory's parent fields (e.g. publishing in non-HCI venues), thus broadening our impact.

One way to achieve this level of quality around the use of theory is to include researchers on our research team who are experts and/or are trained in areas related to the theoretical foundations we are using (also including experts in methodological foundations – see Rigour below). For example, in research on a smart toy intervention to support children to improve emotion regulation through experiential learning the team included an expert in children's mental health, a clinical psychologist and a non-profit organization specializing in implementing

socio-emotional learning programs in schools (Slovak et al., 2018; Theofanopoulou, Isbister, Edbrooke-Childs, & Slovák, 2019). This expertise enabled the team to produce detailed explications of theoretical mechanisms (based on Gross's emotion regulation (ER) theory (Gross, 1998)) which were instantiated into the design of the smart toy intervention, posited to produce multi-level effects, and evaluated in the field, first as a feasibility study then later to establish efficacy through pilot, with a large scale style study in the works enabled by the non-profit organization. The team also recruited James Gross, the originator of the ER theory, to provide input during the project. The inclusion of these experts facilitated high quality research and publications that were taken up in top quality HCI venues but also in a top mental health journal. This broadens the impact of the work where it may receive uptake academically in mental health research and through KT make its way into clinical and educational contexts. In this kind of example we see that the “work” done by theory is multifold, rigorous and detailed, and leads to a substantial contribution academically, through KT via a partner organization. And while not all researchers may have access to funding, time and expertise at this level, this work serves as a valuable exemplar of the way we can use theory to improve the impact of our work in HCI/CCI.

In addition to deeper use of theory, we also need to broaden the range of theories we use to ground our research into children's technology. For example, at the level of an individual there is an opportunity to explore how theoretical frameworks from cognitive psychology that bridge traditional theories (e.g., constructivism) with what we know about the biology of the brain (e.g., neuro-constructivism (Sirois et al., 2008)) might be utilized in our research. Such theoretical approaches could lead, for example, to study how children change together with their technological ecosystems and to specific goals in how these changes occur. We may also want to broaden our theoretical grounding at the social and contextual level. For example, culturally sustaining pedagogies may inform us how to design technologies that support children from non-dominant communities to integrate their own language, culture and literacies into dominant forms of learning, which may help address issues of social justice (see theme 7) (Samy & Paris, 2017).

There is also value in exploring very recent theoretical work where posthumanist theories posit how humans and materials exert reciprocal agency on each other and/or transhumanist theories that describe the opportunities, challenges and ethical concerns related to human augmentation. Frauenberger has begun to explore and posit how posthumanist theories may apply in the context of HCI more broadly (Frauenberger, 2019). A team of young researchers have explored from a transhumanist perspective future design scenarios about augmenting children with technology (Buruk et al., 2020), and Eisenberg presents a thoughtful and inspiring discussion of some of the questions that transhumanist technologies raise for children's design (Eisenberg, 2017).

While we advocate here for a deeper use of theory in CCI, we also acknowledge that there is an ethical imperative in our field to positively contribute to children's lives. An excessive focus on theory that has little practical chance of resulting in anything concrete or of having a positive impact on children may contribute publications and little else. Likewise demands on always having a theoretical framework for research can result in forcing good ideas into theories that do not fit, or even favouring research mainly based on whether it applies a popular theory correctly rather than on whether it has a positive impact on children. Not all CCI research needs, nor produces theory. In fact, theory may limit innovation and creativity. CCI (and HCI) as a hybrid of design and technology development traditions, value – and should continue to value – theory-driven research alongside



artefact-centric innovation, as well as mixed approaches. As we move forward, we envision the CCI researchers taking a more nuanced and well articulated approach to the use and value of theory in their research.

#### 4.4. Addressing rigour and measurement in an interdisciplinary field

One of the challenges of interdisciplinary research like CCI is the variety of research approaches and methodological approaches that may be used to conduct CCI research. CCI research may be basic, applied, clinical or translative, and it may draw methodologically from learning sciences, education, cultural studies, critical research, developmental psychology, social work, ICT4D (information communication technology for development), cognitive science, neuroscience, behavioural psychology and more. It may take a predominantly quantitative, qualitative, mixed method or artefact/design-based approach to data collection and analysis. While we feel strongly that a variety of approaches, research methodologies, and methods are needed to explore all the reaches of CCI, systematic application of these approaches is needed to ensure rigour, commensurate with their home disciplines. The challenge here is that on top of becoming expert in interaction design, interface design, technology development and, as needed, possibly relevant theories related to child development, we must also develop a substantial methodological toolkit that we can use to conduct the research. The complexity of many of the research and applied problems we are solving often merits mixed method approaches. But to do this well, there is just so much to learn.

As a result of the need for multiple and perhaps multi-level approaches to research, a negative side effect is that research may be conducted and published at a level of rigour that is lower than what would be acceptable in the parent discipline that informed the work. For example, in our field we have seen that the analysis of qualitative behavioural and/or interview data is often described in simplistic terms as “content analysis” and “thematic analysis” with very little deep engagement with methodological rigour in terms of how, for example, behaviour psychologists or learning scientists might analyse interactional data or how anthropologists might analyse and interpret interview data. Artefact/design-based methodologies are not exempt from this critique. For example, although research through design may be seen as a love child of HCI and Interaction Design, it is rarely conducted or written about with a sufficient level of rigour or adherence to methodological processes outlined by those who co-developed it. Other forms of artefact/design-based methodologies also pose similar challenges (e.g. autobiographical design, slow design, speculative design, critical design, critical making).

In addition to the quality of research methodologies, the field needs to expand on the ways it measures and improve rationalizations for particular measurement instruments. Many papers lack grounded reasoning in terms of how they define concepts of interest, translate these concepts to constructs that can be empirically investigated and operationalize these constructs, either with qualitative research instruments or with quantitative measures for variables. Researchers should keep in mind that what to measure should depend on a variety of factors, such as the population of children, the social and physical context, and the goal and stage of the research.

Another challenge is the lack of longitudinal and/or field studies that might take our work out of the tidiness of our labs and into the real-world contexts of children's lives. While this work is difficult to do with rigour due to the messiness of the “wild”, the results, accumulated over time and over studies, will provide valuable evidence of potential real-world impact and help us mitigate and steer away from possible harms. Securing funding for

such studies can be difficult and susceptible to changing governments and policies, highlighting the need to work in partnerships, not only across disciplines but across funding agencies.

*Provocation:* Often CCI research lacks methodological rigour commensurate to the level of those methodologies' parent disciplines, and this undermines the quality and value of research outcomes and potential impact.

*Envisioning a Future:* In much the same way as we propose collaborations with experts in theory could improve our research we suggest that, where possible, including experts in methodologies would be beneficial, either formally on research teams or informally as advisors. For work that makes strong knowledge claims (about usability measures, intervention efficacy, learning outcomes, etc.), all methodologies, regardless of origin, should be applied with rigour. CCI reviewers should evaluate claims against methodological rigour, again asking for advice from experts as needed to provide adequate reviews to ensure that research outcomes and potential impacts are reliable and valid.

In addition to excellent resources on evaluation for CCI (e.g. Markopoulos, Read, MacFarlane, & Hoysniemi, 2008) we need opportunities to practice and share successes and failures. For example, the ACM IDC and CHI conferences are excellent venues to hold workshops tutorials on different methodological approaches and may benefit from inviting experts from beyond HCI. This has the dual goal of creating opportunities for cross-disciplinary methodological collaborations. For example, this workshop (Barendregt, Bekker, Börjesson, Eriksson, Vasalou, & Torgersson, 2018) brought together researchers in industrial design, computer science, education and HCI to share knowledge and practices about creating intermediate level design knowledge in CCI.

The need for methodological rigour in empirical research may need to be balanced with the need for rapid technological, design or methodological innovation in a particular research context. We are a field that moves with technology, typically at a much quicker pace than fields like the learning sciences, developmental psychology, or anthropology. Therefore, the level of rigour expected in other fields does not necessarily apply to risky, exploratory, highly creative, or innovative work as long as claims are made commensurate with rigour. In these cases reviewers may need to look for face validity in research designs and/or evaluation methods to ensure knowledge claims are commensurate with empirical evidence. The need for rigour when making strong claims in no way undermines the value of descriptive and argumentative accounts related to innovative technologies, artefacts, design methods, or methodological advances. Rather, claims must be tempered to align with available evidence.

The field has yet to take up methodologies for evaluating technologies and interventions where the design can be adapted throughout a study and yet still evaluated rigorously. For example, there may be a need to evaluate early and often in order to mitigate risk, manage collateral or incidental findings or iteratively improve the design as quickly as possible when working with vulnerable populations. We need to learn from other fields (e.g. biomedical research) how to expand our repertoire of methods. For example, it may be beneficial in CCI work in mental health to consider incremental and cumulative effects such as those that can be measured using micro-randomized trials (Klasnja et al., 2015), Bayesian trials or n-of -1 studies in which children are their own comparators and which open the door to smaller and possibly more informative studies that are still done with rigour. Again, collaborations, highlighting quality exemplars and tutorials may be key for adding to our toolkit.



#### 4.5. Challenging participatory design benefits and ways to leverage PD methods moving forward

There are at least three main strands that have been brought forward to justify the use of Participatory Design (PD) and related co-design methods with children. The first is that, as a rule of thumb when designing technology, the more different the users are from the design team, the greater the need to engage with users in order to understand their needs, abilities, preferences, and contexts of use. As such, PD and co-design methods are used with the goal of higher-quality designs arising from ideas that would not have been developed without children being involved in the process. The second justification is that children have a right to shape the design of technologies they and their peers will use. This strand comes mainly from the Scandinavian PD tradition and researchers who emphasize it put an emphasis on empowering children through these activities. A third and related strand is that children can benefit from these activities by learning about the technology design process, the choices that are made along the way (including ethical choices), and how they can exert control over technologies.

The challenge with all three justifications is that we currently do not have strong empirical evidence to support them. In terms of higher-quality designs, it is the experience of the second author and it has been reported by others (e.g. Guha, Druin, & Fails, 2013) that co-design activities result in design ideas that the adults would have never developed on their own, and these ideas have found their way to a small number of widely available applications (e.g., the International Children's Digital Library<sup>9</sup>). However, it would be difficult to obtain funding to, for example, design the several pairs of technologies using multiple design methods (in particular something complex) and then assess which are the better designs. It is unclear if PD produces better products for children or there is any benefit to children who are future users of technologies under study — that is, to those not involved in workshops or after school programs.

The second strand proposing PD as a means to empowering children is value-based and philosophical and therefore more difficult to evaluate in terms of impact. It is based on a normative assumption that PD benefits children, and it does this, in part, through giving children (which children is unclear) a voice. However, as pointed out by Iivari and Kuutti (2018) there has been little dialogue in CCI that envisions how to link participation in research to forms of empowerment such as supporting children to engage with issues of power, politics, ethics or their own, possibly marginalized, status as minors. In addition, empowerment is rarely defined, explicitly supported or concretely operationalized in PD research with children. That is, PD as currently practised in CCI is rarely political in its origins or outputs, and any empowerment that may occur is largely not scalable out of academic settings (Frauenberger, Foth, & Fitzpatrick, 2018).

The third strand related to the benefit to children who participate in PD sessions would require long-term follow-up with large numbers of children participating in the activities, which also has not been practically possible, although there may be opportunities given some recent developments in Denmark (Smith, Bossen, Dindler, & Iversen, 2020). A particular challenge here is that in CCI children who participate in PD are often recruited through snowball samples, which can result in cohorts of middle-class children with educated parents who want their children to be exposed to research and technology, raising issues of inclusivity and social justice. A few researchers have understood this challenge and are already purposefully working with lower-income and/or children from disadvantaged communities (e.g., Lamichhane & Read, 2020;

Sobel, Kientz, Clegg, Gonzalez, & Yip, 2017; Walsh, Donahue, & Pease, 2016).

There is also a need to adapt PD and co-design methods and activities to the current reality of children's relationships with technologies. Druin developed cooperative inquiry when children mostly used desktop computers at labs in their schools or in home offices. Since then, researchers have worked on adapting these methods to take into account the wide diversity of social and physical contexts of use and the fact that children may arrive at design activities with greater comfort and experience with certain technologies than the adults in a design team.

A final challenge that has long been observed is that these methods tend not to be practical in an industry where products need to be developed quickly and it is not practically possible to involve children as design partners.

*Provocation:* Researchers conducting PD and co-design have not always been clear about why and how they apply these methods, claims about benefits have largely not been commensurate with evidence, and issues of empowerment and inclusivity have not been adequately addressed, nor have methods kept pace with children's changing relationships with emerging technologies.

*Envisioning a Future:* Moving forward there needs to be well reasoned justification for inclusion of children in PD and/or co-design. There should not be an expectation that every CCI project involve children in PD or co-design, as opposed to, for example, informant design. However, when the adults in the project know little about the children and their contexts of use, there is a need to gather this information, and PD and co-design methods are one valuable approach to engaging with children and vice versa.

When reporting on design activities with children there is also a need to step away from claims of having informant design or design partnership and instead focus on providing information on the methods used, number of sessions, number of children, how ideas were incorporated, and so forth. The reason is that different authors tend to have different perspectives on what constitutes PD, co-design, or design partnerships. Other categorizations are too broad. For example, a project where children provide design ideas during one session would be considered as having children participate as informants, just the same as a project that involves children in multiple locations, over dozens of sessions, over several months. If possible, tracing design ideas to specific events in design sessions can also be useful in understanding the level of impact children had on a particular design.

We also advocate for an open mind when considering options to empower children. For example, what can PD methods and their successes and failures tell us about how to empower students to be politically and ethically active in future technology developments that impact their lives? Can we use PD methods to include children in the design of the technology literacy curriculum itself?

In answer to these questions, there are two pressing and related socio-technical educational challenges that we think PD methods could be leveraged to address, creating avenues for new research trajectories related to PD in CCI. First, as early as 1999, the National Research Council (USA) suggested that technologies were evolving at such a pace that education should focus on fluency rather than skills per se (National Research Council, 1999). Technological literacy (or fluency) is a general set of skills and knowledge relevant to participating actively in social and political life. Ensuring that children become technologically literate requires creating culturally meaningful curriculum where they can actively engage with technology in ways that facilitate exploration of what technologies can and cannot provide, and are scaffolded to reflect on the social, political, and ethical implications of everyday technologies. A focus on technological literacy is

<sup>9</sup> <http://en.childrenslibrary.org/>.

integral to children's eventual participation in future political and democratic processes. For example, [Iversen, Smith, and Dindler \(2017\)](#) position children as protagonists in PD, where they engage with real world problems relevant to their everyday lives (e.g., designing public spaces) in ways that support the development of not only technological skills, but the kind of reflective processes required for technological literacy.

Second, there is need and an opportunity to define and support children's "empowerment" beyond giving children a voice and equal power in PD workshops (and in rare cases the resulting products). These forms of empowerment are in alignment with the objectives of the critical research tradition. We see an opportunity to enable children's empowerment through various forms of critical and/or speculative design (see [livari & Kuutti, 2018](#) for a similar discussion). In particular, we see an opportunity to leverage what is known about PD methods and use these to create research projects where children engage in speculative design and/or critical making as part of their education in technological literacy. For example, the first author is developing a critical making workshop for middle school children that would enable them to critically engage with ethical issues of importance to them through making biowearables as part of their technological literacy curriculum. The position of empowerment through technological literacy has been taken up by [Iversen et al. \(2017, 2018\)](#).

The future of PD, co-design, and informant design lies in updating methodology to take into account children's fluid and ubiquitous relationship with technology. Social and physical contexts of use need to be incorporated, which is likely to mean more field research as well as incorporating other people affected by technology. One obvious area to expand is in designing for families.

Taken together we propose that there is an opportunity in CCI to conduct research in PD that ensures that not only are children's voices being heard but they are being empowered to think critically about the role, impact, ethics and development of technologies, focusing on technologies of importance in their current and future lives (e.g. AI, digital health, sustainability, online education). A longer term goal of this work would be taking a KT approach to advocate for widespread adoption of this form of empowerment through technological literacy curricula, alongside the development of ways to measure cumulative impact, perhaps designing the curricula through further participatory processes as advocated for in [Iversen et al. \(2018\)](#).

#### 4.6. The problem with how we "do ethics" and how to reconceptualize what is needed

Despite a lack of definition of what we mean when we say "ethics", it has been – in its many forms – a central concern during the history of CCI. Early on, one primary ethical concern was a commitment to the involvement of children in research processes, ranging from human subjects to participatory design partners, so that children would have input into the technology they would use ([Iversen et al., 2017](#)). Despite this long history of children participating in our research, only 6% of ACM IDC and iJCCI papers included reflexivity about participatory ethics. When the ACM IDC conference organizers added a mandatory section on participant selection and recruitment to all papers, there was a focus on procedural aspects of ethics, such as those elements required by research ethics boards prior to studies. Typically procedural ethics takes a rights-based approach, for example, addressing the protection of minors participating in research using guiding principles related to respect for persons,

beneficence, and justice.<sup>10</sup> Requirements for these types of ethical considerations vary by institutional approval board. What has been lacking is mention of important mainstream issues concerning the participation of children in research as seen in other fields (e.g. medicine, psychology) such as addressing data aging, power imbalances, inclusivity, or appropriate *a priori* assessment of risks and benefits.

As CCI has grown and evolved, we have seen papers that focus on specific ethical areas including concern for negative impacts of technology on children's development (e.g., [Antle & Kitson, 2021; Hourcade et al., 2017](#)), breaches of privacy and the impacts of surveillance (e.g., [Hourcade et al., 2017, 2018](#)), and mediating potential harms and long term responsibility to communities when we work with vulnerable children (e.g., [Alper, Hourcade, & Gilutz, 2012; Antle, 2017](#)). Recently, our community has also begun to explore an ethical focus tuned to *in situ* ethics – a concern for children's well-being before, during and after participation in research, with special consideration for micro-ethics, what happens of ethical import in the moment-to-moment interactions during child-participant research ([Frauenberger, Rauhala, & Fitzpatrick, 2017; Spiel, Brul , Frauenberger, Bailly, & Fitzpatrick, 2018](#)). Again, this important work is nascent and led by a handful of colleagues in related research groups. For example, despite four years of panels, SIGs and workshops at ACM conferences on ethics in IDC, there are still no cross-cutting, community-based initiatives to summarize or propose ethical guidance that addresses accumulated knowledge in participatory, procedural, situational or these specialty ethics topics in CCI.

Another area that has gained attention in HCI but that is largely missing the CCI literature, aside from SIGs, is speculative ethics, also called design ethics and everyday ethics ([Mechelen et al., 2020](#)) (for exceptions, see [Antle & Kitson, 2021; Antle et al., 2021; livari & Kuutti, 2018](#)). Speculative ethics is concern for and investigation into the potential or actual future impacts of interactive technologies in children's daily lives. A related issue of concern is the increasing number of papers published in the field of CCI (and more broadly in HCI literature) where the focus of investigation is on technologies that are already commercially available, many of which assume unavailability of adults in children's lives, rather than proactively exploring the ethical and/or socio-technological issues of emerging technologies in ways that might guide technology development, once we address challenges of KT!

Despite omissions and nascent efforts, since inception, there has been – compared to the broader field of HCI and even more so when compared to computing – a consistent value placed on all these types of ethical considerations. For an overview of the last 18 years of IDC research that mentions ethics, see [Mechelen et al. \(2020\)](#). For a summary of "values" in CCI and ethical considerations from 2011–2019, see [Kawas et al. \(2020\)](#).

**Provocation:** The CCI community has engaged continuously but not deeply nor systematically with important ethical constructs that are deeply relevant, not just for research and technology design with children, but for all humans. In particular there is a lack of published material on this topic.

**Envisioning a Future:** Moving forward there is an opportunity to be more reflexive in our discussions of procedural ethics, perhaps informed by what we are learning (and should be writing about) in situational ethics and to exchange ideas with other fields that have been conducting human-subjects research with children for decades, but who typically do not consult children directly. We need to advocate for dedicated space in CCI publications for

<sup>10</sup> Examples of rights-based approaches to research ethics include the [Belmont Report](#) and [Declaration of Helsinki](#).

explication of ethical decisions that were made during the design of technologies to provide a richer picture of the research. We see a strong need for future-looking ethical discourse that involves children, perhaps through critical making studies or curricula or other forms that enable engagement with difficult and abstract concepts (similar to Antle et al. (2021), Iversen et al. (2018)). For example, PD approaches might be turned to working with well-informed stakeholder groups to explore different future scenarios enabled by mainstream and emerging technologies in order to better understand, advocate for, and steer the research questions we ask as a community, the technologies we develop and the opinions we form, and make known to others (e.g. through expert panels and/or policy advocacy). There is also a need for more universal guidance that can be drawn from existing works in other fields and applied to CCI as well as for CCI specific, theory-grounded frameworks to guide ethical investigations (e.g. Antle & Kitson, 2021), and in particular explorations of potential future impacts specific to the kinds of technologies in development by the CCI community (e.g. using computational alternatives with children and families (Yoo et al., 2020)).

#### 4.7. The call for inclusivity, diversity and social justice: Doing it right

Another normative assumption, and value expressed repeatedly in our response data, is the call to action for our community to work with more diverse and with mixed groups of children as participants in our research (inclusivity) and to create technologies for more diverse culturally, economically, geographically, and mixed abilities groups of children (diversity). Alongside these values is the assumption that we should address issues of social justice (e.g. power imbalances) through our research in technology development. These calls to action have been taken up initially in CCI, for example, see work on inclusivity (Sobel et al., 2017), diversity (e.g. displaced peoples such as refugees and immigrants) (Antle et al., 2019), social justice (e.g. Covid-19 related impacts including the increasing digital divide) (Antle & Frauenberger, 2020)). At times, it may seem that focus in CCI on developing technologies related to informal learning, play and other enrichment activities pales in comparison to the need to address issues of safety and security that many children face worldwide. There is a largely unaddressed need to build child-friendly technology solutions that empower children to be agents in their own safety and security.

While CCI researchers have largely found significant funding to work with disabled and neurologically diverse children, there has been little research with or for other diverse groups (e.g. children from local or distance lower-income regions, ethnic minorities, LGBTQ) (see Skinner, Brown, and Walsh (2020) for a recent exception involving co-design). In addition to lack of funding, this work is fraught with practical, logistic, and ethical difficulties and its high-risk nature make it untenable for those in early stages of their career.

The difficulty in conducting this research stems primarily from the fact that most CCI researchers do not come from these communities or have not lived there for many years. Without actual ties to these communities there can be ethical concerns, even for the best-intentioned researchers, in ensuring that they work on actual problems, address power imbalances, and contribute to something sustainable. Even for researchers with ties to specific communities, it may be difficult to conduct research in distant communities and contribute sustainable solutions. Other issues are directly related to income levels, which can cause significant challenges to children and their communities, making it more difficult, for example, for children to consistently participate in activities, or to even be able to focus on them if there are more pressing issues for them to think about. Finally, because we are

an international community, sometimes it can be difficult to understand the intricacies or importance of working with a specific community in a particular country, which can lead reviewers to not understand the importance and difficulty of the research being conducted.

*Provocation:* There is an imperative to tackle issues of inclusivity, diversity and social justice through research, yet little support from funding agencies for that, and many obstacles to doing it well.

*Envisioning a Future:* Research activities that address inclusivity, diversity and social justice are not, and should not, be for everyone. However, there are researchers in CCI that through ties to places or peoples, or a strong interest or personal experiences, are drawn to this work. Sometimes this is local and other times in geographically distant regions. This work is difficult, and researchers should seek guidance and training specific to cultures, contexts, and situations they seek to address. For example, in doing HCI work with displaced families, the authors in Antle et al. (2019) point out five areas of consideration for researchers working with uprooted children and families. Partnerships, especially those with local organizations, are critical and take time to develop. Working with research institutions from countries less well represented in the HCI literature requires understanding the traditions that research comes from. It is also important to know and step back in cases where it becomes apparent that technology is not a sustainable solution to a pressing social issue.

One longer-term solution to conducting more of this research is to work on developing pipelines of students from these communities who can then return and conduct research within their communities. Establishing ties with educational institutions in low-income and marginalized communities is likely then to be an important component of moving forward in an ethical and sustainable manner.

Within the local communities we work in, there is also an imperative to consider inclusivity and diversity when we develop new technologies and/or recruit participants for our studies. As past CCI research involving and supporting children with autism, children with dyslexia, children with mental or physical illnesses or those living in poverty have shown us, some of the children that are hardest to reach may benefit the most from being supported and/or involved in CCI research. Ironically, the COVID-19 pandemic may open up more avenues for inclusivity and diversity through remote means of reaching geographically dispersed children and their families (Antle & Frauenberger, 2020). Although care must be taken here to address the challenges the digital divide poses for remote participation (that is, for families with limited internet access). It also is important to keep in mind that research with and for more affluent and less marginalized children also has value. Even as we work with other faculty members' children in our research labs, we have an opportunity to design, develop and deploy tools with and for children that give them tools to create more just and fair societies.

## 5. Conclusion

It is a critical time for researchers, scholars, designers, practitioners and students in the field of CCI to pause and be reflective about where we have come from, and where we want to go. To enable such reflection we have put forward seven provocative statements (we hope) grounded in our opinions about issues we see facing the CCI community at this point in time. In describing our provocations we have exposed and unpacked some of the common practices and normative assumptions that we believe underlie the field of CCI research. As we formulated our opinions we "ground-truthed" and expanded them through a survey of



a small group of CCI researchers, which was conducted during the first months of the Covid-19 pandemic based on themes we proposed in our 2020 ACM CHI SIG, which was subsequently cancelled.

As a way to move forward for each theme we have also proposed several avenues that we envision might be valuable to explore. Where we have examples of research that exemplifies in these directions, we provide pointers to that work. Our envisioning is not meant to be exhaustive nor conclusive but to prepare the ground for productive dialogue within our community(s). We hope readers will read what we have written and join into this dialogue through social media channels (e.g. Facebook IDC group), and eventually in face to face conversations over coffee in labs and conferences.

### Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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### Appendix. CCI vision questionnaire

In summary, we ask you to:

1. Respond to the 2–3 themes listed below [varied by participant]
2. Suggest one or more additional themes that you think represent important successes/challenges/new horizons for investigation for the field of CCI
3. Recommend one or two additional researchers or industry practitioners to talk to who have a track record, either as a senior or emerging scholar/practitioner in CCI

We are aware that we are asking a lot of you, but we think that producing this document will be very worthwhile for our community. We especially hope that it will be useful for young scholars and researchers joining the field and will help us collectively clearly articulate the proximate goals and societal aspirations of the CCI community.

Please send us your reply via email by the end of the day DATE.

#### Theme 1: Values, Interdisciplinarity and Reach

One of the defining characteristics of a research field is the questions that the field values. What are important questions that our field has asked in the past? What are the questions our community should be asking in the future? How will interactions with other fields help define these questions? What questions will still matter in 30 years? How do you think your thinking about what our community values has changed this year with the COVID19 pandemic, if it has? Together with these questions there is also a need to define a vision for the reach of the field, both in terms of the children and organizations involved. For example, what is the best way of reaching children from lower-income regions of the world? Is there value in pursuing this? Should we be trying to influence educational systems? Policy making bodies? What areas do you see that our community should extend its reach into in the future? What are the barriers to reaching these goals?

#### Theme 2: Theory

In terms of theory a key question is what theoretical frameworks the field has yet to explore that are relevant for the future. An

example would be theoretical frameworks from cognitive psychology that bridge traditional theories (e.g., constructivism) with what we know about the biology of the brain (e.g., Neuroconstructivism). Such theoretical approaches could lead, for example, to study how children change together with their technological ecosystems and to specific goals in how these changes occur. What theoretical frameworks do you think have been beneficial as grounding for CCI research in the past? What frameworks have been underexplored? What frameworks would add value moving forward and why?

#### Theme 3: Rigour and Complexity

In terms of rigour, much work in our field is still characterized by informal, case based and short term studies and by methodologies that often lack the rigour of their constituent fields. For example, the complexity of rigorously coding behavioural data, which has been well addressed in the learning sciences, remains elusive in CCI research. In addition, the field has been limited (sometimes by funding) in conducting longitudinal studies that try to understand the long-term impact of technology. What CCI projects to you think serve as exemplars for rigorous research that adequately addresses the complexity of CCI? What areas do you think would benefit from more rigorous and/or longitudinal research? What areas of complexity might the field address moving forward?

#### Theme 4: Impact Within and Beyond Academe

One challenge for any scientific field is translating its findings into practice so they can impact millions of people instead of just a few participants in a research study. Within CCI, the participation of large media organizations has facilitated some of this translation. Larger questions remain on the degree to which the field should try to influence policy and society. Likewise, it is important to discuss the value of technologies that are unlikely to scale or to become available to children in lower-income regions of the world. A similar question is the impact on children who participate in research after researchers leave, in particular for vulnerable communities. In what ways do you think CCI research makes an impact within and beyond academe? In what ways do you think CCI should, moving forward, make an impact? What are the barriers to achieving these goals?

#### Theme 5: Technology, Values and Ethics

As the child-computer interaction field has evolved with technology and its role in society a theme that has been a constant through all these changes has been ethics. At first, the primary concern was the involvement of children in the design process, so that they would have a say in the technology they would use. Other ethical concerns incorporated over the years include concern for children's social and physical development (e.g., [Antle et al., 2020](#)), privacy and surveillance (e.g., [Antle et al., 2020, 2019](#)), and vulnerable children (e.g., [Alper et al., 2012](#); [Antle & Frauenberger, 2020](#)). What are the most pressing ethical issues the field is facing? What do you think is the nature of a “good” childhood? What do you think the role of interactive technologies are in a “good” childhood?

#### Theme 6: Emerging Areas of Research

There are many possible emerging areas of research. Below we present several areas of CCI research we think is worthy of pursuing. Pick one that resonates. For this area address: What do you see as pressing questions in this area? What do you see are the key challenges in conducting research in this area? What impact could research in this area make? What are the barriers to achieving these impacts? Are there promising areas that require further research and/or remain underexplored?

#### Children and Nature

A timely topic is the place for technology in children's relationship with nature and the planet. There are a few different



angles to this topic. Perhaps the simplest one is thinking about how technology can help incentivize children's connections to the outdoors, such as parks and wilderness areas in their vicinity. A related angle is to think about technology's role in educating children about their interactions with the planet, the effect of their everyday decisions, and the impact of collective decisions on climate change, pollution, native species, water and air quality, and so forth.

### Children and Big Brother

The relationship between children, big data, and surveillance has been a recurring theme for our SIG meetings at CHI (Antle et al., 2020, 2019). Our community has recognized the concerns about the large amounts of data that a variety of organizations are collecting from children and the danger of normalizing mass surveillance. There are research opportunities for educating children and parents about these technologies and also for developing alternatives to these technologies that address similar goals without compromising privacy or turning children into a set of numbers.

### Artificial Intelligence

Artificial Intelligence (AI) is becoming increasingly commoditized and part of children's everyday experiences. Children's perceptions of such technology as a part of their habitat are likely to be very different than those of adults. Children are growing up with voice assistants and they use biometric recognition and computer vision applications as fluently as previous generations used the mouse. However, we do not have a good understanding of the way they comprehend the function of this technology, the data management policies, and the inferences that can be drawn on their behaviour.

### Families and Mental Health

Studying children's technology in the home and how it interacts with family relationships is another emerging area. This topic can also be studied in combination with mental health topics for children who have experienced trauma. There are promising developments on emotion regulation technologies (Antle, 2017) with the open question of how these should be designed taking into account children's social context, in particular in the home. What do you see as pressing questions in this area?

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