Proceedings of the

CCC Workshop on Computer Science and Global Development

Berkeley, CA

August 1-2, 2009

Position Papers (by Author)

Computing for the Underserved

Rakesh Agrawal
Microsoft Research
Mountain View, CA
rakesha@microsoft.com

Sreenivas Gollapudi
Microsoft Research
Mountain View, CA
sreenig@microsoft.com

Krishnaram Kenthapadi
Microsoft Research
Mountain View, CA
krisken@microsoft.com

ABSTRACT

We make the case for focusing on economically disadvantaged and for developing technological solutions to address their needs. We then discuss compelling computing applications for the underserved: (a) viewing mobile phone as the first computing device (b) democratizing access to education.

1. WHY FOCUS ON ECONOMICALLY DISADVANTAGED

The "bottom of the pyramid", consisting of more than 4 billion people who live on less than $2 a day, presents a huge businees opportunity. In his book [3], Prahalad argues that there is money at the bottom of the pyramid as the large number of economically disadvantaged people represent a significant latent purchasing power. The poor people pay a preium for products and services as they tend to reside in high-cost ecosystems even within developing countries and thus there is huge potential for businesses to unlock this poverty premium.

We list a few key types of products and services that are desirable for the poor people.

- Commerce: Basic financial services including savings, microcredit, and insurance should be provided in a cheap, efficient and convenient manner.
- Healthcare: Efficient treatment models and remote medical services are desirable.
- Education: Currently there are fewer number of good quality teachers when compared to the huge (underserved) demand for good quality education. We would like to use ICT to address this asymmetry in demand vs supply.
- Agriculture: Access to timely and correct information about weather, crop patterns, commodity prices, etc is crucial. Similarly access to efficient marketplace is needed.
- Communications: Technologies such as mobile phones enable improved connectivity and access to the outside world.
- Governance: ICT can be potentially used to provide efficient government services and improve transparency in governance.

We next discuss a few computing applications focused on the economically disadvantaged that could have significant impact.

2. MOBILE PHONE AS THE FIRST COMPUTING DEVICE

Today there are about 4 billion mobile phone users worldwide out of which an estimated 64 percent live in the developing world. By 2012, half of all people in the developing people will have a mobile phone. An overwhelming majority of these people have not used a PC before and thus get connected to the networked world for the first time, through a mobile phone. For example, the number of domestic PCs in 2008 is about 85 million in China and 10 million in India. In comparison, the number of mobile phone users in 2008 is 547 million in China (up from 317 million in 2004) and is 305 million in India (up from 34 million in 2004). Given such rapid growth and the large reduction in the cost of mobile phones and services, mobile phones can soon become economically feasible for the poor to afford. We pose the following question: *"What is the most compelling application we can provide, given that a mobile phone is the first computing device for most people?"*

We propose a few potential compelling applications on the mobile platform. In each of these applications, there is a huge potential to reduce the asymmetry in access to timely and relevant information. The asymmetry arises due to the fact that there is unequal access to information, especially in the rural developing world. Lack of information leads to higher costs in money and time, competitive disadvantage, poor health and sometimes even death. With the rapid growth of mobile phones and services, we now have a way to provide access to the relevant information in a timely fashion.

- Local Marketplace: The goal is to build a mobile-based platform for the exchange of goods and services amongst the poor. We would like to focus on goods and services that are produced as well as consumed by the poor as these represent a market segment that is underserved today. It is desirable for the platform to support self-help groups and new payment mechanisms.

Once such a platform is in place, we can augment to support even a marketplace for locally relevant information. Sharing of information such as estimated demand and commodity prices can be valuable for local entrepreneurs. ICICI's e-Choupal and mobile phone usage leading to improved market performance and welfare in Kerala fishering industry [1] provide evidence to the potential of mobile phones for improving efficiency in local commerce.

- Healthcare: The goal is to build a mobile-based platform for localized content creation and dissemination related to healthcare. Further cell phones can be used to provide more efficient healthcare services such as early/remote diagnosis. We can develop applications that enable dissemination of relevant localized health related information to the end users and similarly let the patients as well as health workers manage health records. This platform can also enable data mining towards early detection of disease outbreaks and greater understanding of health trends at a granular level.

- Education: We envision a platform for collaborative sharing of educational content. This platform need not necessarily be mobile-based. We discuss further in the next section.

3. PLATFORM FOR COLLABORATIVE SHARING OF EDUCATIONAL CONTENT

Today there exists an asymmetry between demand and supply for educational content. There is a huge demand for good quality education which is underserved due to the absence of enough good quality teachers. This problem becomes more acute as we proceed earlier in the education level (e.g. high school level compared to college level). For instance, well-qualified teachers are usually concentrated in cities in emerging markets such as India and China. Consequently students from rural areas (especially those from underprivileged socio-economic backgrounds) receive poor quality education and are not in a position to compete for opportunities with those in cities.

Our goal is to provide a platform for collaborative sharing of educational content, thereby democratizing access to education. We want to provide a level-playing field for access to educational materials in developing countries, bypassing barriers such as urban/rural divide and economic and social disparities, by using internet as a means to deliver such materials. Our platform will help to: (1) improve the quality of teachers in rural areas as they can learn from say, lectures of more qualified teachers (2) provide better resources to students from rural areas.

We would like to support collaborative creation, sharing and management of educational content, using tools and easy interfaces on top of a wiki system. In this aspect, the platform shares common aspects with wikipedia. However, unlike wikipedia, we can have multiple content on the same topic and hence rating and ranking of materials on the same topic is important (eg: How do we rank all lectures on "quadratic equations" for Class IX?). For this purpose, we can make use of the reputation of the contributor (eg: professor at a top ranked university), ratings provided by users for a specific article and the overall user rating of the contributor based on all articles from the contributor. Further we need to support search and intuitive navigation to enable discovery of relevant materials. We also need to address problems such as how to seed content initially to bootstrap the platform and how to incentivize "producers" of content to contribute their materials. We need to study the factors that led to success of wikipedia in order to determine the incentives and the regulations (including moderation) needed for our platform. Initially most of the high quality educational content is likely to be available only in English. Hence we would like to enable translation into other regional languages. We plan to use the wikiBABEL framework [2] in which machine translation is performed first, followed by collaborative editing to correct the errors. We have to also ensure that the content is localized for the region or the social background (for example, "pancakes" may not make sense in India). Hence we may need to classify the available materials into natural hierarchies. We can also support a social network so that the users can form study groups, or interact with teachers and other students. We also need to filter adult, offensive or other inappropriate content and devise ways to discourage misuse of the service. Finally we need to explore ways of broadening the reach of the service. How do we reach out to people with no or very poor internet connection? We can consider other means such as mobile phone network, cable television network or offline access using DVDs. To address the bandwidth constraints, we should make the same multimedia content available in multiple formats such as audio only, low quality video, and high quality video.

4. REFERENCES

[1] R. Jensen. The Digital Provide: Information (Technology), Market Performance, and Welfare in the South Indian Fisheries Sector. Quarterly Journal of Economics, Vol. 122, No. 3:879–924, August 2007.

[2] A. Kumaran, K. Saravanan and S. Maurice. wikiBABEL: Community Creation of Multilingual Data. In WikiSym, 2008.

[3] C. K. Prahalad. The Fortune at the Bottom of the Pyramid: Eradicating Poverty Through Profits. Wharton School Publishing, 2005.

Missing Pieces of the ICTD Research Ecosystem

Muneeb Ali
Princeton University
muneeb@princeton.edu

1 Introduction

More than a billion, less fortunate, people on this planet survive on less than a dollar a day. Technology can help improve their lives. The last decade has seen interest in applying information and communication technologies for global development. Research in this multidisciplinary area, often dubbed ICTD, encounters massive challenges and impacts lives of ordinary poor people. In this paper, we will attempt to analyze the current "big picture" of ICTD research and identify some road blocks. We will limit the scope of our discussion to "technical" ICTD research i.e., building and deploying real networks and systems in the challenged environments of third world countries.

2 Some Roadblocks

We point out some imminent roadblocks in this section.

2.1 Identity

Names are important. Imagine you are at a conference and someone asks you the usual *"So which area are you working in?"* question. Wireless networking, sensor networks, and information theory etc., are all valid replies. In two words you convey a clear description of your research area and implicitly identify yourself with a research community. We argue that ICTD research currently lacks this sense of identity. In our situation, saying *ICTD*, *ICT4D*, or *DR* is likely to be followed by a blank stare. People often revert to saying *"developing regions"* followed by a short description of how this area is about applying computing and/or networking technologies to solve problems in developing regions – a vague and partial description.

The ICTD community needs to converge on using a simple name. Acronyms may not be the way to go. You end up spelling them out from time to time. Writing or saying Information and Communication Technologies for/and Development is not convenient. One can argue that over time the community will converge towards a name anyway. We believe that the sooner this happens the better. Right now is the opportunity to think about better, simpler, and clearer names. Names are important.

2.2 Definition

Converging on a standard name naturally leads us to the next question. What is ICTD research or whatever we may call it? What is the line between research and philanthropy? What separates "technical" ICTD from ICTD? Here we are not asking the more important question of what is *good* ICTD research, but rather a more primitive one. This question is important because what defines us also binds us together. Drawing these lines is non-trivial, given the multi-disciplinary nature of the ICTD space. One approach is to start defining what is *not* ICTD research. We don't intend to offer a solution, but are merely pointing to a road block.

2.3 Dissemination

Journals, conferences, and workshops play a critical role in any field of scientific inquiry. There have been some events solely focused on technical ICTD research, but they were primarily small workshops hosted with other well-established venues. Examples include NSDR with SIGCOMM and WiNS-DR with MobiCom. ICTD is missing a major component of the research ecosystem; the top technical conference.

Leading conferences not only provide a sense of community and direction to the field, but they also implicitly help in building an identity and defining a scope for the field. These days most high-impact technical ICTD works, e.g., WildNet [2] and HashCache [1], get published else where. This needs to change.

2.4 Funding

Research agendas at universities are, sadly, directed by the availability of funds. If funding agencies like NSF or DARPA are not interested in funding a particular area, not many researchers will want to work in it. Currently, a common approach is to find other legitimate motivations for similar research to convince funding agencies. Identifying alternate funding resources and pushing the case for ICTD funding in front of government agencies is critical for the future of ICTD.

2.5 Sustainability

ICTD research is generally considered a luxury that only tenured faculty can indulge in. Young professors running for tenure or PhD students preparing for the job market will not "risk" their career on ICTD research, no matter how genuinely interested they are in the area. Academic job placements are critical for the growth of ICTD as a sustainable area of research.

3 Future Directions

In this section we outline some possible future steps.

3.1 Technical ICTD Conference

The Workshop on Networked Systems for Developing Regions (NSDR) should evolve into a technical ICTD conference next year. NSDR has established connections with SIGCOMM and SIGOPS over the years and we should leverage these links to get official sponsorships. We will also need to explore sponsorships from IEEE and USENIX. NSDR has also established a "brand name" within the systems and networking communities. It is debatable if the technical ICTD conference should keep the same name or not. One options is to name the event Symposium on Networks and Systems for Developing Regions, keep the same acronym, and liberally expand on the scope of the call for papers.

3.2 Cross-department Hiring

It is unlikely that ICTD research will be perceived as a "core" area of research, by Computer Science departments, anytime soon. Current economic conditions and steady growth of faculty over the past decade seem to indicate that departments will hire less in the coming years anyway. There is, however, room for inter-departmental collaborative hiring. Computer Science departments should partner with other departments, e.g., Public Policy or Economics, that are interested in global development. In many cases, the partnering department may have more funds available. Cross-department hiring, in general, is an easier sell to university management as it promotes cross-departmental research.

3.3 Social Entrepreneurship

In recent years, there has been an increased interest in Social Entrepreneurship. Most top business schools have incorporated Social Entrepreneurship as a first-class citizen of their program and some have gone a step further by establishing dedicated centers e.g., the Legatum Center at MIT. We believe that Social Entrepreneurship without technical ICTD research is like the silicon valley without Computer Science research. There is a need to make the case for the importance of ICTD research in front of social entrepreneurs, business schools, and dedicated centers. This will be critical for funding, technology transfer, and visibility of ICTD research.

3.4 Technology Policy

More developed countries often end up dictating technology policy for developing countries. These tech policies, just like technology manufactured in the western world, are often ill-suited for the third world. There is a need for making tech policies specifically for developing regions and this should be considered an important part of technical ICTD research.

3.5 Visibility

ICTD projects have enjoyed considerable coverage in the media. However, this coverage has not yet translated into concrete benefits for the ICTD community. Interest in working in the area remains fairly low and technical ICTD workshops receive few (less than 50) submissions each year. There is a need to promote ICTD as a "serious" field of scientific inquiry. This should be done by leveraging the media interest in ICTD projects. We are not arguing for creating a "media hype" around ICTD research, but for carefully using media outlets as a tool for increasing the interest level in ICTD research.

4 Conclusion

In this paper, we barely touched the surface of some road blocks in ICTD research today and pointed out more problems than solutions. This list of missing pieces is far from complete, but we believe that it presents the most imminent problems.

References

[1] A. Badam, K. Park, V. S. Pai, and L. L. Peterson. Hashcache: Cache storage for the next billion. In *USENIX NSDI'09*, pages 123–136, Boston, MA, April 2009.

[2] R. Patra, S. Nedevschi, S. Surana, A. Sheth, L. Subramanian, and E. Brewer. WiLDNet: Design and implementation of high performance wifi based long distance networks. In *USENIX NSDI'07*, pages 87–100, Cambridge, MA, April 2007.

ICTD in Academic Computer Science

Saman Amarasinghe
Massachusetts Institute of Technology
saman@mit.edu

I believe that there are two main benefits of having ICTD an academic subfield of Computer Science. First, it'll make it possible to make students who are choosing ICTD as a career be better trained and prepared. Second, having dedicated university researchers will bring about research breakthroughs to ICTD.

Anecdotal evidence suggests that, during the last couple of years a non-trivial number of computer science students have chosen ICTD as a career path. However the Computer Science curriculum at many universities, including MIT, does not provide an opportunity for the students to learn and prepare for a career in ICTD. I believe that incorporating a class or two on ICTD into computer science curriculums will be a popular move. This will not only help the students who are looking to ICTD as a career path, but also help other students broaden their horizons and learn how to find appropriate technological solutions to hard real-life problems.

One of the main benefits of legitimizing ICTD as a subfield of Computer Science is the exciting prospect of generating innovative research in ICTD. Computer Science researchers has had an enormous impact during the last two decades. However, most of the problems addressed by computer scientists are the problems faced by them. These problems tend to address first world issues with no direct correlation to illiterate and poor people in the rest of the world. Thus, a dedicated focus on unique problems faced by more than half the world population can have a huge impact on the planet.

It is important to set the expectations of ICTD research so that the maximum effectiveness of the academic world can be brought to solving these problems. It is imperative that the field is able to articulate a research agenda. This will not only help guide the ICTD researcher but also help subfield get accepted by the larger computer science community. Jim Gray, in his 1998 Turing Award lecture describes the properties of a good research agenda as one that is simple to state with no obvious solution; one that will have a clear benefit in solving where the progress and solution is testable and can be broken into smaller steps so that intermediate progress can be measured.

We also have to avoid many pitfalls in describing the research agenda. First of all, research is not product development. Unlike other areas of computer science, where a large industry is poised to take the research and develop them into products, ICTD does not have too many industry partners yet. Thus, there will be pressures from sponsors and from ourselves to develop quick fixes to immediate problems. While a university ICTD program can look for immediate impact with undergraduate and Masters type projects, it is also important to have a long term research agenda. In this, I believe that we can find intellectually challenging problems that the broader computer science community can recognize as "hard CS" problems. For example, providing computing capabilities with minimum resources is an important problem for resource poor communities. Traditional computer science is mostly about how to use more and more resources provided by Moore's law. Thus, looking at the fundamental issue of minimum resource requirements to provide certain capabilities is a challenging and important problem. For example, recent work on passive-dynamic walkers has revolutionized robotics by drastically reducing the components required to build a walking robot. Can a similar approach be taken for devices that can have a direct impact on the poor? Another extremely interesting and challenging problem is how to provide a viable user interface to illiterate and semi literate users.

Computer Science and Global Development

Richard Anderson
Department of Computer Science and Engineering
University of Washington
Seattle, Washington, USA
anderson@cs.washington.edu

1. Introduction

There is a growing community inside of computer science and engineering that is pursuing computing for global development. This community is based in academic departments at major universities, is engineering focused in aspiring to develop technologies that can be used, and draws primarily from people with backgrounds in computing. Inside this group, there are continual tensions in trying to figure out how to do this work in the academic context. This is expressed by concerns about publications, funding, and career advancement.

My belief is that this work is really about maximizing impact on the lives of the very poor. I believe that there is a strong likelihood that computing technology can play a role in addressing some of the problems associated with global poverty, and that there is value in pursuing this work in an academic setting. Neither of these is obviously true – and if they aren't, this effort is misplaced.

The question that I am less sure of is whether or not there is interesting computer science in the area, and it is not at all clear to me if this matters. Where is the target impact – is it inside or outside of computer science? There are many examples of subfields of computing that have external and not internal impact, and I would consider Computing for Global Development to be one of them. Of course, there is a potential problem with this argument – a subfield that only has external impact may not be self sustaining. In particular, impact that is outside of the academic settings may be very hard to measure and recognize. The key for the success of the field is to have a sufficiently large impact that its role is recognized.

2. Impact

What is the strategy for the field to have a major impact on peoples' lives? I think almost everybody in the field recognizes that is necessary to form partnerships and work with experts in application domains. There is little likelihood that computer scientists will have any success working alone, first, deployment of technology on its own is unlikely to do much good, and second, it takes significant expertise, both professional and contextual, to develop a useful technology in an application domain.

What are the "Grand Challenges" in global development where computing can make an impact? In my opinion, Global Health is the domain where there is the highest likelihood that this community will have major impact. Reasons for this include:

1. Health is one of the fundamental challenges facing low resource environments. This includes both serious diseases, such as HIV and TB, as well as chronic conditions arising from poverty and poor health practices.
2. Technology and information play a major role in developed world health systems. There is every reason to believe that this will carry over to the developing world, albeit with different cost structures and applications.
3. There are significant players in the area, and large resources devoted to global health. This means, that when computing technology provides adequate value, it may be sustainable.

There are many challenges in Global Health – here are a few specific topics where I see computing playing a major role. This list is not meant to be exhaustive and exclusion of a topic does not suggest that it is not important for our community to address. Also, there are computing related efforts going on in the majority of these topics already, so I am not implying that the community is ignoring them.

1. Assessing global health status. In many areas, we have a remarkably poor understanding of global health conditions. Even simple questions such as "how do people die" cannot be answered accurately.
2. Medical logistics. There are many very large scale efforts at providing drug treatment and vaccination. These can only be successful if the supporting logistics are taken care of.
3. Medical record systems. Central to any coordinated care is the ability to track individuals' care, which is very challenging in low resource environments which lack a health infrastructure.
4. Global Health Surveys. Related to health status is have accurate demographic data and understanding the prevalence of particular diseases. To get this data, very large scale health surveys need to be conducted.
5. Public Health Education. Many health problems could be addressed by education and by convincing people to take particular actions.

These all present research challenges and it is easy to see how small projects could be started in all of these areas which could lead to productive research and collaboration with global health practitioners. However, for significant impact, these challenges need to be escalated to visible challenges with assessable outcomes such as the 1967 challenge of eliminating small pox through vaccination. Three potential challenges from the above areas: 1) Record the cause of death for everyone who dies, 2) Track the temperature of every vaccine shipment in real time, and 3) Deliver maternal health workshops to every village on the globe.

3. Methodology

In order to contribute to the grand challenges, the computing for global development community must play to its strengths, and use the research methodologies that have had enormous impact on the development of the computing industry.

Core to computer science is systems building: to understand and innovate with the technology, to understand requirements for adoption and to evaluate results. For systems deployment this arena, a platform strategy is essential, allowing for customization, localization and extension in the field. This is because it is not possible to anticipate the full range of use across the globe and leveraging local and application knowledge is essential for wide use. These projects are inherently long term projects with significant engineering. This requires long term, large scale funding.

The field of Computing for Global Development must put more emphasis on addressing large scale problems. We should be thinking on a global scale – how to reach one to three billion people. This is something that should be natural to us as computer scientists! A small number of projects are starting to look at issues of scale, but most work is at the pilot level. Of course, it is necessary to make sure that things work on a small scale first – so this is not a criticism of current affairs. Issues are qualitatively different between projects with one site, ten sites, and one hundred sites. One thing that the community is going to need to understand is how to do research on scalability without going to full scale.

How does the field treat negative results? First of all, true negative results are very valuable for understanding what is going on, as well as to avoid repeating failures. Well constructed studies, which show things are difficult are a clear contribution, but there are a lot of other types of negative results, work that falls into categories such as: “should have known better”, “screwed up on implementation”, and “methodologically unsound”. It can be difficult to evaluate negative results and determine if there is a contribution. Perhaps, this is an area that our publication venues can pay close attention to.

4. Community

Where does the Computing for Global Development community fit? The ICTD conference is very successful, attendance has been growing rapidly, and paper quality is increasing. One of the successes of ICTD is that it brings together many groups. The conference includes both engineers and social scientists, and that seems to work well, with some minor clashes in culture. There are also a set of non-academic communities present at the conferences – people working in global development, NGO’s, independent researchers and small companies. In my opinion, there is tremendous value in linking in the non-academic groups.

Should we establish a computing for global development community that is smaller than ICTD, and as academic communities are linked to conferences, have a technically focused conference? There is the obvious analogy with CHI and UIST. There are risks and benefits of this. Technology work without a broader viewpoint has a serious risk of irrelevancy. The worst thing that we could do is to establish a “technology first” agenda. On the other hander, there are challenging aspects of getting the technology right which includes addressing core CS topics. Having a tighter technical community (in addition to ICTD) could make it easier to evaluate some types of work, and it could also strengthen position of junior researchers in the field.

Social mobile: Myths and misconceptions

by kiwanja

A couple of weeks ago - in "The long tail revisited" - I briefly touched on the topic of "myths in the social mobile space". It wasn't the major focus of the post, but as is often the case it kicked off a completely separate discussion, one which took place largely off-blog in the Twitterverse and via email. I've been thinking more about it since, particularly as the social mobile space continues to hot up and people begin to face tools and projects off against one another - sometimes for the right reasons, more often for the wrong.

So, here's my current "Top Ten" myths and misconceptions in this emerging field. Feel free to add, remove, agree, disagree, debate or dismiss. In no particular order…

1. "High-end is better than low-end"
Firstly, one mobile tool should never be *described* as being better than the other - it's all about the context of the user. There is just as much a need for a $1 million server-based, high bandwidth mobile-web solution as there is for a low-cost, SMS-only PC-based tool. Both are valid. Solutions are needed all the way along the "long tail", and users need a healthy applications ecosystem to dip into, whoever and wherever they may be. Generally speaking there is no such thing as a *bad* tool, just an *inappropriate* one.

2. "Don't bother if it doesn't scale"
Just because a particular solution won't ramp-up to run an international mobile campaign, or health care for an entire nation, does not make it irrelevant. Just as a long tail solution might likely never run a high-end project, expensive and technically complex solutions would likely fail to *downscale* enough to run a small rural communications network. Let's not forget that a small deployment which helps just a dozen people is <u>significant</u> to those dozen people and their families.

3. "Centralised is better than distributed"
Not everything needs to run on a mega-server housed in the capital city, accessed through "the cloud". Okay, storing data and even running applications - remotely - might be wonderful technologically, but it's not so great if you have a patchy internet connection, if one at all. For most users centralised means "remote", distributed "local".

4. "Big is beautiful"
Sadly there's a general tendency to take a small-scale solution that works and then try to make a really big version of it. One large instance of a tool is <u>not</u> necessarily better that hundreds of smaller instances. If a small clinic finds a tool to help deliver health care more

effectively to two hundred people, why not simply get the same tool into a thousand clinics? Scaling a tool changes its DNA, sometimes to such an extent that everything that was originally good about it is lost. Instead, *replication* is what's needed.

5. "Tools are sold as seen"
I would argue that everything we see in the social mobile applications ecosystem today is "work in progress", and it will likely remain that way for some time. The debate around the pros and cons of different tools needs to be a constructive one - based on a work in progress mentality - and one which positively feeds back into the development cycle.

6. "Collaborate or die"
Although collaboration is a wonderful concept, it doesn't come without its challenges - politics, ego and vested interests among them. There are moves to make the social mobile space more collaborative, but this is easier said than done. 2009 will determine whether or not *true* non-competitive collaboration is possible, and between who. The more meaningful collaborations will be *organic*, based on needs out in the field, not those formed out of convenience.

7. "Appropriate technologies are poor people's technologies"
A criticism often aimed more broadly at the appropriate technology movement, locally-powered, simple low-tech-based responses should not be regarded as second best to their fancier high-tech 'Western' cousins. A cheap, low-spec handset with five days standby time is far more appropriate than an iPhone if you don't live anywhere near a mains outlet.

8. "No news is bad news"
For every headline-grabbing mobile project, there are hundreds - if not thousands - which never make the news. Progress and adoption of tools will be slow and gradual, and project case studies will bubble up to the surface over time. No single person in the mobile space has a handle on everything that's going on out there.

9. “Over-promotion is just hype”
Mobile tools will only be adopted when users get to hear about them, understand them and are given easy access to them. One of the biggest challenges in the social mobile space is outreach and promotion, and we need to take advantage of every opportunity to get news on available solutions - and successful deployments - right down to the grassroots. It is our moral duty to do this, as it is to help with the adoption of those tools which clearly work and improve people’s lives.

10. “Competition is healthy”
In a commercial environment - yes - but saving or improving lives should *never* be competitive. If there’s one thing that mobile-for-development practitioners can learn from the wider development and ICT4D community, it’s this.

Biography

Ken Banks, founder of kiwanja.net, devotes himself to the application of mobile technology for positive social and environmental change in the developing world, and has spent the last 15 years working on projects in Africa. Recently, his research resulted in the development of FrontlineSMS, a field communication system designed to empower grassroots non-profit organisations. Ken graduated from Sussex University with honours in Social Anthropology with Development Studies, and was awarded a Reuters Digital Vision Fellowship in 2006, and named a Pop!Tech Social Innovation Fellow in 2008. Ken's work has been supported by the MacArthur Foundation and Open Society Institute, and he is the current recipient of a grant from the Hewlett Foundation

Further details of Ken's wider work are available on his website at www.kiwanja.net

CS is Not Enough: The Need for Breadth in ICTD

John K. Bennett
ATLAS Institute; Dept. of CS
University of Colorado at Boulder
jkb@colorado.edu

S. Revi Sterling
ATLAS Institute
University of Colorado at Boulder
revi.sterling@colorado.edu

ABSTRACT

Despite huge investment, and the best of intentions, most development projects fail. This is particularly true of Information and Communication Technology for Development (ICTD) projects. We contend that a significant contributor to this failure is the lack of breadth in project design and implementation, and in the training of project implementers. Successful ICTD interventions, in addition to being based on the best computer science research has to offer, must be guided by the relevant social, cultural, political, economic and gender factors that underlie the interaction of the technology with the community into which it is being placed. We therefore argue that efforts to distance computer science from the broader context of ICTD scholarship and practice are misguided. ICTD should be recognized as a truly interdisciplinary area of research and practice. We further argue that ICTD as a discipline has a particular need for academic practitioners. We briefly outline a new training program for such individuals.

Categories and Subject Descriptors

K.4.0 [**Computers and Society**]: The fact that the ACM Computing Classification System has no entry actually relevant to the application of computing to development is emblematic of one of the concerns of this paper.

General Terms

Human Factors. Again, there is no appropriate choice among the sixteen alternatives.

Keywords

Information and Communication Technology for Development.

1. COMPUTER SCIENCE AND ICTD

1.1 The Digital Divide

The majority of the world's population does not have adequate access to information or communication. Roughly four billion people (two-thirds of the world's population) live on less than two dollars a day. Among this group, only 1 in 160 has any form of access to the Internet. Illiteracy, lack of education and training, lack of power and communication infrastructure, and higher priority community development objectives combine to limit the effectiveness of efforts to introduce information and communication technology (ICT)-based development solutions. People in developing communities usually cannot benefit from the introduction of telemedicine, distance education, e-government, and other sophisticated Internet and technology-based strategies that are prevalent in the developed world.

1.2 The Need for Breadth

Despite billions of dollars invested with the best of intentions, there is a demonstrable lack of success in achieving global and local development objectives. This is particularly true of Information and Communication Technology for Development (ICTD) efforts. We contend that a significant contributor to this failure is the lack of breadth in both the implementation of development, and in the training of those who practice development. Those who work in development tend to communicate only with those most aligned with their field. For example, food security experts rarely attend community informatics conferences; health experts rarely attend ICTD conferences. Even researchers and practitioners within the same community rarely cross paths (especially when there are significant geographical distances between them), and journals go unread by the communities who have contributed to the findings reported therein, who are therefore unable to put the recommended outcomes into action. It is little wonder that the needle barely moves in the overall progress of under-developed communities.

Many of these concerns have been articulated in the three ACM/IEEE ICTD conferences to date. However, while attendance at this conference is continues to grow, many of the presenters and attendees are the same from year to year – a predominantly computer science-oriented group of researchers and practitioners who themselves bemoan the disconnect in ICTD between the "ICT" and the "D." This separation is largely the result of both legacy and timing. Development studies has been an area of scholarship and practice for over 50 years; mass communications departments have been working in ICTD (although it is called development communications in this space) since the 1960s. Computer science is late to the game – yet often does not adequately acknowledge or build upon development's historical foundations. The underlying view that computer scientists will save the world because other development sectors have not yet been successful in doing so is naïve and overlooks the complexities of real-world development.

Information and Communication Technology *is* a critical component in global change strategies, but it needs to be considered as a component of development rather than as a stand-alone development sector. The eight UN Millennium Development Goals (none of which list ICTD access, use and capacity building as top goals) have encouraged numerous articles about the transformative role that ICT can and should play in attaining these goals. While the MDGs perhaps can be criticized for their normative approach to global iniquities, they have galvanized and coordinated efforts across industry, academia, practitioners, multilateral agencies and foundations, and NGOs. ICTD, especially the ICTD efforts grounded in computer Science, would do well to be as inclusive and cohesive.

1.3 ICT for Development vs. ICT and Development

Mainstream computer science research has the potential to drive ICTD innovation, while at the same time contributing to mainstream "First World" research and development efforts. There are few limits to the hardware and software systems that computer science can bring to bear upon the seemingly-limitless problems that result from sustained community and regional under-development. The current approach – creating technologies based primarily upon our understanding and standpoint – perpetuates a model of ICT *and* Development, where we are technical experts whose talents can be used in development interventions. In contrast, creating technologies that have the potential to catalyze social change, and mapping human needs to technologies that directly respond to specific development problems represents ICT *for* Development.

In a time of stagnant or declining enrollments matched with anxiety myths about the health of the technology sector, ICTD is a natural draw for students and faculty who aspire to greater impact. Yet, academic instruction and research in ICTD is limited to the extent that interdisciplinary lineages exist and support ICTD done "right." There is a critical need to develop institutional infrastructure and funding support models for the academician who is 50% computer science; 25% mass communication, information systems and sociology; and 25% development studies. This need is not unique to computer science departments; social science and humanities programs that have traditionally been the home of graduate development students cannot remain effective without the ability to leverage the power and near-ubiquity of modern ICT. However, the fear of the multidisciplinary field pervades – how can we ensure sufficient depth in the presence of breadth? This argument is not new – interdisciplinary research and education programs routinely face such questions, despite demonstrated funding and research success.

Universities tend to be conservative when it comes to organizational structure. Even as we argue against academic silos, we fight to preserve the purity and primacy of our historic intellectual turf. This contradiction is emblematic of the need cross-cutting academic programs that can focus the intellectual breadth of the entire campus upon ICTD education and research, and in which faculty (especially junior faculty) are rewarded for applying their domain proficiency in larger context of development.

1.4 The Need for Academic Practitioners

Universities have long enjoyed a reputation for advancing learning through service, although the reality is that many such efforts contribute to a schism between research and praxis. Given the potential for ICTD to support a rich assortment of development strategies, universities need to graduate experts who can help bridge the gap between the advantages of the networked information society, and those with the greatest potential to benefit if issues of access, social equity, sustainability, and appropriate design and distribution are addressed. However, the focus has to remain on people - if ICTD experts are not focused on actually serving human need, it's not development.

As a research area, ICTD is just now emerging as a clearly identifiable focus – there are perhaps a half dozen respected ICTD journals, and the premier conference in the field is less than four years old. Although ICTD is emerging as a formal discipline at several of universities internationally, only a few programs related to ICTD exist in the United States. These programs primarily cater to the doctoral student, although there is a trend towards master's level programs, including ICTD certificate curricula and the announcement of two Master's degrees in ICTD to bring the total "practitioner" programs worldwide to six – of which five are in the European Union. Of the 100 ICTD courses taught at Universities worldwide, only 20% are taught in computer science departments. If computer science is late to the game, American universities are also overdue in recognizing the value of the ICTD academic practitioner as a driving force for effective ICTD development and deployment, whose efforts will in turn sustain ICTD research and scholarship.

2. ONE APPROACH: THE ATLAS MS-ICTD

The ATLAS Institute at the University of Colorado at Boulder expects in Fall 2010 to welcome its first class in a Master of Science in ICTD degree program. This program represents part of our commitment to challenge both the academic silos and chasms in development that serve to perpetuate the inequities of underdevelopment.

The University of Colorado has a strong community commitment to development studies, and many of the academic partnerships in technology and the social sciences that are necessary to anchor this program already exist. The MS-ICTD program will build on existing on-campus expertise and strength in many of the core areas that define the discipline, and will provide a focal point for the growing number of CU Boulder faculty who are exploring various pieces of the ICTD puzzle. The research challenges of ICTD are both significant and highly relevant, and the inherent interdisciplinary nature of ICTD complements the mission of ATLAS – to create interdisciplinary programs that make people more capable learners, more innovative teachers, more creative thinkers, more effective leaders and more engaged global citizens.

The MS-ICTD program will train academic practitioners to strategically and efficiently utilize technology to help facilitate health, education, civil service and poverty alleviation initiatives all over the world, as well as to connect these efforts to amplify their impact. To this end, students will specialize in the technical and social aspects of ICT while acquiring a broad foundation in development studies, public health, social sciences and assessment methods, in order to make a positive difference in the complex and interrelated systems of community and economic development. In order for ICT-based development interventions to succeed, technological considerations must be balanced with social, cultural, political, gender and other issues not related to the chosen technology. This balance is the foundation of the ATLAS MS-ICTD program.

Workshop on Computer Science and Global Development

Michael L. Best
Sam Nunn School of International Affairs and School of Interactive Computing
Georgia Institute of Technology
Atlanta, GA 30332 USA
+1 404 894 0298
mikeb@cc.gatech.edu

ABSTRACT

I present here an informal position paper offering a set of grand challenges that arise out of my personal research interests (post-conflict computing, HCI4D, appliances, sustainability, and nomadicity). I believe that this set of questions, when complimented with and harmonized to the core problems identified by others, will result in an important list of grand challenge questions. I then discuss the challenges in establishing mutual respect within this interdisciplinary area of inquiry within computer science and across computer and social sciences. Finally I end with some thoughts on broadening the funding interests of foundations and multi- and bilaterals to include question formulation and theory building and the prospects of building NSF interest in this space.

1. HILBERT PROBLEMS

At the start of the 20th century the German mathematician, David Hilbert, proposed a set of foundational research questions designed to help focus the attention of mathematicians on the most important questions of their time. These Hilbert Problems helped define much of the last century of mathematical research (and led to a good number of surprises and scandals). The computer science and global development community, hereafter called ICT4D, needs to develop a similarly weighty list of grand challenges that will focus our collective attention and help us track forward progress as it is made.

My list of proposed Problems is not complete (in keeping with the spirit of Hilbert) but represents just some of the core challenges that I personally have taken on. When combined with the grand challenges identified by others I hope that a mature list of critical issues will emerge.

1.1 Post-conflict computing

A number of unique challenges are present in immediate post-conflict settings and these settings call into questoin many of the assumptions of traditional computer science or even ICT4D. For instance a common troupe is that penetration rates for computers, mobile phones or the internet are monotonically on the rise. But many conflict and post-conflict settings have seen precipitous wartime declines in ICT penetrations. Or an assumption is made that electric grids are at least available in capital cities. But this is not always the case in post-conflict capitals. A strong research program in post-conflict computing will explore the necessary technologies, policies, institutions, and theoretical framings that will best connect ICTs to peace and reconstruction.

1.2 HCI4D

Why do we deploy *personal* computers into places where the technology is shared and not kept by a single person? Do we need a *community* computer instead? What does the desktop metaphor mean in a context that does not value or use desks? Why do we rely on the QWERTY keyboard for languages that do not include the “Q”, “W”, or “E”? These are just a few of the fundamental problems in the design of usable computer systems for global development. Computer/human interaction designers have only just began to think deeply about the special challenges and needs in global development.

1.3 Appliances

A lot of debate has centered on the prominent rise of mobile phone use in low-income countries and thus whether mobiles are the technological “winners”. The ubiquity of mobile phone networks, now usually with data support, is clear. And the desirability of mobility itself is also clear. Similarly, low cost laptop initiatives have captured considerable attention with the suggestion that these particular systems will solve all of the core ICT4D problems. In reality neither mobiles nor laptops are the perfect appliance for all situations. We need to better understand what the best design and form factors are for end-user appliances regardless of the network or distribution model. When do we want to use mobile phone styled appliances, when will laptops be best, and when are desktop styled appliances best? Furthermore, do we need to design an entirely *new* appliance, something with a more appropriate display or input device or better suited to end-user sharing for instance?

1.4 Sustainability

Financial self-sustainability of ICT4D initiatives is understood as an important question requiring further examination. What also requires ongoing study is ways to ensure other forms of sustainability: environmental, technological, social and cultural, political and institutional. Work in computer science can touch on all of these forms of sustainability. For instance technical sustainability will be enhanced by easy to use systems or systems that allow for remote maintenance. Similarly, environmental sustainability is enhanced by low-power consuming devices.

1.5 Nomadicity

While mobility is a key affordance, problems of nomadicity, either permanent mobility or periods of mobility without a home base, establishes a context where important computer science research can create relevant solutions. Populations are increasingly on the move, from job seekers who must move every few months to find work, to those displaced by conflict, to transnationals in the diaspora. Empathic systems and networks are needed that will be responsive to the special requirements of these populations.

.

2. METHODS, METRICS & IDENTITIY

Emerging from the ICTD conference in Doha and our own Computing at the Margins symposium held recently in Atlanta it is clear that the ICT4D field is struggling with methods, metrics, job prospects, and identity. A plurality of methods and metrics is probably required for ICT4D to be productive. But tensions within the computer discipline (e.g. where core computer scientists question the rigor of ICT4D scholars) and between schools of inquiry (e.g. when computer scientists question qualitative social science methods) is harmful to the research program and has created a crises of identity among some young scholars. As an interdisciplinary area it seems necessary that this research domain embrace mixed methods, bring together both engineering and social science approaches, and build mutual respect while ensuring that research results remain high quality, theoretically grounded while empirically supported, and build on past experiences.

A number of PhD students in Doha, for example, stated that within their home CS department their work was not viewed as “real” computer science. They perceived a trade off between researching “exciting technologies” and finding “relevant solutions” to international development. Significantly, these were PhD students with advisors prominent in the ICT4D community.

An additional tension emerged when those coming from the CS community criticized the social scientific work as lacking rigor or importance. More interestingly to the current audience, perhaps, was the opposite viewpoint of social scientists finding the work of computer scientists immature. A number of people in Doha described the technology focused papers to me thusly: "I wanted to build a technology to do this thing. So I started to build it. I did this. Then I did that. Then I did a bit more. Then it was built. Then I asked 10 people from Ghana if they liked my thing. Nine of them did. Huzzah for my thing."

Thus there seems to be tensions around methods, metrics, problem spaces and identity between computer scientists working in ICT4D and their traditional CS colleagues in some instances as well as between computer scientists and social scientists who are also working within the ICT4D area of inquiry (as well as between practitioners and academics within this space).

Many of these issues cannot be unique to ICT4D (for instance tensions between practitioners and academics). Nonetheless these debates may be particularly important for the nascent ICT4D field especially as we consider job and funding prospects.

The lack of respect for ICT4D work within traditional computer programs (perceived or actual) may be the simplest and most immediate problem to address. The School of Interactive Computing within the College of Computing at Georgia Tech does not (I hope and believe!) maintain these prejudices. We may benefit from having a traditional computer science department sitting alongside this less traditional department. Not all universities will have such luxuries. Information schools have also begun to hire into this space and these departments will have differing computer science expectations.

The above issues of identity notwithstanding we should note with pleasure the placement of a first generation of tenure-track faculty hired to work explicitly and from the outset in ICT4D areas (at Tech, UCLA, UCB, CMU, where else?). This as opposed to already tenured faculty transitioning into the ICT4D space; a wonderful occurrence but something that may not represent the creation of new ICT4D job opportunities in the academy.

3. FUNDING

The ICT4D research program opens up new funding sources that have not been traditionally available to computer science programs. For instance, foundations, multi- and bilateral organizations have at times provided significant investment in this space. These funding streams need to be enhanced especially as some groups have recently been “mainstreaming” their ICT4D programs (out of existence).

Additionally, these funds generally focus on project-centered interventions. It has been hard to generate funds for more conceptual or theory building work. However this sort of work is critical if we are to build a progressive area of inquiry. Thus these funding streams must be sustained while also broadening to include more conceptual and analytic work.

The NSF, traditionally the source of funds for computer science programs (and funds that naturally come with significant conceptual and core science support), has considered funding more programs in this space. They should be encouraged to develop a funding program that centers on ICT4D activities along with domestic USA focused research in a similar vein (e.g. ICTs for economic development amongst the urban poor in the USA); at Tech we call this domestic/international hybrid Computing at the Margins. We have been in discussions with the NSF to convene a workshop tasked with exploring the development of a new funding program in this space. The outcomes and participants of this CCC workshop should certainly inform the development of this NSF workshop.

Computer Science and Global Development

Edwin Blake
University of Cape Town
Department of Computer Science
edwin@cs.uct.ac.za

ABSTRACT

This position paper argues that we should set ourselves the Grand Challenge of creating tools that people in the developing world appropriate for their own purposes.

I suggest that one such Grand Challenge is a tool that makes all telcos obsolete. Our allies in this might be the mobile handset manufacturers.

I also argue for community based values and methods that embrace the egalitarian principles of Action Research. I point out that Monitoring and Evaluation Tools are useful as a metric for development projects but need to be adapted for ICT Design.

1. GRAND CHALLENGES

I want to nuance the need for a "Grand Challenge" approach to Computer Science and Global Development. As a working definition of "Grand Challenge" in this context I'll use "a specific critical barrier that if removed would help to solve an important Global Development problem".

My own experience over a number of years is to see many large policy initiatives come and go with very little lasting effect. One of the early failures was the notion of Telecentres (or Multi-Purpose Community Centres) which were rolled out in South Africa as a way of bringing ICT benefits to disadvantaged communities from about 1998 [1]. The one size fits all approach, even with the best of intentions, leads to implementation failures.

I now think that the mindset of using Grand Approaches in tackling development must be limited to a very specific area, namely that of creating technological tools that people can take over for their own purposes. (This is not to say that Grand Challenges aren't an effective way to mobilize funding and research effort in the Developed World.)

I believe that tackling actual problems in specific places cannot be dealt with in a grand fashion; rather tackle them by enabling many small scale initiatives which people can themselves adapt to their needs. Unfortunately, in terms of mobilizing resources, saying that you want tools to deal with the Billion-and-One challenges of situated development might not be the catchiest of slogans.

The reason I am excepting "tools" from the restriction is that an enabling technology at a low enough level (consider mobile phones, but see below for caveats even there — Section 1.2) empowers people to find their own solutions. Giving people the power to improve their own lives seems to me the best way to achieve real and lasting development [2].

1.1 Why Are Tools the Exception?

The real reason building tools with a grand scope does not seem to violate Easterly's criticism of Big Push approaches is because tools inherently defeat "planners". People will take over a product and use it for their own purposes. Third wave HCI seems to have embraced this. Nokia's researcher Jan Chipchase says: "however we design this stuff — carefully design this stuff — the street will take it and will figure out ways to innovate, as long as it meets base needs" [3].

1.2 Grand Challenge: Replace Mobile Telcos

I believe one grand challenge worth pursuing is to replace all telecommunications operators with a web of local meshed communications systems. Where long haul backbones are required these can be provided by National Governments: the natural monopoly holders of last resort.

To me this seems like the most basic of tools for empowerment of communities.

I believe Onno Purbo has shown the way with his model of "Bottom Up Self-Finance Community Based Approach". He emphasizes that the people in a community can manage their own upliftment if they tap into the financial resources which they already have to exploit the resource sharing that ICT can enable [4]. He argues that the "champions" in this case are the younger people form the community. He draws a distinction between the members of the community who *talk and listen* (that is, older, semi-literate and resistant to change) and the *younger generation*.

The younger generation in the developing world are keen to embrace change and technology. The South African mobile banking company Wizzit that targets the unbanked has used this same insight in its marketing by recruiting young "Wizzkids" as the lever to take its technology to the townships and rural areas [5].

So the challenge is to develop a very low-cost communication system that is locally self-sustaining and has the potential for national and inter-national connectivity.

How? Not exactly sure: there are many alternatives to centrally provided communications services. In fact the whole notion of a single national telecommunications operator seems curiously anachronistic in the Internet age.

Why? Because I think mobile telcos are becoming an impediment to further development just like their fixed line predecessors.

Mobile Phone Operators Considered Harmful

The mobile phone operators have put up a scaffold for development. It is time to thank them but to stand free from them. From now on they are turning into a burden. Many people are beginning to comment on this in their blogs: William Easterly, Steve Song [6], and Richard Heeks [7].

People are spending all their extra disposable income on mobile access. This money is leaving the community and is not fed back into local businesses. Mobile service provision under the current model is a natural monopoly and as we put more and more services onto this monopoly (take banking) we are handing more and more power to these monopolies: not a healthy situation.

2. VALUES AND METHODS

The earliest lesson that we learnt was that an approach to ICT in Development has to be community based. At the Information Society and Development (ISAD) Conference in 1996 South Africa already developed a notion of an "Information Community"[1] as opposed to the information society or information superhighway. The pervasive African philosophy of *ubuntu* provides the grounds for this approach since it regards one's identity as a human being as causally and even metaphysically dependent on a community (which may include the living and the dead). In deriving principles of right action from this, the philosopher Thaddeus Metz has come up with statements like: "*An action is right just insofar as it promotes shared identity among people grounded on good-will; an act is wrong to the extent that it fails to do so and tends to encourage the opposites of division and ill-will*" [8].

Not only do these values argue for a community based approach to ICT, they also point to a community based approach to research ethics. This is an approach in which researchers and target users are equal members of the same community. It seems to me that such an approach leads to some kind of *Action Research* where the legitimate needs of the users for action are combined with the equally legitimate needs of the researchers for research results.

The cyclical nature of action research where questioning and reflection are tied to intervention neatly solves the need of users to learn about ICT while the researchers learn about the community within which they are working. Many popular ICT development methods have assumptions, frequently unarticulated, about users' knowledge of information technology artefacts, one such is participatory design. Such approaches have largely failed to meet the challenges of ICT Design for Development.

3. METRICS

We have looked at adapting some of the metrics used by development agencies in order to evaluate our projects for socio-economic development in deprived areas. Our projects essentially constitute a design problem. When we used Monitoring and Evaluation (M&E) Tools in ICT design we first had to acknowledge that this was not the intended use of these M&E tools.

The advantage is that such tools have focussed for a long time on development issues and are sophisticated instruments for evaluating impact, sustainability and so forth. We have used both the *Real Access/Real Impact* criteria of bridges.org[9] and the *Outcome Mapping* method developed by the IDRC [10]. In both cases we were assisted in this by experts from these organizations.

The problem with using M&E Tools for design is that they do not readily provide design guidelines. One relatively easy way to incorporate such tools in design projects is to use them in the evaluation cycle of one of the modern iterative and agile software design methods, or equivalently in the evaluation phase of an action research cycle.

That still does not remove the fact that such tools do not really give the ICT designer assistance such as, for example, design patterns. More subtly, from the point of view of an ICT designer, M&E tend to treat ICT in a static fashion and not as something that is easily mutable and adaptable. This means planning incremental adaptation is more difficult.

4. CONCLUSION

While I do not think that Grand Projects are an effective way of dealing with development, this does not mean that producing good tools for people in developing countries to solve their own problems is a bad idea. We should try to set up the grand challenges so that they produce such tools.

One of the most important tools to aim for is one that turns the provision of communication services into one of "plumbing" communication pipes. By this I mean a activity that involves a large number of agents with relatively simple ICT skills and no large and expensive communications monopolies to feed. It may be possible to recruit the telephone handset providers in this drive: Nokia has already decided to build Skype into their N-series mobile phones, now we need a developing-world-phone that is capable of peer-to-peer communication.

5. REFERENCES

[1] Benjamin, P. (2001) Reviewing Universal Access in South Africa. SA J. Information & Communication, Issue 2. www.sajic.org.za/index.php/SAJIC/article/view/SAJIC-2-4/105

[2] Easterly, W. (2006) The White Man's Burden: Why the West's Efforts to Aid the Rest Have Done So Much Ill and So Little Good, New York, The Penguin Press.

[3] Chipchase, J. (2007) TED Talks: Jan Chipchase on our mobile phones www.ted.com/index.php/talks/jan_chipchase_on_our_mobile_phones.html

[4] Purbo, O.W. (2003) ICT for Poverty Alleviation. idkf.bogor.net/onno/ict-for-poverty-alleviating-7-2003.rtf accessed 13 may 2009.

[5] Fisher-French, M. (2008) Wizzit strategy works. Mail & Guardian Online. www.mg.co.za/article/2008-09-24-wizzit-strategy-works

[6] Song, S. (2009) Mobiles in Africa - We Need The Eggs, 29 April 2009. manypossibilities.net/2009/04/mobiles-in-africa-we-need-the-eggs/

[7] Heeks, R. (2008) Mobiles for Impoverishment?, 27 Dec 2008. ict4dblog.wordpress.com/2008/12/27/mobiles-for-impoverishment/

[8] Metz, T. (2007) Toward an African Moral Theory. J. Political Philosophy: 15,3 321–341 dx.doi.org/10.1111/j.1467-9760.2007.00280.x

[9] bridges.org. Real Access / Real Impact criteria. Accessed 12 May 2009. URL: www.bridges.org/Real_Access

[10] Earl, S. Carden, F. and Smutylo, T. (2001). Outcome Mapping: Building Learning and Reflection into Development Programs. Ottawa, Canada: IDRC. www.idrc.ca/en/ev-9330-201-1-DO_TOPIC.html

[1] See www.cs.uct.ac.za/~edwin/OldWeb/isad-pm/node6.html

Position Statement
"CCC Workshop on Computer Science and Global Development"
Berkeley, CA – 1 August 2009

Gaetano Borriello
Computer Science & Engineering
University of Washington
Seattle, WA

There are two inter-related points that are important for the community to discuss, and hopefully, agree upon to a large degree:

1. The design space of computing is huge and we only explore a small piece of it when we constrain ourselves to conditions present in highly industrialized societies. Developing technology for the developing world will cause us to look at much more robust systems that require less maintenance and infrastructure.

2. Computing needs to adopt a large element of "clinical research" as well as "fundamental research" as is the case for many other professions. It needs to embrace an inter-disciplinary approach in its fullest sense. Deep dives into applications will inevitably lead to new insights for where computing can generate the most benefit.

Behind these two points is the underlying assumption that some of the best research is accomplished when trying to solve real problems with real users for real societal benefits. This is the fundamental tenet of academic engineering and we, as computer engineers, as well as computer scientists, need to embrace it even more fully than we have in the past.

Position 1: The design space we typically explore is only a small part of what is really out there.

When working in our developed world environment, much is taken for granted. So much so that we do not fully explore the design space and often leave behind what may be important solutions. The best way to explain this is through a set of examples.

Consider early childhood education and simple math and writing drill problems. Toyama looked at this space in India and came up with "multi-mouse" – attaching several mice to a single PC because of the wish to give each child a way to interact in environment with few resources [1]. What he discovered is that collaborative use of a single PC can have benefits over a PC for each child in that it promotes a more social, competitive, and supportive atmosphere in learning. This has led to several investigations of multiple pointers and actors in an application and spurned the development of new game concepts that are easy to set up and do not require networking of devices (which can be difficult to maintain).

A second example comes from studying the transportation information needs of people in a capital city in Central Asia [2]. In this case, mobile phone penetration was much higher than household PCs. People wanted the information accessible on their mobile devices but were not connected via Internet data plans. The solution was to build a system based solely on SMS, where even the server is simply a laptop with a phone gateway. Of course, there were many details surrounding the cost of sending SMS and collecting bus locations to serve. This has spurred interest in business/advertising models for this type of service and even ways to host it entirely on mobile phones (a cloud of phones) so that services such as these can grow from grass roots efforts with minimal barriers to getting things setup and running. These models have not been attempted in years of similar services being available in the developed world and, yet, the developed world models may end up being more robust and easily maintained.

Position 2: CSE should adopt a clinical research aspect to complement fundamental research.

Computing researchers cannot expect to solve meaningful problems in development without teaming up with researchers who understand the problem space well. Fundamentally, the problem has to be tackled by a team that can bring the appropriate set of tools, approaches, and solutions from a set of disciplines to bear on the problem and integrate them into a coherent solution.

The public health space has received a lot of attention. An example of work in this space is making clinic visits more efficient with better adherence to established protocols [3]. The e-IMCI work started with a simple premise, namely, that an electronic version of a booklet-based flowchart would be more efficient. However, there were several discoveries after their deployment in a clinic: (1) clinicians gained prestige through the use of an electronic device and this led to more use and better adherence, (2) they started to understand the process better because they could see how the answers to their questions got to them to the next question, (3) the data from the visit was already in electronic form and could be easily added to an electronic health record. The CSE work was focused primarily on HCI issues; however, public health researchers also learned valuable lessons.

Revisiting the transportation example above, ethnographers did much of the early work in understanding the information needs of the people in the region and their preferred methods for getting that information. CSE researchers stepped in to propose a solution that met these new constraints and now has led to new directions in content-creation and service deployment that were outside the realm of expertise of the ethnographers. This new work will add a new tool to the "clinician's toolbox" that will be used to solve new problems. For example, in how resource-poor societies can optimize the movement of goods rather than just people and leverage their social networks to establish trust.

Summary

The main point of this position paper is that there is much to be gained by engaging CSE researchers in the problems of global development – inefficient work practices, inefficient communication and aggregation of data, robust solutions that require minimal maintenance, local creation of content that is locally relevant, enabling services with minimal support and infrastructure and supporting evolution into increasingly sophisticated models, etc. Often, working with other disciplines can help all involved gain valuable insights while focusing on where their expertise is best applied rather than trying to do it all. Projects, as in core CSE, need to be deeply engaged with the people who have the interest in deploying and sustaining the activity so that it has real benefits. A clinical research approach can help institutionalize this type of interaction. It is time we enrich the models that can lead to interesting CSE research and engineering. In the process, we will motivate a much larger population of students to join us.

[1] U. S. Pawar, J. Pal, R. Gupta, K. Toyama: Multiple mice for retention tasks in disadvantaged schools. CHI 2007: 1581-1590
[2] B. DeRenzi, N. Lesh, T. Parikh, C. Sims, M. Mitchell, W. Maokola, M. Chemba, Y. Hamisi, D. Schellenberg, G. Borriello: e-IMCI: Improving Pediatric Health Care in Low-Income Countries. CHI 2008: 753-762
[3] B. Kolko, R. Anderson, G. Borriello, C. Lustig, A. Poon. Building a Transportation Information System Using Only GPS and Basic SMS Infrastructure, ICTD09.

Discovery Matters

Eric Brewer
Computer Science
UC Berkeley

One of the great challenges of ICT research is that in many cases ***the core of research is not the resolution of a problem but rather the discovery of the details of that problem***. Unlike traditional subfields, the problems of developing regions are rarely obvious before at least a few iterations towards a solution. Thus, many high-impact ICT projects tend to have simple solutions, once the problem is fully understood.

Exacerbating this first problem is a second one: CS reviewers tend to reward complex solutions over simple solutions, as the simple solutions are somehow less impressive. Most research papers work hard to present a clean definition of the problem followed by the solution and its evaluation. This basic format inherently values the solution and devalues the definition of the problem, which is assumed to be clear and known a priori.

Discovering the real problem should be viewed as real research, although as a process it may share more with social science than engineering. Ironically, there is perhaps more science in problem discovery than there is in engineering the solution, but only the latter is valued by most computer scientists.

The HCI community is in some sense an exception to this generalization, in part because has already accepted qualitative methods as legitimate contributions, and is inherently more multi-disciplinary than most of computer science. Both ICT and HCI conferences thus provide some forum for problem discovery.

However, the community would benefit from more explicit acknowledgement and appreciation of problem discovery. Once discovered, many solutions are possible and likely more lessons come from problem discovery than from any particular solution. Papers should be able to claim "problem discovery" and be treated as a contribution even the end solution turns out to be simple.

Persuasive and Motive ICTD

John Canny
UC Berkeley
Berkeley, CA, USA, 94720
1+510-642-9955
jfc@cs.berkeley.edu

ABSTRACT

ICTD projects face major hurdles during *design* and *deployment* stages. During design, the challenge is to discover the contextual constraints, opportunities and challenges, and to find an economically-sustainable solution. Most technical ICTD projects focus on the design phase. But deployment is often a major challenge even for well-designed technologies. Deployment injects new technology and new practices into societies with extremely strong traditions and localized trust networks. By definition, they require breaks with tradition and trust of actors outside of normal local networks. Such change in practices (and concurrent attitudes) triggered by external actors is a *persuasion* problem. Hence we highlight here the importance of *persuasive* ICTs. In addition, large-scale projects in many areas such as health care, education, and service work rely on local workers who are already persuaded (and tasked) with new practices but who face much greater difficulties than their counterparts in the economic north. These challenges include poor training and education, poor infrastructure, weak oversight and supervision, corruption, and lack of respect and cooperation from the people they try to serve. We believe ICTs can have a critical role in helping these actors as well. The approach needed involves *motive* ICTs, i.e. those that enhance motivation and consequently performance of key workers. In effect we propose to broaden the emphasis from economic sustainability to *socially-sustainable* solutions.

Keywords

ICTD, persuasion, motivation, sustainability, design, deployment, education, training, health care

1. INTRODUCTION

Persuasion has a long history in innovation in developing regions. One of the most famous works on is Everett Rogers "Diffusion of Innovations" [10]. Rogers' work explored the role of social networks in diffusion of new technologies. Rogers work showed that the "medium," i.e. the chain of key actors such as change agents, opinion leaders, early adopters etc., is as important as "the message," e.g. the methods and value of high-yield farming. Rogers work also highlighted the deep social rifts that can occur when traditional practices clash with innovations originating from outside. E.g. green revolution farming has had tremendous economic impact, but it is a revolution that took decades to unfold.

In this paper we focus on the attitude and behavior changes that are typically involved in the successful deployment of innovations. We claim that ICTs are excellent substrates for persuasion, and that they can be much more potent than traditional media such as print and television. The choice of persuasive strategy and content is still critical however. ICTs such as cell phones can enable new strategies, but they do not by themselves have persuasive impact.

Our second focus is on motive technology. Here we focus on actors who are working to deploy an innovation (e.g. in health care, education, services), but who face much greater challenges than workers in developed countries. One again, we believe ICTs have exceptional potential to assist these workers.

2. PERSUASIVE TECHNOLOGY

Persuasive technology draws from work in psychology on persuasion and attitudes [11] [4] [7]. Recently there has been much interest in persuasive technology generally [5], persuasive games [3] and persuasive mobile technology [6]. Rather than attempting a survey of this work, we will select a few approaches that seem most promising for use with appropriate ICT in developing regions.

2.1 Dialogic Presentation

In [9], Ramachandran and Canny showed that a dialogic presentation is more persuasive than a lecture format. Specifically, persuasive messages were presented to subjects in one of two ways, both using voice recordings from the same actress. In one case, the information was given as a paragraph-length block – this was the lecture style. In the other case, the information was broken into sentence-length utterances, and listeners had to prompt for the next utterance – this was the dialogic format. Dialogic presentation is a natural format for presentation on cell phones (which are conversational media), and that was the motivation for the study just mentioned. It should also be a good match for users with limited schooling. The earlier study was conducted in the US with well-educated subjects, although the motivation for this line of work was to develop better persuasive techniques in developing regions. For the latter subjects, dialogic presentation should closely mirror natural interaction with other actors, while lecture presentation would appear to be quite unnatural.

2.2 Authorities

People in all walks of life are influenced by actors who they believe to be authorities on a topic. Authority is institutionalized via professional schools, government agencies and review boards and the notion of "expert" on health, technology or law is non-problematic in developed countries. But while such networks exist in developing countries, their reach often fades as one moves down the economic or educational pyramid. Unfortunately, corruption, racial and religious conflicts are all part of the reality on the ground in many developing countries. And even well-meaning non-government agencies may have goals that cross-cut or even conflict with the population they seek to empower. There is much mistrust of outsiders that must prima facie be taken as legitimate.

In rural India, trust networks are strong within the village and key authorities include village council members and local priests. Indeed it seems their influence extends to most issues, and there is less emphasis on “expertise” in a specific area. The support of these actors is often critical for the success of a project, but there is no way to guarantee it. At best, one can try to apply the other persuasive techniques listed here to recruit these actors, and then use their influence to persuade others.

Another enormously powerful group of influencers is celebrities – e.g. Bollywood actors in India. These actors seem to benefit from the ubiquity of TV and movies in India, from soap-opera storylines, and from blurred boundaries between fantasy and reality which effectively draw them inside villagers local trust networks. They are the ideal purveyors of mass-produced persuasive messages.

2.3 Social Proof

Social proof is peer influence. Across a broad set of circumstances, people show a linear adoption probability vs. the number of their peers who have adopted. Social proof can be used as a persuasive mechanism whenever adoption by others can be monitored. By selectively presenting information about others who have adopted, a system can bias users’ perception of adoption in the peer network, which will favor their adoption of the innovation. One can also choose to present information about groups that the user believes they belong to. It’s not necessary that peers present their reasons for adopting – the mechanism driving social proof is really the user’s sense of being “normal” w.r.t. the reference group.

2.4 Logos

None of the mechanisms discussed so far address the *content* of the persuasive message. Classical rhetoric, which is the original art/science of persuasion, is based on three key forms of argumentation: Ethos, Logos and Pathos. Ethos is argument based on ethics, and Pathos is based on empathy with the speaker. Logos is solely about the message. Logos should not be confused with logical argument, although the two share the same linguistic root. Rational arguments have limited persuasive force even for well-educated targets . Logos arguments are based instead on people’s (irrational) *practical* reasoning.

An ideal logos argument should have a *short rhetorical path* from a person’s *strongly held beliefs and motives* to the desired consequence. A rhetorical path is not a logical implication, but one that sounds causal. A good example is Johnny’s Cochrane’s “if the glove doesn’t fit, you must acquit” from the O.J. Simpson trial. Jurors were given a vivid demonstration that the glove didn’t fit, and then an informal implicature (with no logical chain) to the desired consequence.

The great opportunity with ICT’s is that they support *personalized* persuasive messages. People differ greatly in their beliefs and motives. Traditional media support only impersonal messages, or messages tailored coarsely across the political spectrum. By instead interviewing a sample of the target population, eliciting their beliefs and motives that a relevant to a proposed innovation, one can cluster the data and then custom design a small set of persuasive messages that should nevertheless be very convincing for the corresponding recipients.

3. MOTIVE TECHNOLOGY

Inevitably, attitude change precedes behavior change. The former is the realm of persuasion, while motive technology bridges the gap between attitude and behavior change. There are many reasons why an actor who is convinced of the “right thing to do” will still not act. Motivation is a therefore a critical piece of persuasion. And as we discussed earlier, workers in key services in developing regions may show performance deficits related to motivation.

3.1 Self-Efficacy and Planned Behavior

Two influential frameworks in psychology of attitude and behavior change are Bandura’s Self-Efficacy Theory [2] and Ajzen’s theory of Planned Behavior [1] which is closely related. Self-efficacy relates to actors beliefs about their own ability to perform action successfully. Self-efficacy appears to be a significant challenge for some groups we have worked with. For instance, community health workers in India face several challenges that weaken self-efficacy. Their education is often far below the official level (8^{th} grade), their training is well below the required level (which is still just 21 days), and ongoing training seems to be erratic or inaccessible. Furthermore, they often struggle with poor supply chains and infrastructure. Their credibility as health authorities is often weak in the eyes of the villagers they serve because: many come from outside the village they serve, their job is relatively new and poorly understood by villagers, and as working women they have low status in the community. These influences tend to reduce self-efficacy which then weakens job performance. Workers’ low perceived efficacy becomes a self-fulfilling prophesy.

Self-efficacy highlights several mechanisms for improving self-efficacy. Among these are enactment and envisionment of successful behaviors. Regular training can do much to help. Mobile technologies can augment training and also improve communication so that ongoing training sessions are well-advertised and well-attended. Simple messages of encouragement can be very effective, especially if such feedback is lacking in the workers normal daily grind. Finally, vivid stories of success communicated to workers (probably as short audio “plays”) should help workers envision their own success in their role.

3.2 Implicit Motivation

One of the more striking results in the study of motivation is the presence of two essentially independent mechanisms: one unconscious (implicit motivation) and one driven by conscious reflection and goal-setting [8]. Implicit motives have biological correlates, are largely unconscious, and have long term influence on behavior and life satisfaction. Explicit motives on the other hand, relate to individuals conscious goal-setting and decision-making. They have shorter-term influence, and can be influenced by a variety of external forces.

Much basic work on motivation for subjects in developing regions remains to be done. The correlates and effects of implicit motives should be similar to those in developed countries. But explicit motives may show more cultural and educational variation.

In our own work on cell phone learning games, we were struck very early on by the reactions of children to "you win" screens in the games. They clearly provided a simple but powerful answer to their motive needs, most likely achievement/power motives.

3.3 Communities of Practice

Communities of practice enhance motivation and self-efficacy along many dimensions. Since the community is structured around a particular practice (such as health care), discussions in the community also revolve around that practice. The community provides an identity for each member which aligns with performance (individuals start thinking of themselves as "health worker") which entreats them to act out the health worker role as best they can. Communities provide mentoring, idea exchange, social support and sharing of "war stories" which help members deal with difficult circumstances.

The development of communities of practice may lag behind the development of new practices. Or pragmatic constraints may intervene. For instance, community health workers are terribly isolated (only one of each job type per village) and are not able to participate in a community of practice. They lack local peers with shared practice, and their training is far too short for them to develop lasting relationships with peers working elsewhere. Mobile technologies could do much to support new kinds of practice community. By developing multimedia "newsletters" and "voice blogs," health workers can have access to many of the benefits of a normal community of practice. Such communications (and e.g. announcements of ongoing training meetings) should encourage regular face-to-face meetings of peer workers and further strengthen their sense of belonging to this new community.

4. REFERENCES

1. Ajzen, I., *From intentions to actions: A theory of planned behavior.*, in *Action control: From cognition to behavior.*, J. Kuhl and J. Beckmann, Editors. 1985, Springer-Verlag.
2. Bandura, A., *Self-Efficacy: the exercise of control*. 1997, New York: Freeman.
3. Bogost, I., *Persuasive Games: The Expressive Power of Videogames*. 2007: MIT Press.
4. Cialdini, R.B., *Influence: Science and practice*. 2001, Boston: Allyn & Bacon.
5. Fogg, B.J., *Persuasive Technology*. 2002: Morgan Kaufmann.
6. Fogg, B.J. and D. Eccles, *Mobile Persuasion: 20 Perspectives of the Future of Behavior Change*. 2007: Stanford Captology Media.
7. Hogan, K., *The Psychology of Persuasion*. 1996: Pelican Publishing.
8. McClelland, D.C., R. Koestner, and J. Weinberger, *How do self-attributed and implicit motives differ?* Psychological Review, 1989. **96**: p. 690-702.
9. Ramachandran, D. and J. Canny. *The Persuasive Power of Human-Computer Dialog*. in *3rd Int. Conference on Persuasive Technology (Persuasive 2008)*. 2008.
10. Rogers, E., *The Diffusion of Innovations*. 4th ed. 1995: The Free Press.
11. Zimbardo, P.G. and M.R. Leippe, *Psychology of Attitude Change and Social Influence*. 1991: McGraw Hill.

The Simputer Meme and Sustainable Development

Vijay Chandru
National Institute of Advanced Studies
Indian Institute of Science Campus,
Bangalore, Karnataka 560012, India
+91.984.520.2820
chandru@alum.mit.edu

ABSTRACT

The Simputer project recently completed its tenth anniversary and this seems a good reason to review one of the most striking experiments in ICT for development in India. As an open source hardware and software project, conceived and executed by “third world” technologists, this project captured the imagination of the larger global community of technologists and the press. Over the last few years, similar handheld projects have claimed evolutionary superiority over the Simputer meme. This position note, by one of the founders of the Simputer project, is an argument for the revival of the original meme of the Simputer so that the challenges in information technologies for sustainable development that motivated the Simputer can be re-examined.

Keywords
ICT, open source, handheld computers, sustainable development

1. INTRODUCTION

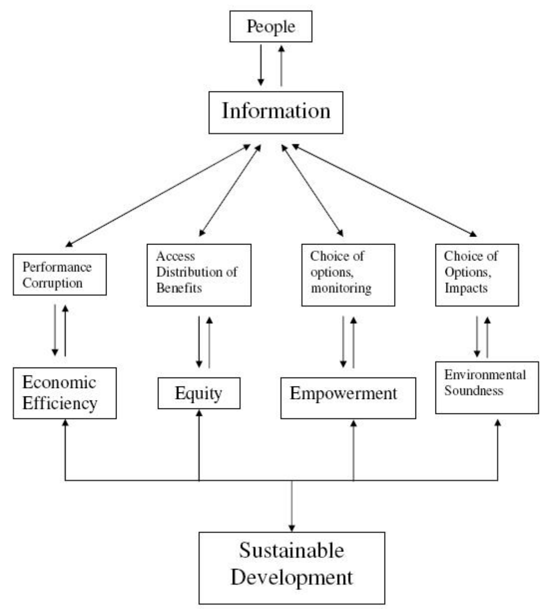
The Simputer (**S**imple, **I**nexpensive, and **M**ulti-lingual **P**eople's comp**UTER**) was an open-source handheld hardware and software project conceived by a group (the Simputer Trust) of computer scientists from academia (including the author) and practicing hardware engineers in Bangalore, India in late 1998. It was smart card enabled for sharing and to enable development oriented applications (micro-finance was the original motivation). Using an iconic interface on a touch screen, the Simputer was a squawk box that spoke in Indian languages using a text to speech synthesizer (dhvani) that was created for applications accessible to semi-literate users. Ten working prototypes were demonstrated at the launch in April 2001.

“Like many others, I felt that we were at the cutting edge of history .... But the real reason for my excitement was that the whole atmosphere

was reminiscent of the launch of ASTRA in 1974 over 25 years ago.”

The above quote is from a private talk to the Simputer Trust by the late Prof AKN Reddy, the creator of ASTRA a celebrated experiment in appropriate technology in the 1970s and 80s also at the Indian Institute of Science. He had just witnessed the launch of the Simputer by the Trust in April 2001. Prof Reddy (a winner of the Volvo prize) is remembered for his deeply scientific, socially sensitive and humanistic approach to the study of alternate energy and sustainable development [4].

The figure below, suggested to the author by Professor Reddy, is an analogous (information taking the place of energy) diagrammatic view of the possible roles that ICT can play in the context of sustainable development.



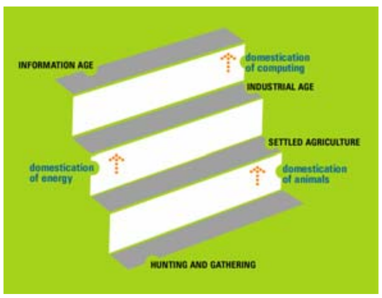
A useful exercise for the Simputer trustees and perhaps social theorists would be to carry out a ten year review of the Simputer project with the above frame of reference to study its successes, shortcomings and failures. Several thousand Simputers of various kinds have been manufactured and engaged in various applications including e-governance, health monitoring and access, education, policing and monitoring of agricultural operations.

A particularly interesting analysis would be to juxtapose the revolution in countries like India brought about by the cell phone and mobile technologies with handhelds like the Simputer in the above framework to see if there are complementarities that have not been addresses by the former as yet.

2. THE SIMPUTER MEME

The Simputer Trust was the coming together of academics and technologists from industry with a broad imperative of harnessing the potential of the Simputer for the benefit of all sections of society. The vision of this non-profit trust was to promote the Simputer, not as an end product but as an evolving platform for social change. We used the slogan "Radical Simplicity for Universal Access!" to describe the vision of the Simputer Trust.

2.1 From a techno-historical perspective, we believed that building a computer from scratch was a necessary rite of passage that Indian technologists needed to make any reasonable claims of having entered the information age. The ability to control technology design and intellectual property through local talent was an equally important driver.



2.2 We would work on a small handheld computer that could penetrate the Indian landscape much the way wireless transistor radios had. Since we could leverage the telecom revolution that had already been seeded by C-DoT and Sam Pitroda's vision, rural connectivity was no longer an issue. The Trustees had a paradigm application in mind – that of rural banking (micro-finance). The idea of a smart card integrated handheld was an early commitment.

2.3 The image below is of children in a school in Chhattisgarh using Simputers as part of their school curriculum. This lab was part of an

educational initiative funded by the South Asia Foundation and implemented by PicoPeta Simputers in 2002-2003. The Simputer in the hands of a child is a meme that Professor Nicholas Negroponte has re-invented in a Western idiom as the OLPC project (one laptop per child). While the Simputer was designed to be "shared", the XO is designed to be "personal". As phrased by Bruce Sterling in the NY Times, "the Simputer is computing as Gandhi would have invented". In contrast, the XO appears to be computing as donated by the invisible hand of Adam Smith!

2.4 An innovative licensing mechanism has evolved through intense discussion within the Simputer Trust. We acknowledge the influence of the Free Software movement in this regard. However, the Simputer General Public License (SGPL) is more complex in many ways, partly due to the nature of hardware and partly to ensure that there are sufficient incentives for continuous innovation on top of the Simputer platform. Some clarifications on the similarities and differences between GPL and SGPL are presented in http://www.simputer.org/simputer/license/sgplvsgpl.php

3. SUMMARY

A local community such as the village panchayat, the village school, a kiosk, a village postman, or even a shopkeeper should be able to loan the device to individuals for some length of time and then pass it on to others in the community. The Simputer, through its Smart Card feature allowed for personal information management at the individual level for an unlimited number of users. The impact of this feature coupled with the rich connectivity of the Simputer could be dramatic. Applications in diverse sectors such as micro banking, large data collection, health information and access, agricultural information and as a school laboratory were made possible at an affordable price.

It appears that these ideas (memes) remain relevant a decade later for the computer science community as it seeks new challenges for ICT and sustainable development.

4. ACKNOWLEDGEMENTS

Standing L to R: Swami Manohar, V Vinay, Vijay Chandru, Vinay Deshpande
Sitting L to R: Mark Mathias, Shashank Garg, Ramesh Hariharan

Courtesy: Frontline Magazine

5. REFERENCES

[1] www.simputer.org The Simputer Trust website contains a lot of material related to the technology of the Simputer, the SGPL (license) details as well as documentation of the evolution of the Simputer project.

[2] http://en.wikipedia.org/wiki/Simputer The Wikipedia entry for the Simputer is also a source for useful information

[3] The Simputer: Radical Simplicity for Universal Access, presented by the Simputer Trust at Development by Design conference held by the MIT Media Lab in 2001 **http://www.thinkcycle.org/tc-filesystem/?folder_id=16173**

[4] Energy, Environment and Development: A Technology Perspective (Selected works of Prof. Amulya K. N. Reddy)", Narosa Publishers: New Delhi, October 2005 .

See also <http://amulya-reddy.org.in/>

Solving Developing-World Problems: Academicians' Challenges

Kameswari Chebrolu, CSE Department, IIT Bombay

Abstract: The following narrative brings to fore some of the challenges faced by people, specifically academicians working in the area of sustainable global development. The challenges are chronicled from idea inception to commercialization. While an approach to address the challenges is suggested towards the end, the final objective of this write up is to facilitate a discussion among like minded people, clearly defining a set of action items that help tackle the various issues. This will not only ease the journey for people working in this area but will also facilitate more people joining this effort.

1 Academician's Challenges

There are four broad challenges we identify below.

1.1 Idea Conception

Prof. X has a background in computer networks and wants to develop a communication infrastructure for rural poor. He believes that this will improve their quality of life by providing them access to health, agricultural and other educational information. From his own experience as well as discussion with his friends, he realizes that any solution in this space has to be very low cost and consume low power since neither the paying capacity nor the power infrastructure in villages is good. After much thought, he hits upon a solution that will provide voice and messaging services (much like what GSM does) at very low cost and power. But he has to justify that the idea is indeed effective and better than what exists currently. He has the following questions:

1. The majority of the rural poor lead a hand-to-mouth existence and their basic needs themselves are not being effectively met. Given this, is such a technology offering a luxury or necessity?
2. What is the cost and power consumption of GSM base stations? Will GSM make in-roads into rural regions so as to render the considered solution obsolete in a few years?
3. Any cost optimization in his design depends on the population density and span of the considered village. What are the typical numbers?
4. Is there a business case for this design? What will villagers be willing to pay for these services? And when can the investment break-even?

Challenges: Justifying ideas is very important in mainstream research as well. However the task is relatively easier given the large number of researchers working in a given area. One among the many has the right contacts/experience to comment on the validity of the assumptions made. However, in our setting, given the very few number of researchers, getting correct answers is not so easy. One needs contacts in industry, government, access to sociologists that can do the right kind of survey, business administrators to do the cost-benefit analysis. A university professor is often clueless on how to get this kind of information. Further, incorrect assumptions can lead to ineffective solutions that end up not only wasting tax-payers money but also the energy/time of the few interested and capable individuals.

1.2 Implementation

Prof. X after convincing himself about the usefulness of the idea, gets down to implementing the same. He advertises the scope of the project to prospective students. Further, the professor realizes that some aspects of the project involve hardware design which is not quite his area of expertise. So he broaches the topic of collaboration with some of his colleagues. However, the response from the students as well as potential collaborators is lukewarm.

The students are concerned that they may not land good paying jobs after their degree program since the topic is not main stream research. Some, who wish to pursue a PhD are worried that this work will not result in publications in well recognized conferences/journals. Some senior students who have worked in the past on such projects have presented grim pictures to other students of exhausting day-long field trips, running around in the hot sun, lack of amenities at the destination villages. Some are also worried that they may not find a support group to share their achievements and frustrations. The colleagues on the other hand, though helpful are unwilling to invest the amount of time and effort necessary to take the project to fruition since most feel that work in this space is developmental and not really research.

Challenges: The main hurdle in involving students and other faculty in this effort is that the whole area is relatively uncharted. Students concern about job prospects is rather misplaced, since the software/hardware skills learned during implementation often find good use in industry, the perception is however hard to remove. While publication in top conferences is feasible, the authors have to often defend the work more vigorously since the audience is not familiar with the settings, assumptions and difficulties of on-field work and often categorize the work as developmental as opposed to research. Compared to students, changing the perception of colleagues is often harder since they have set beliefs.

1.3 Validation On-field

With the help of some students and collaborators, Prof. X successfully prototypes the concept and now wishes to test the prototype on field. However, the NGO his friend has put him in contact with wishes to deploy the prototype in a village some 600kms away. While he and his students can spend 1-2 weeks deploying the prototype, proper validation requires extended operation, the whole cycle of log collection/debugging, all of which can easily span a few months. However, little technical help is possible from the villagers. While some technical alternatives like remote login, hardware reverting to safe state on detecting failures help, its hard to make them work in practice. Lack of proper follow-up can often bring months/years of effort to naught.

Challenges: Management of a remote deployment is in general a very difficult problem to handle and there are no convenient solutions.

1.4 Commercialization

Now that the prototype proved to be a success, the Prof wants it to be used in other villages. He is aware that neither can he bear the cost of the deployment in these other villages nor maintain the deployments. He therefore wishes to commercialize his product and let the deployment be managed by third-party vendors/service providers. But start-ups, marketing, management are not really his cup of tea. He is happy contributing to the technical side of the problem.

Challenges: Not all professors have an entrepreneurial streak and are often baffled and lose interest when faced with the up-hill task of start-ups and management. Some universities do have incubation center that help patent, fund initial prototypes, help contact venture capitalists, but a lot depends on the initiative of the faculty.

2 Solution Approach

All of the challenges outlined above demonstrate a need for a close knit community that can help one another. Such an objective can be partly achieved by forming a group, exchanging emails, posting questions in the group forum, physical meetings every once in a while. But this helps only to some extent. A more organized approach that can help is a non-profit organization with the following features:

1. Faculty, technicians, computer scientists (specifically well-deserving students) get recruited full/part-time based on interest/qualifications to the organization. Funding is through research proposals submitted to government, industry and any other charitable organizations. Patents/royalties, consultancy may also bring in additional money.
2. One of the departments in the organization is a fact hounding unit that takes requirements from employees and verifies ground reality by talking with other government/industry contacts or conducting necessary surveys. It also takes input from NGOs that are looking for specific solutions and puts them in contact with the right personnel.
3. There is a pool of well qualified technical staff that individual project leaders can rope in during implementation and later for validation on field. The staff is expected to spend months if necessary at the required test-site which can be anywhere in the world.
4. There is also a department consisting of lawyers, business administrators which files patents, interfaces the project leaders with venture capitalists, third-party vendors/service providers.

A lot of detail is necessarily glossed over due to lack of space but the above features effectively solve most of the challenges outlined. Some existing organizations have some elements of the above features. For example, International Computer Science Institute (ICSI) [1] is based primarily on the first feature. Media Labs Asia (MLA, India) [2] has elements of features 2, 3 and 4, however the technical help available from it is rather limited. Thinkcycle [3] supports some aspects of feature 2, it is basically a website that puts NGOs in contact with personnel with the right skill set. A careful study of the needed functionality, and final realization of the organization will not only make the journey smooth for those individuals embarked on this path but will also likely help many others join this effort.

References

[1] International Computer Science Institute. `http://www.icsi.berkeley.edu/`.
[2] Media Lab Asia. `http://www.medialabasia.in/`.
[3] ThinkCycle. `http://www.thinkcycle.org/`.

Scaling from One to Seven Billion: Internet, Facebook, YouTube, and eBay?

John Chuang
UC Berkeley
chuang@ischool.berkeley.edu

June 23, 2009

I. Connectivity: Scaling the Internet to 7 Billion Users

Given that the Internet has successfully scaled from 10^0 to 10^9 users, one might conclude that a further scaling to 7 billion users is not a difficult task. Yet, even with significant growth rates across Africa and other developing economies, it is not at all clear that we can reach 7 billion in the same amount of time (~40 years) that it took us to reach 1.5 billion users today.

Are traditional scaling metrics (e.g., number of hosts, number of packets, number of routers, network diameter) still relevant when we aim for the next six billion? If not, what other scaling metrics might become important?

If we look at the topological structure or the routing tables of the global network, will they undergo some fundamental change as the network reaches into the developing world? The Internet, at both the macroscopic and microscopic levels, is shaped as much by economic forces (e.g., strong economies of scale, network effects) and governmental policies (e.g., network neutrality regulation, spectrum management) as by the network technologies, architectures, protocols, and applications. In contemplating the potential scalability of the Internet as a truly universal network, should we consider scaling metrics such as variance in GDP, variance in population density, variance in market size, variance in market concentration (HHI), or even variance in regulatory environments?

Today, the networking research community is grappling with serious issues of network (in)security and architectural ossification. Community-wide efforts are devoted to clean-slate designs of future internet architectures, and even an ambitious "virtual laboratory for exploring future internets at scale" (namely GENI). How can we meaningfully articulate "scaling to 7 billion users" as a first-class design requirement for the Internet?

II. Applications: Facebook, YouTube, and eBay for the Next Six Billion?

With 1.4 billion of the world population living on $1 or less per day, and 74% of sub-Saharan African population with no access to electricity, it is easy to dismiss applications like Facebook, YouTube, and eBay as frivolous and irrelevant outside of the developed world.

On the other hand, mobile phone penetration in Africa has gone from 2% in 2000 to 22% in 2006. What applications compelled 200 million Africans to acquire their mobile phones? What are the "killer-apps" that will fuel further adoption of information and communication technologies (ICT)? To date, information and communication technologies and development (ICTD) projects have focused primarily on applications for communication, education, business, or healthcare. What about "inculturated" versions of applications for community building, grassroots knowledge creation and sharing, or connectivity to a global marketplace?

How do we gauge the demand for different classes of applications across different populations? Can the design principles and design methodologies for the first billion be used for designing applications for the next six billion? Are the economic institutions and innovation levers that brought us Facebook, YouTube, and eBay, or even Google, Twitter, and Wikipedia, well-placed to deliver similar innovations for the market of the next six billion?

III. Grand Challenges

What are some grand challenges for scaling ICT to 7 billion users?

1. Reduce total cost of ownership and operation of personal ICT hardware, software, and connectivity to less than $1 per user per month for the bottom billion. Is reliance on Moore's Law sufficient to drive hardware costs down to under $1 per month in a sustainable way? Is free (as in beer) software sufficient or appropriate for these systems and users? What forms of spectrum management, interconnection agreements, and universal service regulation, across jurisdictional boundaries, offer the most promise to affordable access to ubiquitous connectivity in undeveloped regions? In what ways might the *economics of free* [1] be profitably applied to ICTD economics?

2. Protect the newly deployed billions of devices from joining the rank of botnets or other cybercriminal networks, and the billions of owners from ending up with jobs solving captchas for spammers and phishers, or becoming cybercriminals themselves. Harness the resources of these devices and users to deploy socially valuable background applications (e.g., for planetary-scale cyber-defense, environmental sensing) without compromising user privacy, with a possibility of income growth for the users.

3. Develop scalable and sustainable ICT applications and business models that will raise wages for the bottom billion, directly or indirectly, from $1/day to $2/day. Boosting wages by $365 billion each year in undeveloped regions is by no means a trivial task. In what ways might farmers, artisans, and even the unemployed, improve their productivity, increase their market size, or realize their earning potential, through ICT applications? In what ways might market inefficiencies due to information asymmetries be mitigated or overcome by ICT applications? In what ways can ICT augment or further improve the

[1] Anderson, C., Economics of free: Free! Why $0.00 Is the Future of Business. Wired 02.25.08.

group-lending and peer-monitoring mechanisms that have proved so successful in overcoming problems of adverse selection and moral hazard in microfinance?

4. Demonstrate measurable and sustainable improvements in health, education, and other development indicators due to use of ICT. Given the diversity of geographies, languages, cultures, populations, and livelihoods, it would be foolish to assume that the improvements will come from a single one-size-fit-all solution. On the other hand, it would be extremely beneficial to distill a set of design principles from a systematic cataloging and analysis of successes and failures of different attempted solutions.

Challenges for Computer Science Research in ICTD

Edward Cutrell
Microsoft Research
1 Microsoft Way, Redmond, WA 98052, USA
cutrell@microsoft.com

ABSTRACT

Research in computer science related to global development faces a number of unique challenges that are somewhat new for computer scientists. These challenges range from the interdisciplinary nature of work and discourse in global development to a variety of issues surrounding the context of working in the field. CS researchers thus face the twin challenges of communicating their work to people with very different goals and conventions, and people in their own field who are not familiar with the unique challenges of work in global development. In this paper I would like to identify and discuss few of these challenges and identify some parallels with human computer interaction (HCI), another multidisciplinary area that CS has (mostly) come to terms with. With these observations, I hope that we may be able to find a common ground of accepted practice and vocabulary to make a home for computer science as a rigorous discipline in global development.

INTRODUCTION

CS researchers who want to work in the context of global development (i.e., information & communication technologies for development or ICTD) face several challenges that are largely unrelated to technical difficulties. The interdisciplinary nature of development work requires researchers to communicate with non-CS colleagues who may have different ideas surrounding the content, methods, and goals of research. In addition, the goals, metrics and constraints of CS research in ICTD are often quite different from those of researchers' peers in more traditional sub-disciplines of CS. These challenges are particularly acute for researchers who must communicate their work to university faculties, funding committees, etc. who are not familiar with these constraints and may expect more traditional computer science metrics and methodologies without understanding the larger context of work in global development.

INTERDISCIPLINARY CHALLENGES

ICTD as a research community is still very new and is still in the process of defining itself. It is strongly interdisciplinary, bringing together researchers from such varied fields as computer science, cognitive and social psychology, design, anthropology, development theory, economics and public policy. In turn, researchers in each of these communities bring with them their own conventions, methodologies, vocabularies, and notions of contribution. Even within those disciplines that consider themselves largely empirical, there are often huge differences in data collection methodologies, analysis and interpretation. For example, in MSR India, write-ups of ethnographic explorations [1, 5] are extremely different from traditional CS research written up in the same group. Differences are even greater in work by social and development theorists. Something like Kleine's recent paper at ICTD in Doha [4] would likely never be seen in any ACM or IEEE conference that I'm aware of, yet it won an honorable mention as best paper. The point here is that these (very different) traditions of the social sciences and engineering are attempting a discourse within this broader, heterogeneous community.

These differences can lead to a number of difficulties in communication between the different groups who are trying to share knowledge and build the ICTD community. What is often overlooked is that there are also significant difficulties raised between researchers and other members of their own communities. Computer scientists may have a hard time justifying their research to their departments and funding agencies when these groups do not understand the range and variety of work that may appear alongside (or integrated with) a CS researcher's.

History of HCI as model for ICTD

The problems of "interdisciplinarity" are not new for CS, and it may be useful to look a how they have been resolved (or not) before. I am thinking here of the field of human computer interaction (HCI). It is only in the past 5 to 10 years that HCI has come to find itself embraced by the top computer science departments, and even now there remain many debates about the relationship between HCI and more traditional CS. When I came to Microsoft Research in 2000, I was only one of four researchers trained in psychology, and there were only a handful of people working in HCI. Today, there are 7 or 8 different research groups working in HCI in MSR, comprising psychologists, designers, ethnographers and (of course) computer scientists. Similarly, HCI is now a major endeavor at the top CS departments around the world.

Like ICTD, the field of HCI is interdisciplinary, including researchers and methodologies from computer science, psychology, design, ethnography, management and engineering. While there remains significant friction where these touch, HCI has evolved into mainstream acceptance within CS. Interestingly, there were few if any computer scientists in HCI at its infancy. Then, psychologists and cognitive scientists were the main players and it was only later that voices from other disciplines came to be appreciated as important contributions. Jonathan Grudin has written extensively on the history of HCI (e.g., [2, 3]), discussing how issues of quality, originality and methodological rigor have come to be defined in HCI (for good or ill). It may be instructive to view ICTD with some of his observations in mind. An important difference between ICTD and HCI is that while HCI steadily evolved to incorporate more and more disciplines, ICTD has moved much faster, "jumping into the deep end," so to speak, attempting to bring together the various voices and agendas in very short order. Another important difference is that while there is some overlap between disciplines participating in HCI and ICTD (bringing some familiarity with norms, conventions, methods, etc.), fields such as public policy and social theory are still quite new to this mix.

One interesting point that Grudin brings up that seems particularly relevant for this discussion is how the venue for quality publications in HCI has shifted from peer-reviewed journals to the "conference-centric" model typical of U.S. computer science. This has resulted in a serious tension between CS researchers (who want to use the conferences as "quality publication venues" for tenure, funding, etc.), and other researchers who have a very different view of the role of conferences. As the field of ICTD moves forward, I think we must tread carefully, aware that the expectations and desires of CS researchers may be significantly different from those of other disciplines involved. If CS researchers want the area to remain largely interdisciplinary, we must take these concerns seriously. In HCI, a result of this move to a conference-centric model has been the fragmentation/ spin-off of various sub-communities (practitioners, anthropologists, management researchers, etc.) who no longer feel welcome at the main conferences.

CHALLENGES SPECIAL TO ICTD RESEARCH

Another challenge for CS researchers working in global development is the broad range of issues associated with working in situ in developing regions. Common problems include:

- Issues related to physical locations. E.g., remote locations, problems with power and connectivity, security concerns, etc. These are probably the most obvious problems and are often relished by researchers as interesting technical challenges.
- Relationship management with governments and NGO partners. It is often very difficult to gain access to populations, resources or even locations without such partners, and they often have different or even contrary agendas than researchers. For me, this was one of the most surprising (and sometimes vexing) constraints on doing work in the field.
- Concerns working with target populations. The people we work with are often extremely poor, with needs that go well beyond those being studied. As a result experimental aims may be subverted for more pressing needs. In addition they are frequently illiterate and require local translation for communication with the researcher, which brings further opportunity for misunderstanding and misdirection. Finally, there are serious concerns with informed consent; the notion of informed consent borders on meaningless for many of these populations, yet this is a critical ethical requirement for working with human subjects in Europe and the U.S. How do we maintain the letter and spirit of IRB requirements and still manage to do research effectively?
- Fundamental questions of research aims and goals. Work in global development is *different* from most other CS research because academic research goals may not necessarily align with development goals. Yet fundamentally, it is the development goals that motivate most researchers to work in this area in the first place. This tension plays out in a variety of ways. One of the most insidious may be the sense of burnout common to aid workers, and this can be particularly problematic for students and young researchers. This misalignment in goals may also lead to problems in funding (whether from aid agencies or more traditional quarters).

While I do not have space to go into these in any detail, I believe they are all critical issues for computer science researchers wanting to make progress in this area. There are no easy answers for any of these, but a common awareness can help to build a set of best practices and common approach. For example, could the community arrive at a common approach for informed consent in ICTD? Would it be helpful to share experiences working with NGOs and governments to arrive at win-win compromises for the different agendas each have?

Of course, these are but a few of the myriad challenges that ICTD researchers face. Hopefully the CS community can come together to identify and craft responses to these challenges to insure a strong role for CS research in global development.

REFERENCES

1. Donner, J. & Escobari, M. (2009). A review of the research on mobile use by micro and small enterprises (MSEs). In *Proc. of the 3rd ACM/IEEE Int'l Conference on Information and Communication Technologies and Development*, (Doha) 17-26.
2. Grudin, J. (2004). Crossing the divide. *ACM Transactions on Computer-Human Interaction (TOCHI)*. **11**(1), 1-25.
3. Grudin, J. (2006). Why CHI fragmented. In *Proc. of CHI'06, Extended Abstracts on Human Factors in Computing Systems*, (Portland), ACM press, 1083-1084.
4. Kleine, D. (2009). ICT4What? – Using the choice framework to operationalize the capability approach to development. In *Proc. of the 3rd ACM/IEEE Int'l Conference on Information and Communication Technologies and Development*, (Doha) 108-117.
5. Rangaswamy, N. (2006). Social entrepreneurship as critical agency: A study of rural internet kiosks. In *Proc. of the 2nd ACM/IEEE Int'l Conference on Information and Communication Technologies and Development*, (Doha) 143-152.

CS(4GD) should look like CS not do-goodery

James Davis

UC Santa Cruz - Dept. Computer Science
davis@cs.ucsc.edu

1. INTRODUCTION

Why?: Why should Computer Science for Global Development (CS4GD) be focused on Computer Science and not on achieving the goal of global development? This is an intentionally controversial and exclusionary statement. After all, most of us with interest in this area are motivated by the desire to improve the world. We have valued collaborators in Sociology, and indeed many of the most important problems in global development are sociology or economics problems, not technology problems. So why should we be CS centric? Shouldn't we embrace interdisciplinary work and directly adopt the language of global development? I believe there are two good reasons why we should be CS centric: effectiveness and pragmatism.

Effectiveness: Technology is effective at solving problems, and technology has a clear history of progress. Cell phones have done more to provide communication and employment opportunities to the poor than any intentional development program. The internet and search engines have done more to increase access to information globally than any intentional library project. Both arose from the advances made in technology research. These advances were not an accident. Technological progress is the result of a well oiled joint government, academic, and industrial machine. Unfortunately, the application to development in both of these examples was an "accident". Our goal should be to more systematically channel CS research towards development, even targeting our research in this direction. But make no mistake, to be effective, we should be working on computer science, not on development.

Pragmatism: There are purely pragmatic reasons to focus on computer science research. The most important is that groups in CS departments *must* function like CS. We need to fund graduate students, faculty summer salary, and large equipment budgets. Our funders expect us to be working on CS. We also need to get buy in from other parts of computer science. Our colleagues sit on dissertation and tenure committees and its important that we are speaking their language. HCI practitioners have suffered dearly because many departments can not come to an agreement that HCI is indeed a part of CS. Within CS4GD people have turned down faculty positions in part because its not clear they can be tenured, and I myself am not 100% convinced its responsible to take impressionable young Ph.D. students in an area for which I can neither fund them reliably nor convincingly argue the topic is actually CS. Our work is by its nature interdisciplinary enough, we should be trying as hard as we can to make it appear *not* interdisciplinary, fitting squarely in CS.

2. ACTIONS

Branding: We need a good brand. Computer Science for Global Development *sounds* like an application area. We might as well work on CS for Literature Studies or CS for Art Practice. To be accepted we need to be a *core* CS area. It is not by coincidence that departments don't hire anyone in the area of CS for Biology… but Bio-infomatics, well now, that's a hot CS topic. Unfortunately I can't come up with a good name that gives the connotation of computing for a future of nearly everyone, instead of the minority of elite that are the current targets. "Bottom Billion Computing" isn't right – I don't like the word "bottom", since it connotes charity and not opportunity and growth. "Global Computing" is a maybe, but doesn't seem right. "ICT4D" has the same problems as CS4GD and is already quite tied up with sociologists and development experts, so adopting it will create name space conflicts. We lack a good name now, we need one, its crucial to our efforts, and the choice is important because the words chosen will influence which topics are included and which are excluded from this new area.

Definitions of good research: We should define the quality of our research in ways that are as similar as possible to other areas of CS. I believe orderability and impact are two good criteria.

Orderability is what makes it possible to discuss whether one set of work is "better" than the prior art. Nearly all of CS is quantifiable in some way and it is possible to know that a new solution is 20% better than a prior solution. I believe this is critical to the rapid rate of progress in technology research. As an example, I do not prefer research that concludes that kiosks do or don't work in a particular case study. I much prefer a conclusion that they would be viable if the computer cost $X. In this case X is quantifiable and the next paper is challenged to compare their findings to the prior work in a direct way. It is not important that we agree on the metrics used to make quantifiable claims, this will be impossible. Neither is it important that the metrics are perfect, they won't be. However it is important that we are in the habit of making direct comparisons, and are not afraid to argue that our new work is "better" and thus someone else's prior work is "worse".

Impact should be measured in the long term. We shouldn't allow our work to become about the *direct* impact on lives that we may or may not have. Research is not better simply because it serves 10,000 people instead of 10. Of course it may be necessary to validate against populations of some size in order to be credible, but this is not the same as having a direct goal of serving the population. By analogy, if we invent a new memory architecture, it is not important that the researchers themselves start a company and commercialize this work. They don't even need to fabricate a real chip. If a paper design leads to new understanding that allows someone else to ship millions of units, that's still impact. *Indirect* impact counts.

Marketing: As with any startup endeavor, proper administration and marketing are crucial.

Create ACM Transactions on [~~CS4GD~~]. It is important that this is ACM, since this stamps it as computer science. It is important that it's part of the Transactions journal series since this stamps it as serious work. Regular publication in ACM Transactions on X is by definition good enough to satisfy any computer science dissertation or tenure committee.

Get NSF CISE to include [~~CS4GD~~]. This field should explicitly have a home somewhere buried in the hierarchy of IIS/CCF/CNS, in the same way that computer vision, or database systems has a home. When NSF says its real, then it becomes a grant target. This is critical since only the large and relatively stable funding of NSF can provide for sustained employment of graduate students. I believe the justification with regard to NSF is not "We have a duty to help poor people," but rather "The US has 5% of the world population, if we want our companies and economy to grow, we need to be the innovators in serving all these new consumers. We have to do this, its in our national interest."

Make it safe for junior faculty to declare this is their research area. I chose my institution intentionally as one which I perceived to have a campus culture that would be accepting of this area. Even so, I was heavily advised against entering this area pre-tenure and was too risk adverse to ignore that advice.

Implement consistent branding across universities. We should adopt the same brand, rather than some of us doing 'technology for social issues', some 'ICTD' and some 'CS4GD.' A good goal would be to get at least one faculty in 5 of the top 10 CS departments to list the brand as one of their primary research areas. This should be sufficient to bring everyone else along. A consistent brand will go a long way towards making this a "core" research area in CS.

3. CONCLUSION

This has not been intended as an argument that we shouldn't collaborate. Indeed, I believe we should. This is an argument that there is value to creating a CS centric identity that has meaning even in the absence of true collaborative interdisciplinarity. Further, its not just valuable, it's a practical necessary precondition to being accepted as CS research, and thus allowing faculty to run labs full of graduate students who spend full time working in the area.

A customer led agenda for ICTD research

Andy Dearden
Communication & Computing Research Centre, Sheffield Hallam University,
Furnival Building, 153 Arundel Street, Sheffield, S1 2RU, UK Tel: +44 114 225 6878
a.m.dearden@shu.ac.uk

ABSTRACT
In this paper, I examine some basic literature in development studies and consider its consequences for the future of ICT for development. I shall argue that effective interventions should focus on the institutional, rather than the individual level. I suggest that a central goal should be to build the ability of public service bodies, non-governmental organizations and community based organizations, to design and implement technology that is relevant to their needs and the needs of the communities they represent.

Categories and Subject Descriptors
K4.2 [**Computers and Society**]: Social Issues.

General Terms
Management, Design.

Keywords
Capacity Building, Participatory Design, ICT for Development.

1. INTRODUCTION
The idea of using information & communications technologies (ICTs) to support development is as old as the idea of development itself. The form of information and communication technologies has changed from supporting free and open local markets, building road and rail networks, connecting telephone systems, developing local radio stations, to today's mobile and wireless services. During the 60 years since development was introduced as a social and political ambition in President Truman's inaugural address [13], work has progressed in all these areas of ICT for development. Indeed, Truman's initial conception of development was of (one-way) technology transfer, "…making the benefits of our scientific advances and industrial progress available for the improvement and growth of underdeveloped areas." And yet, 60 years later, we live in a world filled with extreme poverty, social exclusion, poor life expectancy, political repression and all sorts of un-freedom.

Given this history, we who innovate with current and future generations of ICTs should be highly reflective, and humble in our expectations of our potential contribution. We must pay close attention to the catalogue of lessons already learned by others.

2. Development is a social phenomenon
Development is a complex social phenomenon. Sen [12] presents development in terms of freedom – increasing people's freedom to make their own choices and enabling their capability to act on those choices, furthering their own goals. Sen identifies a range of types of freedom that should be considered: political freedoms (such as freedom of speech and democratic governance), social opportunities (such as education and social mobility), guarantees of transparency (from agents of government and other wielders of power), protective security (health care and other social safety nets), as well as the economic freedom in the form of opportunities and capabilities to earn or create a livelihood. All of these elements contribute to people's freedom to determine their priorities and to improve the quality of their own lives. Improvements in economic capacity should not be at the expense of other types of capability.

By implication, positive development is not automatically achieved by technical fixes to support new economic practices (although economic gains can open up new choices for people) but depends on interventions in multiple domains. For this reason, ICT for development should explore ways of intervening to support these multiple types of freedom.

3. Development is situated in a context
The livelihoods framework [2] provides one perspective on this context. The framework highlights different types of 'capital' that people have available to them in working out their own livelihoods. As well as financial capital, the framework considers: social capital; human capital, such as personal health, skills and knowledge; physical capital, such as tools and buildings; and natural capital such as land, water, forests and sunshine. People use this capital in a context provided by "transforming structures and processes" (such as government agencies, legal frameworks, private sector and civil society). People apply their capital, in context, to enact their livelihood strategies. These strategies result in outcomes that feed back to their stock of available capital.

In consequence, development efforts may act on this system by different mechanisms: One approach contributes directly to the individual's stock of capital, e.g. supporting human capital by enabling new skills development, or developing social capital by enabling easy contact to other people. A second strategy focuses on the transforming structures and processes. For example, enhancing the availability of health care skills, ensuring greater transparency of governance, strengthening microfinance initiatives, or enhancing the capabilities of local farmers' groups.

It has been recognized that technologies for development must be low cost & locally appropriate [8]. Given the huge disparity between the incomes of the poorest, and the price of modern ICTs (even with $100 laptops), it is more feasible to design technologies that are affordable and appropriate for community organizations, than affordable technologies for individuals. The potential of this approach is demonstrated by examples such as the use of FrontlineSMS to support community health workers (www.jospa.org), mobile phones for microfinance data collection [11], and multimedia messaging in farmers' co-operatives [3].

4. Development must be locally owned

A second consequence of development as freedom is that, since development activity involves power, the intended beneficiaries must have the right to engage in, influence and critique development interventions. The OECD argues that to be sustainable, development must be locally owned [8] with external agents providing resources to support local capacity building. Participatory development approaches [1, 5] promote a two-way dialogue that recognizes marginalized people as the best people to interpret their needs and objectives and to mobilize local capability to achieve change. Participatory ICT design also sets people in context as experts on their own situation and objectives [4]. Designers and end-users collaborate to explore the potential of technology, and to make informed choices. Over time, designers become more attuned to the needs in context, and users become more aware of the potential of technologies, and more innovative in their proposals [6]. Both innovative thinking and detailed awareness of context contribute to success.

However, in the marketplace, the interests of consumers and producers are not completely aligned. Although 'win-win' possibilities exist in most negotiations, it is important that customers for ICT can negotiate effectively to identify their own interests, to maximize their gains, and minimize their costs. The OECD emphasizes governments' public procurement capability as a critical factor for development [10]. Given the complexity and risk of failure in ICT projects, ICT procurement capability must be a concern. A key driver for the impact of ICTD will be the capability of institutions, such as NGOs, community based organizations and public service bodies, to manage design and development of innovative ICT systems for themselves. Similar arguments can be applied to the need to build up locally owned capacity in technology and design research.

5. Conclusion: A grand challenge

To serve the needs of the poorest, research in ICT for development should work with locally accountable social institutions and local research partners, to devise systems that are low cost, appropriate and sustainable in context. Through ongoing collaborations, and working with local ICT providers, ICTD researchers should build up the capacity of social organizations to innovate and to procure ICT systems that they can use to advance the interests of their communities, and to develop the local research capabilities.

One technical response to assist capacity building would be easy-to-configure, interoperable, open and extensible toolkits, working across devices and platforms, from which appropriate solutions can be created by local organizations and local programmers. The Health Information Systems Project (www.hisp.org) is an example of a global toolkit, for one domain, that can be tailored to the needs of local health service providers, with local adaptation being conducted by local developers [7].

The grand challenge is to create such easy-to-configure frameworks; and to build up the technical, creative and research capacity at local levels to exploit and extend these technologies.

An integrated work program could offer multiple routes for participation from established researchers, research students, educators, technology companies and service providers from both 'developed' and 'developing' regions.

6. REFERENCES

[1] Chambers, R. 1994. The Origin and Practice of PRA. World Development, Vol. 22, No. 7.

[2] DfID 1999. *Sustainable Livelihoods Guidance Sheets*, London: Department for International Development

[3] Dearden, A. 2009. Integrating Mobile Data Services into an Existing Information Ecology. Presented at W3C workshop on *Africa Perspective on the Role of Mobile Technologies in Fostering Social Development*, Maputo, Mozambique 1st – 2nd April 2009. Available from http://www.w3.org/2008/10/MW4D_WS/papers/dearden.pdf

[4] Dearden, A. & Rizvi, H. 2008a. Participatory Design and Participatory Development: A comparative review. In *PDC'08: Experiences and Challenges, Proceedings of the Participatory Design Conference*, Indiana University Press, Bloomington: Indiana, pp 81 -91.

[5] Fals-Borda, O. 1987. The Application of Participatory Action-Research in Latin America. International Sociology 2(4) pp 329- 347.

[6] Hansen, T, R. 2006. Strings of Experiments – Looking at the Design Process as a Set of Socio-Technical Experiments. In, Jacucci, G, Kensing, F, Wagner, I & Blomberg, J (Eds.) *Proceedings of PDC 2006*, volume 1, pp1 – 10. ACM Press.

[7] H. Kimaro and O. H. Titlestad, 2008. Challenges of user participation in the design of a computer based system: The possibility of participatory customisation in low income countries. *Journal of Health Informatics in Developing Countries* 2(1).

[8] OECD 1996. *Shaping the 21st Century: The Contribution of Development Co-operation.* Available from: http://www.oecd.org/dataoecd/23/35/2508761.pdf

[9] OECD 2000. Shaping the Urban Environment in the 21st Century. Available from: http://www.oecd.org/dataoecd/29/7/31621883.pdf

[10] OECD 2005. Harmonising Donor Practices for Effective Aid Delivery Volume 3: strengthening procurement capacities in developing countries. Available from: http://www.oecd.org/dataoecd/12/14/34336126.pdf

[11] Parikh, T., Javid, P., Sasikumar, K. Ghosh, K. and Toyama, K. 2006. Mobile Phones and Paper Documents: Evaluating a New Approach for Capturing Microfinance Data in Rural India, *Proceedings of CHI 2006*, ACM Press.

[12] Sen, A. 1999 *Development as Freedom*. Oxford University Press, Oxford.

[13] Truman, H.S. 1949. *Inaugural address*, Available from: http://www.vlib.us/amdocs/texts/41trum1.htm

Computing for Community Based Health Systems

Dr. Prabhjot Singh Dhadialla
Director of Program for Health Systems,
Development and Research
Earth Institute, Columbia University
Co-Executive Director, Community Lab
pdhadialla@ei.columbia.edu

A PREMISE

A community of computer scientists convening to consider coherent contributions to global development is a truly exciting event for those of us who work on the development of community based health systems. It is exciting not only because we anticipate the adaptation of the IT helpdesk "(x2015)" to low-resource settings, but because there is a richness of organizational thought, language and execution expertise in the computer science community that has increasingly clear cognates on social scales. With a firm command of command line languages, computer science is poised to control user interfaces to community based health systems as tools like mobile phones become global development standards. The ease of use and immediacy of information that technology provides in developing settings is at once liberating and constraining – connectivity comes with new societal dots upon which to hang policy lines. The premise of this little ditty is not to tie you in an ethical tangle, but to walk with us through a short excersise that we hope will illuminate the power and potential of your discipline's language. In turn, we hope you will continue to converse with the communities who will benefit from your well meaning words, however rendered, on how best to translate a field's abstractions into adaptive implementations. In one more turn, we hope those computer scientists who work at this vital social interface will abstract their experiences to enrich the computer science core.

1. ABSTRACTION

System-level, Cross-layer Cooperation to Achieve Predictable Systems from Unpredictable Components

In coming years, a number of factors will lead to a significant shift in the way computer systems manage reliability, variation, and fabrication. Currently, computer systems assume perfect device fabrication and operation. For high-reliability systems, the usual methods of increasing system reliability involve ECC coding on memories and triple-modular-redundancy (TMR) of critical components. These brute force methods are able to increase system reliability when silicon fabrication processes are able to deliver high individual device reliability and low variation. However, as the critical dimensions of devices, such as transistors and wires, used to implement computer systems shrink to only a few nanometers, rates of transient faults, permanent faults, and variation between devices on the same die are expected to increase to the point where this approach will no longer be practical. Instead, computer systems will need to adopt a model in which each layer in the abstraction hierarchy — applications, O/S, architecture, circuits — is prepared for the layer below to transmit bad data and in which all of the layers in the hierarchy cooperate to deliver correct operation in spite of faults, variations, and other effects.

This shift to a multi-level approach to resilience is further motivated by trends in fabrication processes where device manufacturing is increasingly limited by power consumption instead of device density, and by trends in computer architecture where designs with large numbers of independent execution resources, such as cores and reconfigurable units, are becoming more common. The need to decrease system power consumption makes it critical that schemes to tolerate errors and variation consume as little power as possible during error-free computation cycles. Diagnostics resources, error-correction facilities, and spare resources can sit idle until needed. Repair software in the O/S can be triggered to manage the repair, perhaps using distinct execution resources from those where the error was detected. […][1]

2. TRANSLATION

System-level Task-Shifting to Achieve Predictable Health Systems Services in Low-Resource Environments

In coming years, a number of factors will lead to a significant shift in the way health systems manage service reliability, population variation and morbidity/mortality. Currently, health systems assume nearly perfect operational management of clinical situations given supplies and personnel. For high-resource health systems, the usual methods of increasing health system reliability involve highly trained professionals and heavy regulatory oversight of critical components. These high-resource methods are able to increase health system reliability where resources are available to provide high individual attention to yield lower variation in services. However, as the availability of resources—such as trained personnel, fiscal space and physical infrastructure—becomes a critically limited factor in the implementation of health systems, the rates and variability of individual and systematic medical errors within low-resource systems increase to the point where this approach will no longer be practical. Instead, health systems will need to adopt a model in which each layer in the traditional subdivisions – public health programs, clinical management centers, health workers and communities – is prepared for the adjacent layer to transmit flawed information and in which all of the layers in the system cooperate to deliver correct operation in spite of supply faults, environmental variations and resource constraints.

This shift to a multi-level approach to resilience is further motivated by trends in the health system development process where universal coverage is increasingly limited by the financial ability to provide rural/underserved access instead of technical health clinic capability, and by trends in health systems architecture where designs with large numbers of independent health providers, such as community based health and private health care options, are becoming more common. The need to reach rural/underserved populations makes it critical that supervisory schemes to tolerate errors and variation in health provision consume as little resource as possible at times where quality is relatively high. Monitoring and evaluation resources, error-triggered skill retraining, and excess fiscal space can remain embedded until needed. Health system expertise that resides at clinics can be triggered to manage decreasing quality, perhaps using a distinct resource pool from those where the quality issues was detected.

The Community Based Management for Health (CBMH) framework uses an adaptive implementation approach to determine where these optimal points of exchange exist within a developing health system.

3. TRANSPORT LAYER

Community-Based Management for Health is the operational framework to enable tighter interconnections between clinics and the communities that they serve in low-resource settings. Although various national policies exist that preclude uniformity in the package of primary care skills and services offered, CBMH focuses on the optimal management, supervision and implementation of available resources. Through this process, human resources for health such as Community Health Workers are developed from a management/operational perspective to tightly interlink communities with clinics and alleviate the inappropriate burden of preventable ailments in health centers. This requires well-developed supervisory mechanisms to ensure that errors are corrected and services are optimally delivered.

Adaptive Implementation is an approach to implementation that is predicated upon adapting “core” strategies to local resources and needs. Instead of implementing a static model and waiting to see if it leads to a desired outcome, the adaptive implementation approach begins by assembling a suite of best practices and then adapting them with local data feedback to find an optimal approach. For example, regional policies may dictate that Community Health Workers must be volunteers, paid as a group, paid individually or engage pay for performance. Each of these parameters requires a different approach to organizing support, skills and supervision to meet common goals. The adaptive implementation approach uses high frequency operational research to improve service delivery and inform management decisions. In most scenarios, the desired destination of lower mortality and morbidity is clear while the sustainable path less so; adaptive implementation is an approach to bridge this gap.

Exchanging Systems Thought for Community Action through the adaptive implementation approach is predicated upon tight partnerships between communities and health systems to find optimal solutions for health service delivery. The Millennium Villages Project (MVP), which is a partnership between the Earth Institute, UNDP, Millennium Promise, 12 host governments and 15 communities (~400,000 people), is an ideal place to practice adaptive implementation. The MVP is a time-bound, costed approach to achieving the Millennium Development Goals in an integrated manner. Close partnerships between communities, technical advising and United Nations Development Program operational support creates an environment where CBMH develops local solutions while finding core processes that work over many countries and environments. Since CBMH provides a framework for the exchange of information and processes over disparate environments, unanticipated solutions for financial and human resource sustainability have emerged through productive interactions.

4. COMMUNITY BASED MACHINE LEARNING: ICT FOR CBMH

Information and Communication Technologies for CBMH, when appropriately used, can greatly enhance the cycle-time of making informed management decisions for the development or maintenance of CBMH. This approach is predicated upon requesting the minimal information from populations and associated health providers to improve service delivery. Ideally, this information should be embedded in the exchange of services rather than requiring the critical intrusion of traditional Monitoring and Evaluation frameworks (extra-systemic and sporadic benchmarks and system diagnostics). Using this approach to ICT, the greatest utility of nearly ubiquitous technologies such as mobile phones comes when information is immediately made available to the provider and patient after interacting with the CBMH “core”. In turn, new information modifies this core in a weighted manner, leading to an increasingly evidence-based and appropriately experience-based approach to providing health care at all levels of the health system. ICT for CBMH focuses upon operational and service delivery aspects of health systems as much as providing decision-support for all levels of health workers, including Community Health Workers.

5. WHAT IS THE CBMH "CORE"?

The CBMH "core" is comprised of all the human and ICT interacting elements of the health system – the CBMH "core" is in turn the community upon which the process of improving health care is brought to bear (i.e. via CommCare [2]).

6. COMMUNITY LAB

The call for community-based approaches to health for all was raised at Alma Ata in 1978. Over the past 30 years, models to achieving this goal have been proposed, implemented and reviewed. Increases in development aid have come with an increasing emphasis on monitoring and evaluation, resulting in a tension between the expectations of donors, country plans and community needs. Mechanisms to align or harmonize these multi-stakeholder agendas have been proposed and operationalized in the form of the Global Fund processes and improved communication. Nevertheless, difficulties in implementing static models has required the development of more flexible implementation approaches that still provide mechanisms for accountability and progress indicators. The adaptive implementation approach for CBMH brings community mobilized voices and leadership resources in alignment with operational and management approaches to achieve better health in a sustainable manner. Most importantly, we feel that this process changes the ethical dimensions of engaging in community development: when native languages of fields that are brought to bear upon "the field" are oscillated across exchanges at their interfaces, all benefit from the harmonious merging of the laboratory with the community. Our common challenge is striving to learn how this can be true by ever-tuning into the frequencies where voices are heard.

1. ACKNOWLEDGMENTS

Thanks to Six Silberman for an illuminating email exchange earlier in the day before I wrote this where he introduced me to a wonderful piece called "Cybernetics of Cybernetics" by Heinz von Foerster (1979). Like Reinhold Neihbur said when accused of plagirizing an article, "these ideas have been rattling around for centuries, although I swear I wrote this myself"* – hence the use of "we" in this...thing.

*almost surely a misquote – I have a limited internet connection here in Kisumu, Kenya and I'll be grateful if this simply gets in before the deadline.

2. REFERENCES

[1] "Cross System-level, Cross-layer Cooperation to Achieve Predictable Systems from Unpredictable Components" CCC Visioning Proposal, 2008.

[2] Lesh N, Jackson J, DeRenzi B, Gayo T, Dhadialla PS. "CommCare: A Mobile Phone Application to Strengthen and Monitor Community Health Programs." February 2009.

[3] Dhaliwal Z, Frydman F, Lesh N, Dhadialla PS. "Community Lab: Communities serving Communities" March 2009.

Artificial Intelligence for Development (AI-D)

Nathan Eagle [*] [†] **and Eric Horvitz** [‡]

[*]Santa Fe Institute,[†]Massachusetts Institute of Technology, and [‡]Microsoft Research

Position Paper for *CCC Workshop on Computer Science and Global Development*

The 2 billion mobile phone subscribers living in the developing world continuously generate an unprecedented amount of movement, communication, and financial transactional data, as well as a variety of other heterogeneous and longitudinal data ranging from census data to medical health records. This position paper proposes that artificial intelligence, when applied to these massive datasets, could have profound implications on a wide range of global development areas ranging from public policy and urban planning to disease control.

ICT4D | mobile phones | artificial intelligence | complex social systems

Never before have we had the opportunity to mine such a wealth of information on the complex behavior of human societies. Within the last decade, new methods of quantifying interactions have resulted in behavioral datasets many orders of magnitude larger than anything previously obtainable. Although most analyses of these data have focused on understanding Western societies, under-studied populations within the developing world are generating data that hold even greater potential.

Earlier this year, the number of global mobile phone users exceeded 4 billion - with the majority of these living in the developing world. The implications of this fact are profound: over 2 billion people in the developing world are effectively carrying computers that can transmit information about their relationships, movements, and even financial decisions. While the privacy implications of this data should not be understated, we believe these types of massive datasets can be utilized in ways that better serve both the billions of people who generate the data, and ultimately the societies in which they live.

The Opportunities for AI-D

There has been great interest in Information and Communication Technology for Development (ICT-D) over the last several years. The work is diverse and extends from information technologies that provide infrastructure for micropayments to techniques for monitoring and enhancing the cultivation of crops. While efforts in ICT-D have been interdisciplinary, the field has largely overlooked opportunities for harnessing machine learning and reasoning to create new kinds of services, and to serve a role in analyses of data that may provide insights into socioeconomic development for disadvantaged populations. The unprecedented volume of data currently being generated in the developing world on human health, movement, communication, and financial transactions provides new opportunities for applying machine learning methods to development efforts, however. Our aim is to foster the creation of a subfield of ICT-D, which we refer to as AI-D, to harness these opportunities. To this end, we hope to bring together a critical mass of researchers who are interested in applying AI research to development challenges.

The initial goals for this position paper are to, (1) identify representative datasets in this realm, and (2) to lay out potential AI-D research projects and opportunities. Topics will include the potential for machine reasoning to make valuable offline and real-time inferences from the large-scale mobile phone data sets currently being generated in the developing world. Such analytics could provide a better understanding of social relationships and information flows in disadvantaged societies, as well as guiding and monitoring ICT-D interventions and public policy, and giving insight into population responses to crises. We will also explore how machine learning and inference could help us understand human mobility patterns, yielding real-time estimates of the progression of disease outbreaks, for example, and guiding public health interventions. Machine reasoning could also provide remote areas with medical support through automated diagnosis, along with guidance for the effective triaging of limited resources and human medical expertise. Other applications include instant machine translation for better communication and coordination among people who speak different languages, user modeling for online tutoring, investment advisory tools, and simulation, modeling, and decision support for agricultural optimization.

AI-D Data

Call Data Records (CDR). While obtaining access to operator databases is not a trivial process for researchers, today's mobile phone service providers occasionally allow limited access to the data they log about their subscribers' behaviors. This data, typically referred to as call data records (CDR), consists of all communication events (phone calls and text messages) as well as the cellular tower that enabled the communication to occur. When represented as a network, CDR consists of nodes as individual subscribers and edges as interactions. Edges between nodes in CDR can be multi-dimensional, representing phone communication, text messaging, airtime transfers, proximity, and even financial transactions. While subscribers are represented as nodes, each node can have a wide range of temporal attributes including location, tariff plan, phone type, regional affiliation, language, services adopted, and even demographic data.

Mobile Client Data. Collaborating with mobile operators is not the only way to study mobile phone data. We are deploying specially programmed phones that continuously collect even more in-depth behavioral data. Ongoing studies involve subjects ranging from taxi drivers in Nairobi, teenagers in Delhi, and male prostitutes in Mtwapa.

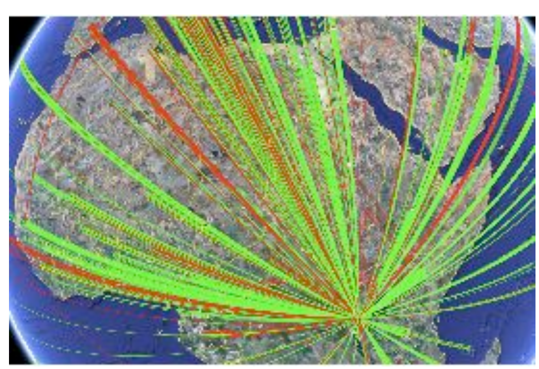


Fig. 1. The international calling patterns during a day in Rwanda.

Additional AI-D Data. There are many different types of additional behavioral data that could be relevant to AI-D:
Online Communication. Email, IM, Facebook, MySpace
Urban Data. Crime, Commercial Districts, Slums, Traffic
Financial Data. Local Market Prices, Monetary Flows
Census Data. Regional Income, Disease, Vocation

AI-D Research Challenges

Applying artificial intelligence techniques to the data above provides valuable insights into a variety of critical development issues, from public health to city planning to basic economics and social behavior. Working with epidemiologists, for example, we are attempting to identify behavioral signatures associated with regional disease-outbreaks, and to model human movement in East Africa in order to support informed decisions about allocation of malaria control resources. Using data from every mobile phone in Rwanda over the last four years, we are working with the city planners of Kigali to understand the dynamics of slums and the impact of policy decisions ranging from road construction to the placement of latrines. In collaboration with developmental economists, we are quantifying a society's reactions to exogenous events, such as the collapse of crop prices in local markets or natural disasters, such as droughts and earthquakes.

The Social Impact of Urbanization in the Developing World. Urbanization is occurring in the developing world at an unprecedented rate, however the social and political consequences of these rapidly changing societies are poorly understood. Now that we have over four years of data on every mobile phone subscriber in a country, however, we can identify individuals who live in rural areas during year one, and then move to urban areas in year two. By comparing their social networks before and after this transition we can start to quantify the effect of living the city on individual behavior. Indeed with several years of data, we can also learn if these individuals maintain relationships created in the urban area if they move back to their rural home.

Mobility Patterns for Disease Modeling. A major rationale for quantifying mobility patterns is to model the dissemination of a contagion, whether it is an airborne pathogen or the diffusion of a parasite such as malaria. The majority of epidemiological models assume that the host population is well mixed, such that the probability of infection is equal for all. Social network structures are clearly not always well mixed, however, and the complexities of people's interactions and movements may have profound implications for the interpretation of epidemiological models and clinical data. The accurate quantification of a population's movement and contacts, and therefore the associated variability in the probability of infection, is clearly of great importance. While hypothetical models are valuable for understanding the kind of effect different social network structures would have on disease spread, CDR can provide a much more realistic interpretation of human social network dynamics. With detailed data on mixing parameters within currently unstudied populations of Africa and the developing world, epidemiologists will be able to make informed decisions about the placement of public health resources for the control of endemic diseases like malaria, and strategies for the prevention of epidemics due to emerging diseases such as Ebola.

Towards a Physics of Society. The recent analyses of data from mobile phone service providers have given us new insights into the aspects of human movement patterns that are shared in all societies. While some researchers take issue with labeling these insights as "universal laws of human movement", it is clear that through the analysis of cellular tower location data from hundreds of thousands of people, it is possible to finally quantify some of the more fundamental rules of human mobility. As researchers replicate these findings in increasing numbers of different countries and cultures, we are beginning to observe general rules governing the "physics of society". The volume of data that is now available from across the globe will allow us to determine to what extent these rules are universal.

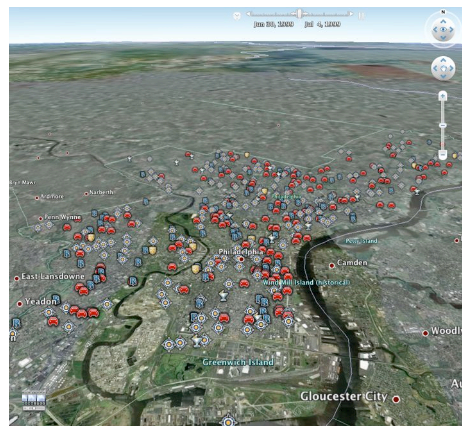


Fig. 2. A complete set of reported crime events over one week - this data was extracted from 330,000 spatial-temporal crime events within the city over the course of a year. This is an example of the continuous, longitudinal data being collected about human behavior within cities.

Additional AI-D Topics. There are a variety of other topics that AI-D potentially can address including:
Expertise and Accuracy Inference. Mobile Crowdsourcing
Generative Urban Models. Dynamics of Slums
Transport Planning. Road Optimization
Speech Interfaces. Recognition & Synthesis
Real-time Translations. IM / SMS applications
Crises Detection. Disease, Natural & Economic Disasters
Remote Diagnosis. Medical & Agricultural

CONCLUSION

As our sample size inevitably expands into the billions, it is important to consider the implications of planetary behavioral data and comparisons across cultures and continents. At first glance, it appears that some information spreads through a village in Kenya in a very similar way as it spreads through Greater London. Mobility patterns in the Dominican Republic are comparable to those among Rwandans, which bear striking parallels to movement in San Francisco. While we may be nearing the point of making claims about universal laws of human behavior, we believe the harder questions occur after we fit the distribution and declare an understanding of a complex social system: how can we use these data and insights to improve people's lives?

Environmental monitoring as the killer app for distributed sensing research: possible lessons for computer science and global development

Deborah Estrin, UCLA Center for Embedded Networked Sensing (CENS)
Draft: May 25, 2009

From its inception CENS (NSF Science and Technology Center for Embedded Networked Sensing, funded 2002-2012) has struggled with its own identity--were we about computer science research first and important applications second? Or vice versa? Like a pendulum, our annual site visits would emphasize one end of the continuum and then the other. Whichever way we put ourselves forward, the other way appeared to stand out as inadequate. Moreover, when within each project we would try carefully to achieve a balance, we sometimes found ourselves doing neither well.

We eventually learned some lessons, found a sort of dynamic balance, and its seems are having some success in applying these lessons to new domains (such as health), suggesting some relevance of the experience to achieving a sustained engagement between computer science research and global development.

- **Meaningful multi-disciplinary collaboration is essential and not for the weak of heart:** Its just not possible to do this well without intimate and prolonged engagement between those developing new technologies and those who have deep knowledge about the application domain. From design of instruments to design of experiments it essential to engage domain knowledge and generally entails an iterative process. As part of the process, the most successful graduate students (particularly PhD) effectively do a minor study (at least) in the domain discipline so that over time they establish the ability to innovate and iterate internally as well as through collaboration. When we have been able to engage doctoral students (or postdocs, but NOT only profs) on both sides of the tech-app divide simultaneously, we have been most successful. However, early on in the development of our field this was difficult because the technology lagged too far behind to be useful to domain science students who after all wanted to make new scientific discoveries. One of the important insights here, was that a technology might be 'relevant or applicable' to a domain science, but it is not going to attract the best and the brightest unless it really has a chance of making a meaningful dent in the application problem domain, not just an incremental improvement. Using any new instrument or methology is time consuming and has unexpected costs and sources of error. So we were much more successful where the magnitude of potential impact was quite large. This also lead us to the realization that has been articulated by Eric Brewer and others, that initially it is the embracing of mundune approaches to the domain application needs that can lead to the best initial results not only for the domain scientists but also for the computer scientists. Because by deploying the 'mundane' or available, you can get away from the head game/simulation, and get back real experience, real surprises, real opportunities that then inspire technical innovation (for us, mobility, imagers, and interpreting in situ measurements in the context of GIS server side models and data were all examples of findings and new directions that came out of this 'surrender to the mundane'). Ultimately it is about respect for the domain topic, and about putting the fear of 'I will never find a thesis, I wont publish enough papers' aside to pursue matters of importance.

- **Challenge of building the ecosystem:** An area of study cant really take root and be sustained without having support in all aspects and phases of the ecosystem. Computer science students have to be able to find thesis topics. Faculty and students alike have to understand and define metrics for evaluation of the work. Students have to be able to publish in respected venues. And of course there has to be a funding stream. In fact, if there is a funding stream its remarkable how all the other issues can end up working themselves out! Finally, what happens to the students? Where do they go? What do they do when they graduate? Is this a process that they then apply to more traditional domains and topics? Or does it grow to be a domain itself, as “greentech” could end up being for students who studied environmental monitoring as their killer app?

- **Pit-falls**: Some of the pitfalls we encountered in more than one area and at more than one time are worth additional mention, even though they are to some extent the complement of the constructive prescriptive comments above. First insufficient engagement of real domain experts and users can easily lead to poorly defined problems or assumptions which can undermine the relevance and impact of the end solutions. The results might get published in the CS literature, might even get published more easily. But they may well be irrelevant, unusable, and therefore unable to contribute to that positive cycle of iterative use and innovation. This lack of engagement is related to a second pitfall of fetishizing abstraction. Computer scientists love abstraction, they love generality, they consider application specificity an unnecessary evil. This desire to meet the professions standards for abstraction is on the one hand healthy, and has produced one of the most broadly applicable technologies of our century, however, if you abstract too early, you end up with irrelevance and starve the process of interacting with reality that is often needed to move things forward. On the other hand, and that is what makes it hard, is that the need to engage meaingfully with the application in order to know where the problems lie and what constraints and assumptions apply, means that we are unable to project 10 years forward and go from there...we dont have the technology of 10 years from now. We have todays technology. So if we have a model of iterative (NOT the same as INCREMENTAL!) innovation, it means we ar always somewhat more tied to the now than to the later. A fourth pitfall that plagued many of our early applications was that the CS students did not have or invest in a deep enough understanding of the domain and so their creative juices and critical voices were not well matched to their work. As time went on and students had the time to learn more deeply about the domains, and as we took n some domains that already matched the students individual life experiences (participatory sensing for example), things began to gel and cycle more successfully.

- **Leveraging the up-sides:** Finally, with all these challenges it is worth mentioning at least two huge upsides that by themselves make it all worthwhile and are likely to fuel the sustained attention it will need to create an ecosystem of computer science research for global development. The first is that the problems are without a doubt, inspiring in their potential relevance and impact. How innovative the technical solution will be its hard to know going in. But its not hard to know how important the problem is. Related to this is that students are very motivated by these global issues, and a more diverse type of student body seems to be drawn to a definition of computer science that embraces this sort of research.This opportunity to attract and engage is playing out for us at the graduate level, undergraduate, and even high school. High school students and teachers see it as transforming their view of computer science as a discipline and career.

A quilt of creativity: ICT knowledge and tool gaps for knowledge rich economically poor people

Anil Gupta
Indian Institute of Management
anilg@iimahd.ernet.in

Many of the gaps identified here were formally articulated during 1993-2001. Since then broadband access has increased in India and many other developing countries. Millions of mobiles have replaced PCs as the most democratic communication platform (although low end models far outsell the higher end-high memory mms enabled models). When SRISTI had visualised setting up the first ICT kiosk in a village in Mehsana district and other regions in Gujarat in 1995, it took Infodev five years to decide whether to support it. Eventually when it was set up in 2000-1, the bandwith was hardly 24kps and connection did not last more than a few minutes if at all. The CD based updating model seemed logical. The scanner proved the most useful ICT tool rather than communication or any other most logical gadget. The farmer and the women used it for scanning old marriage cards, photographs, border of sarees etc. In the GMK (Gian Manthan Kendra/Village Knowledge Churning Centre) several other data bases of cultural, bio-diversity and technological databases. Photographs of the utensils and the kitchen were organized in a knowledge based just as any old cradle or other artefact photographs and kept in the database. The idea was that before bridging the digital divide between north and south and urban and rural, there was a need to bridge the knowledge divide and gaps within the communities. Unfortunately, subsequent kiosks have not paid much attention to using kiosks as a repository of local talent, skills, cultural, biological and other resource diversity, traditional knowledge and innovations etc. A highly utilitarian model has evolved in which cultural and aesthetics have little space. Even the people to people learning has not been stressed enough.

The ideas that I am mentioning below need technological, cultural as well as institutional innovations. I have always argued that technology is like word, institutions like grammar and Culture is like thesaurus. We need to build synergy among all the three dimensions.

1. Agenda for Education

1.1 Open source tools, libraries, databases of educational aids for children in different languages and using different media are necessary for democratising access to education. There exists no special search engine for education. It is a paradox that most research is done in educational institutions and yet children have to search so hard to find for example, animation of eclipse in a properly illustrated manner for primary school children. What is available is neither interesting nor very educative. There are thousands of other such subjects on which somebody somewhere may have worked but lot of children in other places remain deprived of that knowledge. Children themselves should be encouraged to discuss their ideas across the world.

1.2 Campaign for documenting students' ideas, innovations, traditional knowledge of elders and unsolved problems has to be mounted along with mentoring and incubation support. The concept of global classroom in which children from anywhere can join with a few mentors monitoring and tracking the discussion to ensure accuracy and authoritative resolution of conceptual conflicts, has not yet taken off.

1.3 Technology students, almost everywhere do a project in their final year of B.Tech., M.Tech., etc. and yet there is no portal which pools at least the summary of these projects. New portal www.techpedia.sristi.org is an attempt in this direction. How else would one know that four girl students in a polytechnic in Latur in Maharashtra, India had tried to develop a black box for the car. Insurance companies might find information in such a black box extremely useful for fixing responsibility and compensating the affected parties. The safety people can use this data and design safer vehicles. New hot spots of creativity will emerge by mapping the minds of young people. Originality will be rewarded.

1.4 Inter institutional and across the border collaboration among the students and common people such as grassroots innovators, small scale entrepreneurs, mechanics, labourers etc., will require online technology collaboration platforms with different kinds of open source design and fabrication tools (see fablab at MIT). So far such collaboration has not worked out much. The tools for mobilizing the knowledge and experience of workers in the fields or at the shop floor to make the world more green, safe and efficient have not yet emerged.

1.5 Lot of research by Ph.D. and other post graduate scholars is done without an obligation to explain the findings in simple language to the lay people. The accountability of science to society in this form has not yet been institutionalised. It is not a requirement at any University that every scholar should also share one's finding in local language with local communities in a manner that they can understand. But why not? Tools for translation are being developed but the resource allocation for the purpose is totally out of sync with the need.

2. Innovations

2.1 Local language content accessible on open source platform which inspires, instigates and implores people to overcome their inertia and indifference towards more creative and efficient solutions for local problems. We do not have adequate capabilities for delivering searchable Indian language content on Linux platform.

2.2 Multi-Media Multi Language (MMML) learning and disseminating centres: Gyan Manthan Kendras (knowledge churning centers) for creating appetite for innovative solutions, learning from common wisdom, connected to Honey Bee database at NIF and SRISTI and other Honey Bee collaborators.

2.3 MMML museum of innovations, both fixed and mobile to kindle curiosity, generate understanding of creative problem solution, share historical global knowledge about roots of innovations. It will encourage visitors to download information on hand held devices and take with them away as knowledge memories of visit to be shared with kith and kin.

2.4 Virtual multimedia portals like Indiainnovates.com become one point window on the creative and innovative face of India, generate demand for local innovations, operate technology exchange (like stock exchange with bidding/auction model for innovations) and pose problems for solutions by students and public spirited scientists and technologists. Collaborators.

2.5 Web enabled multi-language GIS based database on innovation, traditional knowledge and biodiversity to track the evolution of dynamic local knowledge systems, enable people to people sharing for expanding knowledge repertoire, and link knowledge, resource, institutions, and technology (KRITe) across space, time and social-cultural boundaries.

2.6 Distributed Multi-media multi-language content creation studio and tools for creating decentralized community knowledge and cultural heritage libraries.

2.7 Integrated MM National Database on Biodiversity and its uses, conservation status, people's Biodiversity and knowledge registers in Indian languages to make local communities have access to this extraordinary knowledge base scattered in different labs, institutes, bureaus and departments.

3. Design of Technologies

3.1 User driven innovations are persuading even the large corporations to reorganize their design strategies. However, grassroot design centre are not available for young and old people to make their own cell phones, PCs and other gadgets for urban and rural applications. We need to create such design centres where people can design their own versions of ICT applications. If a tablet can be combined with the cell phone and a small projector can be fitted into it, one might get lot of functionalities of computer in cell phone itself.

3.2 Incorporating weather and other such environmental information into village level knowledge management system (VKMS) will require integrating expert systems with real data interfaces. A farmer can then plug in the parameters of his farm and generate real time options.

3.3 Honeybee on mobile is another application that remains to be developed. It would also require real time expert systems linked to innovation database in multiple languages and multimedia. Thus if a farmer wants to know solution to a pest problem in paddy in September third week, the system should identify the likely pest which would affect crop in the region from where SMS has come and at the stage of crop cycle at which it has come. In due course it can be made more sophisticated with reference to input management conditions as well. The database will find the farmer based innovations and SMS the same to the concerned person. In many cases it may be voice recognition protocol which is preferred over SMS.

3.4 There is a tremendous scope of embedded technologies and control systems being incorporated in the everyday tools used by labourers and workers to make them more efficient and reduce their drudgery.

A great deal of new applications needs to be generated which transform the capabilities of ICT tools for the common people whose life remains often untouched or touched not always positively.

The Role of Academia in Social Entrepreneurship

Jonathan Jackson
Dimagi
529 Main St, Suite 606
Charlestown, MA 02129
+1-617-580-3100
jjackson@dimagi.com

Cory Zue
Dimagi
529 Main St, Suite 606
Charlestown, MA 02129
+1-617-580-3100
czue@dimagi.com

ABSTRACT

This position discusses interaction between social enterprise and university research to further the field of computer science for global development. The perspective discussed is from the viewpoint of a social enterprise that has been deploying IT solutions in low resource environments for the past seven years.

1. INTRODUCTION

Dimagi, Inc. is a social enterprise based in the United States that focuses on building open source health solutions for low resource settings. Dimagi was founded in 2002 by students from MIT and Harvard who had all performed research on PDA software for health solutions (a sector that is now called mobile health). One of the projects was a diabetic game for children that was researched in Boston, MA, led by Dr. Vikram Kumar. Another was Ca:sh, a mobile solution for community health workers in India to provide decision support and tracking of immunizations for children, led by Dr. Vishwanath Anantraman. Through this research, it became clear that there was a need to use the innovative, rapid prototyping popular in research labs like MIT's Media Lab to target problems in resource constrained areas. It was hypothesized that you could create a sustainable business entity to perform this work either through product or consulting revenue. These conclusions led to the formation of Dimagi.

2. HISTORY

Since 2002, Dimagi has grown slowly and steadily through consulting projects and grant revenue. During this time period its business model has been revised several times. While initially Dimagi thought it would be product based, the idea of generating profit through selling a proprietary product quickly become difficult to reconcile with the social mission of the business. As a result, Dimagi committed to exclusively providing open source software solutions. Another of Dimagi's goals was to provide as impartial a service as possible to partners to find the right solution for a particular problem. The mission of the company is to create the biggest impact possible, and not necessarily to create the most revenue possible. As such, Dimagi purposefully set off on two goals in 2004: 1) to expose Dimagi to as many technologies, projects, and partners as possible to determine where and how to make the biggest impact and 2) to ensure Dimagi could selectively choose the most innovative projects to participate on.

Seven years later, Dimagi is still trying to accomplish these goals. Dimagi has been fortunate to have worked with partners across many different health sectors and thus has done well in getting exposure to many different types of projects. Dimagi has performed projects in 12 different countries, and is continually adding new technologies to its portfolio. Dimagi, its partners, and the US Center for Disease Control, have built one of the largest Electronic Medical Record systems in use today: SmartCare. As of mid-2008, SmartCare was in use in 459 sites in Zambia and contained over 120,000 patient records. It has also been piloted in South Africa and Ethiopia. More recently, Dimagi, partnered with UNICEF, has deployed an SMS-based Malaria Net tracking application in Nigeria on an open source platform called RapidSMS. If used for the entire Malaria Net campaign, the system will have tracked the distribution of 62 million nets, the largest net distribution ever performed.

Dimagi has been a huge proponent of open-source technologies specifically designed for use in developing countries. As a founding member of the OpenROSA consortium – a group dedicated to providing open standards for mobile protocols – Dimagi has led development and fostered the community surrounding the consortium's flagship product. That product, JavaRosa, is a data collection and decision support tool designed to work on a broad range of low cost mobile devices. Finally, Dimagi has recently come full-circle, and is using JavaRosa to build an application to aid community health workers in their home visits and patient management. That project, CommCare, is currently being piloted in Tanzania and Bangladesh.

3. MEASURING IMPACT

3.1 The Difficulty

Measuring impact is a challenge that has long plagued IT development projects. One cause is that proper study design, measurement, and analysis can often be more expensive than the IT intervention itself. The current explosion of mobile health projects is a good example. One of the appeals of the mobile health field is that the solutions can be deployed at scale for a low cost (relative to more traditional solutions that require more expensive equipment, such as desktop computers with UPS power supplies and solar charges). While it is often true that a single targeted intervention will be more cost effective on mobile devices relative to PCs, it has not been established how effectively mobiles can be used for many different verticals at the same time.

A second problem with measuring impact is understanding the relative and cost-based impact of projects as they scale. Due to the ubiquity of donor organizations and absence of large-scale funding for IT projects in developing countries, many IT projects never make it past the pilot phase. This presents evaluation challenges on both sides. On the one hand, pilots contain many fixed costs that would not be associated with a long-term project, such as the development cost of the software. However, the impact of pilots can often also be over-estimated because of the attention and resources devoted to them. An example of this is the SMS bednet distribution system Dimagi helped build with UNICEF. While the costs for the pilot were high, the software is now stable and ready to be used with little extra work for the remainder of the campaign. However, it's extremely difficult to predict the relative effectiveness of the application for the rest of the campaign – when significant amounts of Dimagi and UNICEF's time and resources will not be spent to make sure that everything is running smoothly on the ground.

Measuring impact is a challenge, and this assumes that there is some person or process in place to measure impact. More often than not, solutions are built first and evaluated long after the results are useful (or sometimes never).

3.2 Comparative Effectiveness

Measuring effectiveness of IT interventions is challenging. Measuring comparative effectiveness benchmarked against cost is even more difficult.

A huge problem with measuring comparative effectiveness is the landscape under which IT projects in developing countries are run. More so than the academic world, development IT projects are largely independently run within the context of a single organization. Additionally, often times different organizations run similar interventions with competing technologies or technology stacks, and collaboration can be rare. Even within the open source communities we work in, we have witnessed strong pressure from the funding organizations to be loyal to the projects those organizations are contributors to.

Because of the disparate nature of these projects, comparative evaluations are extremely rare. No individual organization has the incentive to do a comparative assessment, unless it is to tout its own solution against the alternatives, which are not the conditions under which these programs should be objectively evaluated.

3.3 True Impact

Measuring the true impact of a particular project is even more challenging because one must take into account the many inputs and outputs over a long period of time.

Many donor-funded projects appear quite successful, but after the money disappears, the project fades and the long-term effects of the intervention are not known. The real-world analogy to this problem is the organization that brags about the thousands of miles of roads it has built, only to do nothing 10 years later when the roads are full of pot holes and unusable.

4. UNIVERSITY RESEARCH

4.1 The Benefit

Universities can provide the solution to many of the problems in measuring the impact of technology interventions. University researchers are ideally suited to assess these projects because they are unbiased, interested in learning the process of research, properly incentivized, and economical.

The role of a researcher is to provide a novel answer to an open question. In the case of technology in developing countries, the researchers will likely be interested in measuring the immediate benefit of an intervention, comparing the effectiveness of different interventions on a cost basis, and determining the true impact of the intervention on a longer time scale

Unlike the organizations implementing or funding the projects, universities have less vested interest in the outcome of their assessments, and more in the credibility of the results. This makes university researchers better equipped to deliver unadulterated, unbiased results than the organizations implementing the interventions.

Additionally, university researchers can be more thorough, and less cost-sensitive because learning research techniques is an explicit goal. To a student, learning the process of research, exploring the issue in depth, and having interesting results in their thesis is the motivation for their work. Students are not paid by the hour, their rewards come from the depth and insight to which they are able to explore the questions at hand.

5. Further Discussion

While the issue is complex, it is likely that university research is in an ideal position to take a leadership role (or *the* leadership role) in creating the methods and the application of them to determine if IT interventions are actually improving global development. IT projects in the context of global development present an interesting dilemma in that every dollar spent on IT is a dollar not spent on food, water, or shelter. This requires the practitioners in the field to truly demonstrate the value of the interventions, due to the obvious alternatives. Unfortunately, to date, these evaluations are extremely hard to find.

Computer Science for Global Development: Research Methodology and Agenda

Ravi Jain and Joe Mucheru
{ravijain, jmucheru}@google.com

This paper is not a formal research paper. Its goal is to stimulate discussion at the Computing Research Association workshop on this topic. The views are of the authors and not necessarily of Google.

ABSTRACT

The goal of this workshop is to suggest ways in which CSGD can become a more rigorous, legitimized, research discipline within Computer Science. We take the position that the goal of the discipline is not primarily advancing technology but assisting development, particularly poverty alleviation and access to relevant ICT in developing countries. This orientation emphasizes not only advanced research programs in developed countries but the role of practice, technology adaptation and deployment, local direction and local capacity building. We also present several methodological proposals: projects should explicitly state end goals (philanthropy or commercialization), the target demographics, and the extent of local participation. We discuss the need for not only technical metrics (e.g. performance and cost) but social and economic impact, usability, sustainability and replicability. To support these additional metrics, and their rigorous evaluation, specific support should be included in project plans.

1. CLARIFYING THE GOAL

The critical question is whether the primary goal is advancing the state of the art of technology, or assisting the process of global development, particularly poverty alleviation and access to relevant ICT in developing countries. While a facile answer is "obviously both", we take the position the primary end goal is the latter, which raises some issues we frame in terms of the following objectives.

Technology adaptation and deployment. While innovative research in new technologies is no doubt worthy, the mission of CSGD should include addressing the substantial need to overcome the myriad problems of adapting and deploying existing technologies in the context of the poor and under-served in developing countries, and to replicate successful efforts.

Pragmatic and non-formal efforts. Significant advances on the ground are being developed by practitioners, sometimes outside the formal discipline. An example is the communications solutions utilizing innovative uses of SMS, WiFi and mechanical networking that have been developed more or less independently in several countries. In contrast, researchers in formal research settings face requirements such as publications, grants and students that can be in tension with the goals of developing workable solutions that promote development in their target communities. CSGD should include objectives to develop further support, infrastructure and recognition for these pragmatic efforts.

Local direction and capacity building. CSGD should actively and conscientiously focus on developing the infrastructure, support and capacity of researchers and practitioners in the developing countries to design and deploy solutions, rather than relying on research that is solely or largely directed by researchers or donors in developed countries. This includes mechanisms such as partnerships, joint research projects, student and faculty exchanges, and research grants. There needs to be liberal financial and logistical support for qualified researchers and experts in developing countries to travel to conferences, to serve on program committees, and to collaborate with and inform the efforts of colleagues in developed countries. There should also be support for promoting research, interaction and replication efforts between developed countries. This should not be regarded as external to the research project funding but a core part of it, to help meet the long-term goals of the discipline.

2. METHODOLOGY

Each research program or project needs not only a statement of its concrete technical objectives, but a clear "meta-statement" of its goals. For example, the following questions should be explicitly answered:

> *Is the underlying motivation philanthropy, near-term commercialization, or something in between?* Computer science can be applied fruitfully across the entire spectrum. But a philanthropic project is likely to have different constraints, time horizons, methodology and stakeholders than a commercial one. Many development-oriented projects fail to make these assumptions clear, or state somewhat vague intentions of starting with philanthropy and ending with commercialization in some unspecified future. There is room and need for exploratory or open-ended research just as there is for deployable solutions, but not stating the intention clearly makes evaluation difficult.
>
> *What is the target demographic of the project, and what are its salient relevant characteristics?* For example, development projects typically target the lower part of the economic pyramid, or users in developing countries, but these statements are insufficient. Solutions that make sense for the middle half of the pyramid often make no sense for the lowest quartile, even though both are underserved. There can be vast differences between emerging economy countries. For instance, while India has a much higher GDP than Kenya (about $2700 vs $1700, in terms of 2008 PPP), the latter has a much higher literacy rate (85% vs 61%). Similarly, there are differences between different segments of the pyramid within each country, and between urban and rural populations; being clear on the demographic being targeted is critical to the research approach and project evaluation.

Many of these questions are straightforward and obvious, but experience in looking at technical papers, both published and submitted, shows they are often ignored, left implicit, or answered vaguely. This not only makes the evaluation task harder but raises questions about how well the project itself has been thought out.

Based on the goals outlined in the previous section, research projects should also address the following regarding local participation:

> *Were requirements gathered from a reasonable cross-section of local participants and stakeholders?* Attention to requirements methodology is especially important when information about the target demographic is difficult to come by and its tempting to make broad assumptions.
> *Were local researchers and practitioners involved in interpreting the requirements and in designing the solutions?* It is not always possible to involve local staff in every project. But local researchers and domain experts can help ensure the project is solving the right problems and asking the right questions.
> *Will local capacity to carry out similar or related projects be enhanced in some way?* Are there components of joint research direction, training, or infrastructure development that were incorporated?

3. METRICS

Aside from technical metrics for a project (performance, capacity, cost, etc) we propose some additional metrics:

Impact. There should ideally be some evaluation of social or economic impact. Generally the more quantitative and rigorous this is, the better. Examples include before-and-after studies of indicators in samples of the target population (for a disease monitoring project, say, the decrease in the time from an outbreak to visits by field health workers, or reduction in target population morbidity). Clearly not all projects lend themselves to such an evaluation, but attempts should be made to incorporate them as far as possible. Such studies are expensive and time-consuming, so requisite funding and support should be included as an integral part of the overall research program. Any substantial research program should include support for long-term longitudinal studies that are themselves subject to publication and peer review..

Usability. Specific efforts should be made to evaluate, as quantitatively as possible, usability and appropriateness for the specific target demographic and environment. Metrics would vary by project but could include quantities such as number of unique users, retention rates of users after 30 and 60 days, task completion times, task abandonment rates, and mean opinion scores from surveys.

Sustainability. Economic and logistical indicators of the project sustainability should be evaluated. This is a notoriously difficult area to report and also an area where development projects typically tend to fail.

Replicability. What are the economic, logistical or technical factors that affect the replication of the project in other countries or locales? Which would be good or poor choices for replication targets?

4. CONCLUSION

The primary end goal of CSGD should be assisting the process of global development, particularly poverty alleviation and access to relevant ICT in developing countries. This entails specific methodologies, including clarifying project objectives and target populations, requirements capture and interpretation from local stakeholders, as well as inclusion of broader non-technical metrics.

Towards a Literacy-Based Research Agenda for ICTD

Matthew Kam
Human-Computer Interaction Institute
School of Computer Science
Carnegie Mellon University
5000 Forbes Avenue
Pittsburgh, PA 15213, USA
mattkam@cs.cmu.edu

ABSTRACT
As a fledging discipline at the crossroads of the computing and social sciences, one of the highest priorities in ICTD is to develop stronger conceptual foundations that enable us to address the toughest problems confronting our field. Low literacy is a consistent theme that cuts across multiple areas in "human-computer interaction (HCI) for development": user-interfaces and information visualizations, methods for performing user studies, and technology-aided learning – all in the context of users with limited formal schooling. While much of the above work lack theoretical underpinnings, we are witnessing more recent work which draws on isolated theories from an area called "literacy studies." But drawing on a theory without considering its relation to other theories is just as limiting as atheoretical approaches. As a literacy researcher, I feel compelled to write this proposal since we have a historic opportunity to examine how literacy studies can inform ICTD. The goal of this proposal is to generate a more holistic awareness of literacy studies within the ICTD community, so as to initiate a discussion on how it informs new directions in the computing sciences, in areas such as: HCI, speech and language technologies, as well as artificial intelligence.

INTRODUCTION
UNESCO, the lead United Nations agency responsible for combating worldwide illiteracy, estimates that there are 880 million illiterate adults on this planet, which comprises over 13% of the global population [7]. Indeed, low literacy is a distinguishing characteristic of many marginal communities whose lives ICTD researchers work to improve. Based on a recent literature survey that I did as preparation for a class on "HCI for development" that I taught, I have observed at least three areas that are impacted by literacy-related issues: user-interfaces and information visualizations, methods for performing user studies, and technology-aided learning – all in the context of users with limited schooling.

Out of the above areas, information visualizations and user-interfaces for semi-literature users is most active in terms of the number of recent publications. Existing approaches to designing such visualizations and interfaces usually adopt the stance that since semi-literate users cannot comprehend textual information, the same information should therefore be communicated using other modalities including speech (Plauché et al. [12]) and graphics (Medhi et al. [9], Parikh et al. [11]).

The second thrust is proposed by Sherwani et al. [14], who observes the difficulty of performing user studies with low-literates using approaches such as asking abstract interview questions and conducting surveys based on Likert scales. These methods require a reasonable familiarity with literate practices among respondents. The third thrust is reflected in the work of ICTD researchers such as my research group [5], which investigates how educational technologies such as cellphone-based e-learning games can enhance learning opportunities among out-of-school children.

One of the strengths in most of the above work is that they are backed by considerable user studies. However, none of them explicitly considers the cognitive processes that semi-literates use to perceive and reason with information and information-processing tasks. An exception is Plauché et al. [12], who reviews a few neuro-imaging studies but does not attempt to draw insights based on these studies to inform design. In our work which targets computer-aided learning among out-of-school children, we have observed that it is challenging to come up with designs for those learners with absolutely no schooling. However, our instructional design processes continue to lack a conceptual framework on the learning processes among out-of-school children that could guide our work.

LITERACY STUDIES
It is not possible to provide a detailed survey of the literacy studies literature here. We instead refer the reader to survey articles such as Akinnaso [1]. While this article is dated, it nonetheless summarizes some of the major arguments and controversies in literacy studies that the ICTD community does not appear to be familiar with. For our purpose, we note that some scholars espouse a "developmental view" of literacy, which asserts that it is the acquisition of literacy that leads to new cognitive capabilities (Bruner and Olson [3], Ong [9]).

This view has been challenged by studies such as Bernardo [2], and Scribner and Cole [13], which distinguish between literacy acquired in school and non-school settings. These studies show that respondents who acquire literacy in non-formal settings (vs. school) do not outperform non-literate respondents on most information-processing, experimental tasks. Instead, respondents who acquire literacy in school perform better on most tasks. Indeed, Scribner and Cole's study [13] has become a landmark in literacy studies for demonstrating that the cognitive effects hitherto attributed to literacy *per se* are in fact an outcome of institutions such as schools that are prevalent in literate cultures.

In terms of implications for theory, Scribner and Cole [13] give an alternative view of "literacy as a social practice." In other words, the cognitive impacts of literacy – if any – are not "universal" but arise from particular cultural practices. This view is in turn based on sociocultural perspectives of cognitive development (Luria [8], Vygotsky [15]), which argue that the development of higher mental processes is socially mediated. For this reason, I express concerns that recent attempts (e.g. Sherwani et al. [14]) to ground ICTD research in frameworks from literacy studies such as Ong's notion of orality [9] have not accounted for other (often opposing) theoretical viewpoints.

TOWARDS A RESEARCH AGENDA

While studies in ICTD have shown semi-literates benefiting from ICTs, as a community of ICTD researchers, we need to better conceptualize how literacy and cognition relates to each other. Doing so will enable us to approach the design of user-interfaces and visualizations, experimental tasks, user study methods and instructional sequences with greater awareness about the sociocognitive processes of semi-literate users. Only then can we truly extend the computing revolution to support underserved communities around their unique cultural, literacy and cognitive characteristics.

Where should we proceed from here? Despite Landauer's [6] misgivings about the limited applicability of cognitive theories to HCI, we take diSessa's [4] view that cognitive theories constitute local sciences for HCI. In other words, the search is not for universal design principles. We invite the ICTD research community to join us in discussing the complexities behind literacy, and how our frameworks for design research can take the local into account. Our hope is to have such considerations inspire more vibrant directions in the computing sciences, in synergistic areas such as HCI, speech and language processing, and artificial intelligence in education.

REFERENCES

1. Akinnaso, F.N. The Consequences of Literacy in Pragmatic and Theoretical Perspectives. *Anthropology & Education Quarterly*, 12(3), Autumn 1981, pp. 163-200.
2. Bernardo, A.B.I. *Literacy and the Mind: The Contexts and Cognitive Consequences of Literacy Practice.* UNESCO Institute for Education, 1998.
3. Bruner, J., and Olson, D.R. Symbols and Texts as Tools of Intellect. In *Interchange*, 8, 1977-78.
4. diSessa, A.A. Local Sciences: Viewing the Design of Human-Computer Systems as Cognitive Science. Carroll, J.M. (Ed.), *Designing Interaction: Psychology at the Human-Computer Interface*, Cambridge University Press, 1991.
5. Kam, M., Kumar, A., Jain, S., Mathur, A., and Canny, J. Improving Literacy in Rural India: Cellphone Games in an After-School Program. In *Proc. International Conference on Information and Communication Technologies and Development 2009*.
6. Landauer, T. K. Let's Get Real: A Position Paper on the Role of Cognitive Psychology in the Design of Humanly Useful and Usable Systems. Carroll, J. M. (Ed.), *Designing interaction: Psychology At the Human-Computer interface*, Cambridge Series On Human-Computer Interaction. Cambridge University Press, New York, NY, 1991, 60-73.
7. Lievesley, D., and Motivans, A. *Examining the Notion of Literacy in a Rapidly Changing World*. UNESCO Institute for Statistics, August 9, 2000. http://www.uis.unesco.org/ev.php?ID=4931_201&ID2=DO_TOPIC#fulltext
8. Luria, A.R. *Cognitive Development: Its Cultural and Social Foundations*. Harvard University Press, Cambridge, MA, USA, 1976.
9. Medhi, I., Sagar, A., and Toyama, K. Text-Free User Interfaces for Illiterate and Semi-Literate Users. To appear in *Proc. 1st International Conference on Information and Communication Technologies and Development 2006*.
10. Ong, W.J. *Orality and Literacy: The Technologizing of the Word*. Methuen, NY, USA, 1982.
11. Parikh, T., Ghosh, K., and Chavan, A. Design Studies for a Financial Management System for Micro-credit Groups in Rural India. *Proc. CUU 2003*, ACM Press (2003).
12. Plauché, M., Wooters, C., Ramachandran, D., Pal, J., and N. Udhaykumar. Speech Recognition for Illiterate Access to Information and Technology. In *Proc. International Conference on Information and Communication Technologies and Development 2006*.
13. Scribner, S., and Cole, M. *The Psychology of Literacy*. Harvard University Press, Cambridge, MA, USA, 1981.
14. Sherwani, J., Palijo, S., Mirza, S., Ahmed, T., Ali, N., and Rosenfeld, R. Speech vs. Touch-tone: Telephony Interfaces for Information Access by Low Literate Users. In *Proc. International Conference on Information and Communication Technologies and Development 2009*.
15. Vygotsky, L.S. Mind in Society: The Development of Higher Psychological Processes. Harvard University Press, Cambridge, MA, USA, 1978.

The Need for Robustness in ICTD Systems

S. Keshav
School of Computer Science, University of Waterloo

1. BACKGROUND

Over the past four years, my research team and I have been working on the KioskNet project to provide Internet access to rural areas by means of 'mechanical backhaul' [3]. Inspired by DakNet [2], we used wireless-equipped buses and cars to physically carry data from disconnected kiosks to towns or cities with Internet access [3]. We deployed prototype systems in three sites in India and one in Ghana in collaboration with NGO partners.

Experiences in the field highlighted four significant drawbacks:

- Wireless networks tend to be both unreliable and hard to debug. When possible, it is best to avoid them.
- It was a mistake to rely on Linux and special-purpose hardware (in our case Soekris and VIA single-board computers). Expertise in using these systems is rare in developing countries. The only systems in widespread use are Windows desktop PCs.
- We should not have assumed that field technicians were capable of solving technical problems. Systems need to be entirely self-managing.
- We could not assume the availability of electrical power. Systems should recover after power spikes reboot a computer and delete all in-memory state. Our early prototypes did not support this feature.

Besides these technical issues, the overarching problem is that NGOs already have many problems on their hands, ranging from raising funds to dealing with entrenched power struggles between socio-economic classes. They cannot spare the time to deal with unreliable technology. Systems deployed in the field, therefore, *have* to be reliable. Otherwise they create more problems than they solve.

We used these lessons to re-engineer our solution from the ground up [1]. Specifically,

- We replaced wireless communication between a kiosk-based computer and a vehicle-based computer with the vehicle operator physically carrying a USB-memory key from computer to computer. This eliminates problems with wireless networks. Moreover, we no longer need a computer in a vehicle, greatly reducing costs.
- We ported our solution from Linux to Windows desktop PCs. Because we no longer use special purpose hardware, NGOs need to only download software (at no cost) from our website to provide rural connectivity.
- We do not assume that the users or the deployers of the system have any knowledge of computer systems. We added considerable support for logging so that we can diagnose problems remotely. Besides, system operators can recover from any problems simply by rebooting the PCs.
- We rewrote our system to store all state in a database, so that the system works despite arbitrary failures in any component.

The essential point is that the need for robustness caused us to re-do the entire system. Although the revised system will only be released around the end of May 2009 (which will allow me to discuss its success or failure at the workshop), strong early interest indicates that this system is likely to be widely deployed. We already have commitments to deploy the new solution from NGOs in India, Sierra Leone, and East Timor.

My thesis is, therefore, that *robustness should be an overarching theme in the design of all ICT systems designed for developing countries*.

2. METHODS

Robustness as a design principle is unexceptionable, but what does it really mean? What are the engineering and design practices that will result in robustness? After all, no one wants to design a non-robust system! In this section, I will outline my thoughts on what constitutes robust design and how to achieve it.

2.1 What is robust design?

A system can be said to be robust to a fault if it can carry out its desired functionality, albeit with reduced performance, despite the fault. Robust system design, therefore, requires us to catalog a set of potential faults, and then prove, either by analysis or by actual test, that the system is robust to the fault. In developing countries, the set of potential faults is larger than in developed countries. In addition to node and link outages, and the unreliability of wireless links, we also have to deal with problems caused by dust (jammed CD drives, burnt CPUs), power spikes (loss of power supplies), petty theft (cables that disappear), lack of spare parts, heat, and unreliable infrastructure in general. 'Faults' also include lack of technical training, inability to procure specialized systems, and lack of electrical power. Finally, there is also the problem that field workers have limited abilities to deal with these faults. To design robust ICT systems for developing countries, we should systematically test the robustness of a proposed system to each such fault.

2.2 Two ways to achieve robustness

Instead of each project discovering these problems on their own, I propose that the community should put together a comprehensive list of possible faults (or stressors), as well as the best current practices, both engineering and management processes, to deal with them. We should gather this common knowledge in a shared repository, such as a wiki. Well-known techniques to mitigate, for example, power spikes, or techniques for training field workers, should be placed in this repository. This would give the researchers in the field a chance to employ and refine these practices, instead of re-inventing the wheel with every new project. *One practical outcome from this workshop would be to set up a repository of best current practices for achieving robust system design.*

Some design vulnerabilities are difficult to determine, especially by practitioners who cannot necessarily see the forest for the trees. These could be better determined by an external expert reviewer. I suggest that the community set up a voluntary review panel of experts who could critique a project. Project leaders could submit a project for review and receive detailed feedback from the panel. This approach will not only raise the general level of robustness in ICT projects but also potentially result in cross-institution collaborations. Such a review could also convince funding agencies of the maturity of the community. *A second practical outcome of this workshop should be the selection of a review pool and a process for assigning reviewers to projects.*

3. METRICS FOR EVALUATION

Evaluating ICTD research, and, in particular, systems research with an ICTD focus, should take robust system design into account. Typically evaluation happens at two levels: when funding proposals, and during paper evaluation prior to publication. System robustness should be taken into account in both situations.

Specifically, when evaluating funding proposals, a key metric should be the degree to which the system designers have taken into account the deployment environment. Assuming that a repository of faults and best practices exists, a funding proposal should indicate which faults are expected, and how these are to be handled. The chances that a funded system gets used in the field would greatly improve if writers of funding proposals were forced to think of these issues up front. Speaking from personal experience, we could have shaved off a year of work and much wasted effort if we had had to focus on robustness from the start.

Similarly, when evaluating papers, reviewers should judge whether the system would be deployable, taking into account its robustness. Keeping in mind that robustness sometimes comes as a result of *avoiding* technical novelty, the criteria for paper evaluation should be modified. Again, speaking from experience, when we built the first version of our system, we were 'fully buzzword compatible' using technologies such as HIBC, DHTs, and flat names. The third version of our system is far more robust, provides nearly the same capabilities, and has eliminated all of these exotic ideas. Our current system is likely to be far more useful in the field, but, unfortunately, is not publishable research. This ought to change.

Summarizing, *a third practical outcome of this workshop should be recognition of system robustness as a key evaluation criterion, both for funding proposals and research papers.*

4. CONCLUSIONS

ICT systems designed for developing countries need to be more robust that those designed for developed countries because of intrinsic environmental, social, and economic factors. Many interesting and innovative projects have failed to be widely deployed because they have not taken these factors into account. I argue that we need to focus more on system robustness, even at the expense of novelty, and identify three practical steps that can be taken towards building more robust systems. I hope that by taking these steps we will not only help to make the field more mature, but we will also make the resulting systems more useful to their users.

5. REFERENCES

[1] VLink, http://blizzard.cs.uwaterloo.ca/tetherless/index.php/VLink

[2] A. Pentland, R. Fletcher, and A. Hasson, "Daknet: Rethinking Connectivity in Developing Nations," IEEE Computer, 37(1):78-83, 2004.

[3] S. Guo, M.H. Falaki, E.A. Oliver, S. Ur Rahman, A. Seth, M.A. Zaharia, U. Ismail, and S. Keshav Design and Implementation of the KioskNet System, International Conference on Information Technologies and Development, December 2007.

Including Local Entrepreneurs in ICT4D

Jonathan Ledlie
Nokia Research
jonathan.ledlie@nokia.com

Billy Odero
Nokia Research
billy.odero@nokia.com

Nathan Eagle
SFI/MIT
nathan@mit.edu

ABSTRACT

This position paper outlines both long-term and short-term methods for building platforms that enable entrepreneurs to guide future ICT4D research. While in the future we envision inexpensive and ubiquitous hardware platforms that facilitate rapid application development, it is also possible to build platforms for application development on existing infrastructure. Protocols such as USSD can act as a universal user-interface — enabling any GSM-enabled phone with the ability to browse menus and interact with server-side databases. Alternatives to mobile money transfers can include re-purposing airtime-sharing protocols and scratch cards. Given the significant potential benefits of directly involving local entrepreneurs in testing and evaluating new ideas emerging from the ICT4D research, we see it as a core challenge of this community to build the platforms necessary to ensure this happens.

1. INTRODUCTION

Many projects in Information and Communication Technologies for Development (ICT4D) are based at world-class universities and partner with local organizations for day-to-day operations. This relationship works well for certain types of projects — for example, remote medical examinations. However, a significant group of local entrepreneurs are underserved by the current approach. Instead, much like the phone acts as the springboard for deploying applications in developing regions, local entrepreneurs should be directly involved in testing and evaluating new ideas emerging from our work.

One potential drawback to working directly with entrepreneurs is that it can lead to a short-sighted, incremental approach. They cannot develop technology on platforms that do not exist. It is this community's role to carefully consider and develop cost-effective platforms that they can use — but with a longer horizon than they can afford.

An example of a platform that this research community should develop is a "cloud" for entrepreneurs. If we began from a blank slate and assumed that, like now, many people have phones, what kind of cost-effective client (on phone) and server (local, cheap) platform could be provided so that people could build a business on a shoestring? The roadblocks to doing this currently are so large that it is hard for large corporations. Instead, what if a system like Amazon's Elastic Compute Cloud existed? We believe developers could start businesses with three orders of magnitude less seed capital.

What would this platform look like? It would require phone-side and server-side intelligence. It would require a clear mechanism for deploying new and updating existing applications. It would need to work on existing communications infrastructure (voice, SMS, USSD, GPRS), and tolerate the exigencies of the local network (power failures, high loss rates, long down times). It would also require secure personal and business payment and escrow services. Ideally, because we imagine that many of the businesses the platform would support would be small, the information that they each have should be mutually accessible — both by the phone-side of each application and the server-side.

Legacy phones and their operating systems are perhaps the largest impediment to the platform we envision. Saying that one's application works on J2ME or the iPhone or Android is irrelevant: these are all too heavyweight and expensive. Instead, this community should assume the existence of a very low cost device that multitasks, runs Linux at its core, has a camera, but is memory and processor constrained. In our personal opinion, an open, usable phone-platform like this seems so compelling that its emergence seems almost a certainty. Let us build for it.

We believe that there exist a set of local developers and entrepreneurs eager for such a platform and clearly there is a demand for applications. For example, the Skunkworks group in Nairobi, Kenya [2], is full of technically strong people. However, this group and others like them cannot set the rest of their work aside to develop a clean-slate solution. We see it as a core challenge of this community to develop ideas for and prototypes of this platform, and for the larger industry players to help academics deploy it.

2. CHALLENGE AREAS

We see the main components of this platform as including:

Phone-side components Similar to standard mobile platforms, safe deployment of new applications will require a "sandbox" limiting the application's functionality while protecting important user data. Phone operating systems should provide a set of components that connect to form many types of applications. The concise descriptions of these connections would constitute an application and could be deployed in a few SMS messages. Getting these components and their interconnection language right would be hard.

Server-side "cloud" For the early stages, this would be run much like *PlanetLab* [1], where students would have easy access to SMS, USSD, and voice-based services. Getting sharing to work well in PlanetLab has been difficult, and this hosting service would have similar challenges.

Intelligent networking A reliable DTN transport needs to exist between the phone platform and hosted services.

Financial Services Assuming that banking services like MPESA continue to be successful, how would this open platform offer *transparent* financial services?

3. BOTTOM-UP DESIGN

The design of technology for the developing world has been almost exclusively top-down. Western corporations determine the specifications and functionality of a platform based on their assessments of the local market's needs and technical savvy. While these corporations are becoming more in touch with local needs, reflected in increasingly appropriate design of these platforms, we believe there is much to be gained by incorporating local entrepreneurs into the design loop.

Empowering local entrepreneurial developers with a platform for easily launching innovative services and products has several advantages for major companies:

Entrepreneur-Informed Design We believe that providing local entrepreneurs with a monetary incentives for successful applications is an extremely effective method of understanding and meeting local market needs;

Rapid Prototyping With a "cloud"-like system that dramatically reduces application development and deployment times, many more products and services can be market-tested. This enables their commercial viability to be rapidly assessed and provides an opportunity to conduct many more design iterations and leading to more "killer" applications;

Learning Local, Applying Global Lessons that are learned by empowering entrepreneurs with the ability to easily deploy applications to local markets has a potentially large global impact. While it is important to allow the innovators to maintain ownership of their innovations within the local market, the lessons learned through this iterative design process may be applicable to other companies and other markets.

4. INITIAL METHODS

While the proposal above outlines an ambitious long-term agenda, there are specific aspects the research community can pursue in the short-term. One of the major, and significantly underutilized, tools that can be used for rapid application deployment across the developing world are universal protocols such as USSD. Additionally, until mobile banking and money transfer services become ubiquitous, it is possible to integrate other payment mechanisms into mobile services such as airtime-sharing protocols such as Me2U or even customized scratch cards.

USSD While USSD is almost unknown in the West, it is available on all GSM phones. Unlike SMS, USSD is a not a store-and-forward protocol, but rather is session-based (a difference similar to that between email and telnet). Typically, USSD is the method for prepaid subscribers to check their balance and top-up their account. Initiating a USSD session is typically done by entering the following numbers * *shortcode* # *command* # and pressing send. It could function as the basis for many new services.

Integrated Airtime Sharing While interactive mobile banking and financial transactions services such as MPESA have yet to be deployed, airtime sharing is available in many regions. These types of services can provide the foundation for an entirely new suite of applications that can leverage future mobile payment systems. One example of this is a water pump company in Nairobi: this company has sold water pumps in rural Kenya for many years, but they recently changed their business model away from hardware sales, and instead they sell water "vending machines." They now attach phones and solar panels to their water pumps, instead of paying for the pump with upfront capital, villagers get the pump for free and transfer small amounts of money (or airtime) to the pump in exchange for water.

Repurposing Scratch Cards In most of the developing world, mobile phone subscribers purchase scratch cards from local vendors to "top-up" their airtime credit. Because billions of people are comfortable with scratch cards, and there already is an infrastructure for selling scratch cards in even remote areas, there is an opportunity to repurpose scratch cards as a mobile payment mechanisms for other commodities. For example, in Rwanda, until recently individuals had to travel to the capital and wait in line at the national electricity company, ElectroGaz, to "top-up" their electricity accounts. This posed a dilemma: most people could not afford to outlay a significant amount of capital, but they did not have the time to go into town and wait in line every few days to keep their electricity account topped up such that their service is not automatically shut off. Jeff Gasna at SMSMedia in Kigali recognized that just as prepaid airtime can be sold by scratch-card dealers, so could prepaid electricity. Jeff partnered with ElectroGaz and printed his own electricity scratch-cards. Within a year of launching this service, over 30% of electricity users in Rwanda are now using their mobile phones to buy electricity using Jeff's system.

5. REFERENCES

[1] L. Peterson et al. A Blueprint for Introducing Disruptive Technology into the Internet. In *HotNets*, Princeton, NJ, October 2002.

[2] G. P. Zachary. Ping: Inside Nairobi, the Next Palo Alto. *New York Times*, July 2008.

CCC Workshop Position Paper

Gary Marsden
ICT4D Centre
University of Cape Town
South Africa
gaz@cs.uct.ac.za

BACKGROUND

I am a researcher based in the ICT4D Centre and the Computer Science Department at the University of Cape Town in South Africa. I move there from London (UK) ten years ago. My background is in HCI, specifically in mobile interaction design. My contribution is, therefore, mainly from an HCI perspective.

1. GRAND CHALLENGES

1.1 Getting out of the way

The guiding philosophy of our research group is how we get out of the way and let people in developing countries create their own solutions. We have worked for years conducting ethnographic studies trying to understand the needs of our users, but we inevitably end up with some degree of mis-interpretation. Over time then, we have shifted our focus away from building technologies to help end users and instead have started to focus on tools that people within a community can adapt to their own needs [1]. Working with a key member of a community[1], whom we call our 'human access point', we develop technologies that can be 'communitised' so that users can create their own solutions. Examples would include Frontline SMS [2] and BigBoard [3].

Whilst this methodology has much to recommend it, we arrive at two grand challenges:

(1) Does there exist a core set of communitisable tools that we should be working on?

(2) If we autonomously empower people with technology how do we educate them about the impact that technology may have? (Do we even know the answer to this within our own contexts?)

1.2 Literacy

HCI literature advocates the use of native language in an interface. For developed countries, this is self-evidently a good thing. However, for the developing world, it is less clear. For example, many people we work with want the interface to be in English (usually their second language) as they know they are more likely to get a job if they are trained in the English version of the software.

For those who do want to use an interface in their native language, then there are problems in simply doing a word-for-word translation of the interface. Some cultures we have worked within South Africa, for example, have no intrinsic notion of hierarchical classification. So, whether the names in a hierarchical menu are presented in English or in a mother tongue becomes irrelevant. So:

(3) How do we gain a deeper understanding of other forms of literacy (e.g. visual literacy) and their appropriateness, so that we can more effectively create interfaces for our target users.

1.3 Ethical Research

Realistically, most academic research is based around grant funding cycles and the duration of research student degrees. We have to be very careful when working with any community, therefore, in providing realistic expectations about what we can achieve and how that interaction will impact the community. In our case, we have teamed up with an NGO called bridges.org who assess our proposals and projects to see if they meet ethical standards and are sustainable in the long-term. Bridges also have a lot of experience in ICT projects in the developing world and provided us with checklists to measure the likely success of a project. It would be fascinating to explore how this community measures the wider impact of their projects and how they manage sustainability.

The corollary to this is the impact on the research student. Their goal is, not unreasonably, to gain their degree. However, by asking them to conduct their work in rural locations whilst being sensitive to a local culture with which they may not be familiar, obviously creates more work than a student based in a research lab. It would be helpful to discuss with others how they manage these constraints on their students.

(4) How do we improve the sustainability of projects without exploiting the good will of our students or target community.

2. APPROPRIATE RESEARCH METHODS

2.1 Rapid Prototyping / Participatory Design

One of the most favoured practices in HCI currently is that of Participatory Design [4], wherein the end user of the system becomes an active part of the design team. Whilst ethically seductive, there are a number of problems in realising this in the field.

For anyone who has used computers over a period of time, it is abundantly obvious that software is highly mutable. Even if the user is not familiar with software development, as they install patches and updates, it becomes clear that software is changeable. Therefore, even none experts can take part in ICT participatory design sessions, as they have some idea of how software can be altered. In our experience, this is not true in the developing world.

[1] Typically this is a person who has had some exposure to ICTs, or it may even be an NGO who can act as a liaison.

The users that we work with have often had no exposure to any form of computing technology. To them, technology is from a foreign place and cannot be altered. This makes running participatory design sessions next to impossible. Even using the medium of paper, as advocated in rapid prototyping [5], is problematic as not all users can make the connection between the paper and the design of the software. Another problem we have encountered is with testing high-fidelity prototypes. We often have to conduct extensive training with the users (say six months) before they can use the software effectively. Once the training is complete and the usability problems identified, the users are not keen to have the software altered – the do not want to loose the six months invested in training.

Participatory design and rapid prototyping are two highly effective tools in interface design. My suspicion is that if we were to alter the way in which these methods were used, then it would be possible to improve their effectiveness in developing world contexts. In our case, we can overcome this through using a 'human access point'. Other possibilities might be in using live action role playing, or presenting designs as theatre.

2.2 Technology Probes

One method we have been exploring recently is that of technology probes [6]. These are used by people like Bill Gaver who produces technology outside the frame of reference even for people living in the developed world (e.g. tablecloths that glow around items that have been placed on them). He builds these artifacts not as ends in themselves, but to explore their impact and appropriation.

Our work in the developing world requires us to create technologies for people who have no frame of reference for how they might be used or what they might be for. We have therefore recently adopted a technology probe approach and now build high-fidelity prototypes of systems and deploy them, not just to evaluate their performance, but also to help predict the likely impact on our users and to give them an insight into what we are proposing.

3. EVALUATION METRICS

We have found that the techniques for evaluation from HCI often fall flat with our users: Questionnaires cannot be used as they require too much literacy; Users in focus groups tend to defer to the person with the strongest voice and there is a strong Halo effect [7] whereby users try to guess what the researcher wishes to hear.

3.1 Journalists

To overcome this, we have adopted another technique from Gaver's work. In many of his projects, he uses journalists to interview the users after the technology has been deployed. The idea is that the journalists are trained in asking questions and the users are more likely to give honest answers, realising the journalists are an independent third party. We have adapted this approach by using journalists from the same language group as the users we have been working with. We have only done this once so far, but the results have been very encouraging.

3.2 NGOs

In other situations where we work with NGOs, we have adapted the success criteria for those organizations to become our success criteria. The funding success of NGOs often depends on them showing that they are having an effect in their given domain. We have discovered that most NGOs have therefore developed instruments to measure that impact. Using those same instruments we are able to measure the impact our technology might have within that domain.

4. STUDENT JOB PROSPECTS

Being based in the developing world, our students have a direct way in which they can exercise their ICT4D skills – there are companies here in South Africa selling technology into emerging markets.

For students who go to developed countries, we encourage them to take the tack that if they can build technologies for this user group, then they can build technology for any user. On top of this, there is a strong argument that having worked within the design constraints imposed by these countries, new solutions are created which are relevant in the developed world (similar to the arguments around universal usability).

5. BUILDING THE FIELD

Being based in a developing country may give me a slightly different perspective on what is meant by building the field. A critical problem for us is finding enough students from within Africa to work on these problems. Following Papanek's maxim of "teaching the teachers" [8] we would like to explore how we educate ICT lecturers in the developing world and perhaps create an ICT curriculum for the developing world (e.g. focusing more on mobile computing).

Another avenue to explore is the overlap between the current push in 'Green' sustainable design and the fact that ICT4D solutions often require a smaller technology footprint than those coming from developed countries. This is only an insight currently and we have not yet explored this approach to design.

6. REFERENCES

[1] Marsden, G., Maunder, A. and Parker, M. (2008) "People are People, but Technology is not Technology" Philosophical Transactions of the Royal Society A, October 2008, pp 1-10.

[2] Ken Banks, www.frontlinesms.com

[3] Maunder, A., Marsden, G. & Harper, R. (2007) "Bluetooth interaction with situated displays" Proceedings Mobile HCI 2007. Singapore. ACM Press pp 188-191.

[4] Muller, M. J. and Kuhn, S. 1993. Participatory design. Commun. ACM 36, 6 (Jun. 1993), 24-28. DOI= http://doi.acm.org/10.1145/153571.255960

[5] Scott Jensen, (2002) "Simplicity Shift", Cambridge Press

[6] William Gaver , Anthony Dunne, Projected realities: conceptual design for cultural effect, Proceedings of the SIGCHI conference on Human factors in computing systems: the CHI is the limit, p.600-607, May 15-20, 1999, Pittsburgh, Pennsylvania, United States. [doi>10.1145/302979.303168]

[7] Matt Jones, Gary Marsden, Mobile Interaction Design, John Wiley and Sons, 2006.

[8] Victor Papanek, (1984) "Design for the Real World"

Computing as if People Matter*

Amit A. Nanavati
IBM India Research Lab
New Delhi, India
namit@in.ibm.com

ABSTRACT

Traditional computing has focussed on modelling and optimising computing resources. How do we design ICTD systems where we optimise on human cognitive resources? What sort of constraints and optimality criteria should algorithms focus on so that the output is more efficiently consumed by human beings? How do we evaluate systems in their success with human interaction? Can novel formal models of computation and interaction help?

1. INTRODUCTION

Time and again, a new subfield is born in Computer Science. Often it starts with interesting specific solutions, and over time, it shapes and develops new methods and techniques unique to it. The last century witnessed the evolution of many subfields: parallel computing, distributed computing, databases, to name just a few. In the last few years, there has been a lot of activity in the area of Information and Communication Technologies for Development (ICTD).

ICTD research can be broadly be classified into two categories (a) those that are related to robust infrastructures [1] and networks [2] in remote and challenging environments, and (b) HCI-oriented research for a unique population with special needs. In the former case, since the assumptions are relatively consistent across geographies (say, low Internet penetration in Africa and India), the solutions are generally applicable. In the latter, cultural and behavioural variations in human populations are considerable, so the specific solutions are not always generalisable.

2. GRAND CHALLENGES

One of the strengths of computer science lies in constructing models of computation and interaction: Turing machines, Finite state machines, Calculus of Communicating Systems,

*With gratitude and apologies to E.F. Schumacher, author of "Small is Beautiful: A Study of Economics as if People Mattered."

CRA-CCC 2009 UC Berkeley, California, USA

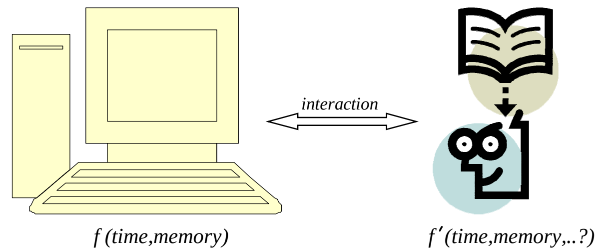


Figure 1: So far, computer models have been interested in optimising the time and memory of the computer. Now, we need to extend the model to account for the *cognitive demand* on the user.

Communicating Sequential Processes, Petri Nets, Input/Output Automata. Such models have proved to be useful for multiple reasons: (a) they define "computation" and typically illustrate the boundaries of what is computable in the specific context, (b) separate the difficult from the easy and (c) implicitly provide mechanisms for evaluation.

In the past, the focus has been on computational resources, their measurement and optimisation. From an ICTD perspective, the human is the focus. How do we design interaction systems such that the *cognitive demand* from the user is minimal. The cognitive demand is a representation of a human's resources: the time, memory and attentiveness required of the user [4]. Just as space and time constitute fundamental measures of computational complexity for a machine, we will have to isolate factors affecting the cognitive demand of a user. This is extremely difficult to do, since the factors are numerous. We are at a juncture where the various successful point solutions can provide us with enough insights and experience in order to make a modest beginning.

Even as we explore novel models of human-oriented computation and interaction, we could start by modelling a human as an automaton and the interaction as a distributed system [4]. The interaction between a system and a user can be modelled as a formal language, a set of strings on the event/action alphabet. (many paths) for accomplishing the task (reaching the goal state). The length of the various paths could be different, and the shortest path need not be the most easy one from a user's perspective. How does one characterise "ease" or define the cognitive complexity of the task? Is it the one which requires the least number of inputs from the user? Or the one which requires more interactions but are easier for the user to answer?

Grand Challenge 1: *Define a model of computation (and/or interaction) which includes and makes precise the notion of "cognitive demand" on the user. As a result, it should be possible to define the cognitive complexity of any application (Figure 1).*

Such considerations can extend to algorithms as well. For concreteness, let us consider Folksomaps [5]. To go from one point to another, people in India prefer landmark-based routes rather than the usual map based east-west-north-south directions (for various reasons). The paper proposes a hierarchical approach (country, city, district) for path finding based on various symmetric but not quite transitive relationships ("nearTo","connectedTo","sameDistrict"). Given a graph with accurate data in terms of the relationships mentioned, computing the shortest paths is not obvious. Here, we have an example of a traditional problem (shortest path) to be solved under atypical assumptions (the relationships are not transitive). What sort of constraints and guarantees are possible in this setting? Instead, one might be interested in paths that are easy to remember, or landmarks easy to find (new problem), rather than shortest paths. The cultural preferences of a user may change the setting, constraints and optimality criteria of an algorithm.

Grand Challenge 2: *Find a class of graph-based (for now) problems which introduce different user-centric optimality criteria for algorithm design. In cases where deterministic optimal algorithms are not found, we may discover that approximation algorithms with different kinds of guarantees have to be invented.*

The first two grand challenges aim to introduce the formal aspects of computing science into ICTD research. Such an approach complements the empirical approach that is more often followed. How does one define reasonable evaluation metrics that combine these aspects?

For example: In order to evaluate dialogue call-flows on resource constrained mobile devices [3], the authors define an $\langle m, q, a\rangle$ measure, where m is the memory used by the application, a is the accuracy of the speech recognition, and q is the number of interactions expected from the user. The $\langle m, q, a\rangle$ measure incorporates the hard device constraints as well as one soft user constraint.

Grand Challenge 3: *Define evaluation measures that incorporate both hard and soft constraints for particular ICTD systems.*

3. THE ICTD ECOSYSTEM

ICTD researchers (both academic and industrial) typically tend to collaborate with "on-the-field" organisations (both government and non-government) to work with their target populations 2. These "on-the-field" organisations typically have a deep understanding of the needs of the target population. There are some facilitator organisations, such as the United Nations Solution Exchange in India [6] who provide a common meeting ground for the researchers and the organisations. The IEEE has also launched several initiatives in the area of Humanitarian Technologies [7]. Such facilitator organisations can play a pivotal role in the systematisation of the collaboration. The funding sources are various and independent. Most often, the organisations have their own separate sources of funding, as do the ICTD researchers.

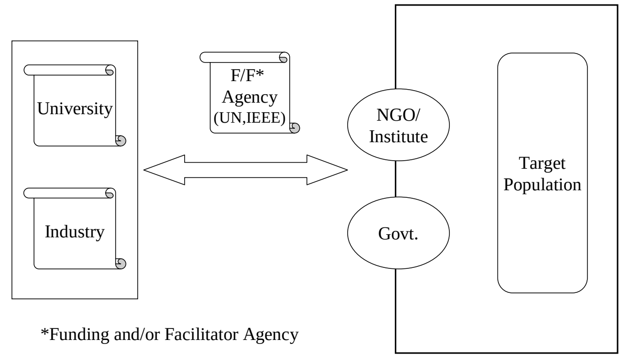


Figure 2: The various players in the ICTD ecosystem: The researchers, academic or industrial, typically work with non-government organisations or government organisations in order to connect with the targets. Organisations such as the UN and IEEE can play the role of facilitators in order to connect the researchers with the (N)GOs.

It might perhaps be worthwhile to consider the possibility of funding organisations that fund projects (which means both parties), and the outcome of such projects are made available for reuse. What kind of organisation would maintain such a repository? Why? Can we create a different kind of stakeholder for this?

4. ACKNOWLEDGEMENTS

The author is grateful to Sheetal K. Agarwal, Dipanjan Chakraborty, Anupam Jain, Arun Kumar, Priyanka Manwani, Nitendra Rajput and Aaditeshwar Seth of IBM India Research Lab for many an enriching conversation about ICTD. He has greatly benefitted from innumerable chats with Nitendra Rajput on formal method based approaches for speech interactions. He would also like to thank Richard Gowen(IEEE Foundation), Neil Patel(Stanford) and Rajen Varada(UNSE) for several insightful discussions.

5. REFERENCES

[1] E.Brewer,M. Demmer, M. Ho, R.J. Honicky, J. Pal, M. Plauche, S. Surana,"The Challenges of Technology Research for Developing Regions", IEEE Pervasive Computing, 2006.

[2] http://blizzard.cs.uwaterloo.ca/tetherless/index.php/Main_Page.

[3] A.A. Nanavati & N. Rajput,"Characterising Dialogue Call-Flows for Pervasive Environments", InterSpeech 2005, Lisbon, Portugal.

[4] A.A. Nanavati & N. Rajput,"A Theoretical Model for Adaptive Contextual Conversational Systems", In prep.

[5] A. Kumar, D. Chakraborty, H. Chauhan, S.K. Agarwal & N. Rajput,"FOLKSOMAPS - Towards Community Driven Intelligent Maps for Developing Regions", ICTD 2009, Doha, Qatar.

[6] http://www.solutionexchange-un.net.in/

[7] http://ieeehtn.org/

The Hesperian Digital Commons: A Multilingual Primary Health Resource

Srini Narayanan (snarayan@icsi.berkeley.edu)
Matt Gedigian (gedigian@icsi.berkeley.edu)
ICSI and UC Berkeley

Motivation

There are considerable inequalities in health, particularly between rich and poor. These inequalities stem from income disparities and from lack of access to health services, clean water, sanitation and education. We believe that health is the fundamental right of all people. There is thus a moral imperative to address this social and economic injustice and we believe that information technologies can potentially play a role.

Currently, the wealth of digitized information is not available to most of the world's population, especially to speakers of the over 5,500 languages of developing regions, where access to basic healthcare, education, and training could be greatly enhanced by these technologies. In recent years, there has been some effort, both indigenous (http://www.tenet.res.in) and international (http://tier.cs.berkeley.edu/wiki/Home), with the goal of establishing computing and communication infrastructure the developing world. These efforts have focused primarily on hardware solutions and there is now a minimal hardware infrastructure connecting rural regions in some developing countries (such as India). However the development of local language content has lagged behind these advances and consequently little use is being made of the existing frameworks to enable relevant information access to the millions of potential users.

Expanding the reach of preventative healthcare information using new technology has been an area of much recent interest in academia and international health development practice alike (Braa 2004). The public health community has been particularly concerned about the development of reliable, relevant, usable information that can be easily accessed across the world (Godlee 2004). The need is particularly acute in the developing world where access to doctors is often limited and large regions are almost entirely serviced by field health workers.

In many parts of the world, community field workers have limited access to information that is reviewed by professionals. The Hesperian Foundation (http://www.hesperian.org) is a non-profit publisher of books and educational materials that help people in developing regions take the lead in their own health care and organize to improve health conditions in their communities. Simply written, heavily illustrated, and developed in collaboration with groups around the world, Hesperian Books, such as "Where there is no doctor" (WTIND), contain a wealth of life-saving information on diagnosing and treating a broad range of health problems. Where there is no doctor is a community handbook available in 100 languages and used by rural health workers in over a 120 countries. Hesperian materials are used to train health workers in violence-torn areas of Colombia, create community-based care for refugees in Thailand, provide support to children affected by HIV/AIDS in Africa, combat toxic poisoning from mining in the Philippines and support a host of other public health needs across the globe, at the community level.

Goal

The Hesperian Digital Commons project (HDC) is an ongoing initiative to offer an open, multilingual, accessible web portal to Hesperian content. The project aims to use information technologies to improve the timeliness of the content, facilitate translation, and reduce the marginal cost of distribution.

Basic Design

The HDC is, first and foremost, a public website which health workers can search, read, and explore. It provides access to the content found in Hesperian books in a web format. In addition to the text and illustrations from the books, the site allows multimedia content such as photographs, audio, and video. Web content can be updated with the latest health information (e.g. changing HIV/AIDS treatments), without being tied to the publication cycle of the printed books. On the web, references to other content (e.g. "see page 35") are replaced with hyperlinks to facilitate navigation.

Wiki

Hesperian partners with independent groups around the world who translate and localize their books. Traditionally, these field partners used a variety of software, such as Microsoft Word, Adobe Pagemaker, and Adobe InDesign. The HDLP uses a wiki format to provide collaborative editing, so field partners can translate and update information directly on the site. Every article has a "talk" page where contributors can discuss proposed changes. In the same manner as Wikipedia, content which appears in multiple languages can be linked together.

Semantic Wiki

Although wikis are well-suited for the creation of content, they provide limited search and navigation facilities. The HDLP is a *semantic wiki*, meaning that the content structured around an ontology. Each article on the wiki corresponds to an entity in the ontology. These wiki pages contain, in addition to the displayed content, formal statements about the properties of the underlying entity. With this metadata, we can formulate queries such as:

- List diseases which have stomach ache as a symptom
- List diseases which can be treated with either Streptomycin or Penicillin
- List diseases which cause fever over 40º C

In constructing the HDC, we formulated an initial ontology which specifies the main concepts from Hesperian's books. This ontology spans a number of Hesperian books and is intended to capture the specific viewpoints and contextual information that Hesperian editors wish to disseminate with their materials. Specifically, the ontology intends to capture the social, political, environmental, and community aspects of health care as a context which situates individual conditions, actions, treatments, and lifestyle. Here are the basic concepts that will be annotated in the Hesperian materials that cover health organization and promotion, aspects of the environment, and health practices including local practices and remedies in addition to individual health conditions.

The semantic structure can simplify the task of meeting immediate and specific health needs in a particular region, i.e., in response to a flood, a field partner can quickly create pamphlets (in the local language) based on queries of health issues related to water-borne diseases. These queries can be specified interactively or embedded within articles.

Annotation

Fundamental to semantic queries are the notion of annotations. Annotations specify the semantic properties of a concept on the wiki. For instance, the concept Health Condition has a property Symptom which in specific cases would be the set of symptoms (fever, headache) for a specific Health Condition (flu). The annotation [[Symptom::Fever]] when specified in the wiki page Flu then denotes that Fever is a symptom of Flu. A property search for diseases that have the symptom Fever then is able to return the page Flu as one of the answers. An annotation for a concept thus corresponds to identifying the value of a property of the concept in the wiki. Such a value may be a piece of text, a number, another wiki page, or even a link to some external resource on the web. Annotations are added to wiki pages just like other content, using a slightly modified wiki syntax. The server translates these internally to a standard form (RDF/OWL), making them available to programs that can manipulate RDF data.

Low Bandwidth and Wireless Access

Many of Hesperian's users are likely to have sporadic, low bandwidth (possibly only cell phone) access to the internet. The appropriate solution will depend on the situations with the users and careful assessment needs to be done to identify an optimal strategy. For the initial phase, we plan to target organizations and users who have reasonable network access and help them create appropriate adaptations for printing, mobile phone, or other formats that can serve the specific needs of their community.

While in the next few years, most areas of the world will have some connectivity, it is also likely that in the foreseeable future, the connections will be low bandwidth, cell phone based, and the carrying capacity of the local ISP providers will be limited making for intolerably long delays during high load hours. There are several techniques being developed (some within the Berkeley TIER project) to use smart scheduling (schedule file I/O during low peaks), low power and robust wireless loopback technology, and speech and cell phone based interaction to address these issues. Particularly interesting is the work done by the CATER group at NYU (http://cater.cs.nyu.edu/wiki/index.php/Rural_Cafe). We are in close contact with this exciting research and wherever possible, it is our intention to utilize the results of these efforts.

References

Braa, J., Monteiro, E., Sahay, S. 2004. "Networks of Action: Sustainable Health Information Systems Across Developing Countries" Management Information Systems Wuarterly 28:3 pp 337-362

Godlee, F. N Pakenham-Walsh, D Ncayiyana, B Cohen, A Packer. 2004, Can we achieve health information for all by 2015? The Lancet 364:9430 pp 295-300.

First-Class Access For Developing-World Environments

Vivek S. Pai
Department of Computer Science
Princeton University
vivek@cs.princeton.edu

Sunghwan Ihm
Department of Computer Science
Princeton University
sihm@cs.princeton.edu

Anirudh Badam
Department of Computer Science
Princeton University
abadam@cs.princeton.edu

KyoungSoo Park
Department of Computer Science
University of Pittsburgh
kyoungsoo@cs.pitt.edu

1. OVERVIEW

While the gap between the "haves" and "have-nots" in Internet access is wide, the gap between the "haves" and the "almost-haves" may not be much better. As the first world moves toward user-generated content, social networking, blogs, comment-driven sites, and more participation, having anything less than full access to the Internet will degrade the Internet user experience. While many people believe that read-only offline access to the Internet is "good enough", we believe that this approach will hinder developing-world users from sharing information not only with the developed world, but also with each other, which appears to be a largely unaddressed desire [10]. In the longer term, we also believe that offline-only access will fail to spur the kind of Internet growth seen in the first world.

While being a second-class Internet citizen is no doubt better than being excluded completely, a number of technological advances may soon render the choice between first-class and second-class a false dichotomy. Low-cost laptops can bring personal computing to large numbers of people [4, 5]. Long-range wireless can bring connectivity where no connectivity existed [8]. Large-capacity low-cost disks can provide bulk storage that transforms how developers think of data retention. Solid-state disks can boost the performance of out-of-core applications. Current low-cost laptops combined with USB-attached hard drives can provide this level of hardware for $400 USD per unit, and this cost may drop over time.

Our focus, at a high level, is to use these technologies to narrow the gap between the usage experiences of the developed world and the developing world. This combination will likely mean that our focus will be on the growing urban middle class and the upper-middle class in particular, but even this target audience is sizable – if we assume that one-quarter of India's and China's population falls into this category, the number of users exceeds the total population of the United States. We target this audience based on the observation that even if $400 USD is a large value in local currency, many middle-class parents in these countries view it as an investment in their children's education. If such technology can provide a usage experience similar to that of the developed world, it also provides self-empowerment rather than charity, with a family laptop seen as an aspirational item, akin to a television, scooter, or car.

We also view targeting the urban middle class as a means of helping build local ecosystems. Online access can more easily drive advertising and advertising-based purchases, both of which subsidize the cost of developing and delivering content. As more people use the online Internet, the fixed costs of traffic delivery are spread across more users, lowering the cost of delivery, which can then generate more demand from more users. Commercialization of the Internet in the US has generated so much volume that the researchers who originally used the Internet can now buy bandwidth and access much more cheaply than prior to commercialization. We hope that lowering the cost of online access in the developing world can generate a similar effect

2. A DEVELOPING WORLD STACK

To achieve these goals in a way that best exploits our backgrounds, we intend to focus our efforts on a networking software stack tailored toward developing-world usage. The main goals of this stack will be focused on improving the perceived bandwidth and latency of Web applications by localizing activity as much as possible, and moving activity to where it can be most efficiently served. The components of our network stack include: a static Web cache, a WAN accelerator, bandwidth shifting, prefetching, snooping, and off-line access.

At the heart of our networking stack is a caching storage system called HashCache [1], which enables terabyte-sized caches to be shared among applications, while providing selectable trade-offs between RAM consumption and performance. In its lowest-performance mode, HashCache requires no main-memory indexing. From a developer's standpoint, HashCache coupled with large disks provides a practically-infinite cache store at low cost. Assuming connectivity of one megabit per second, a one-terabyte disk is sufficient to store all communication for three months. A low-overhead cache system combined with this kind of storage capacity frees the developer from having to wonder if some data should be stored, or whether prefetching will pollute the cache – if the least-recently used data on disk is three months old, eviction is not an issue. The HashCache Web Proxy provides a standards-compliant cache for static content.

On top of HashCache, we layer a WAN accelerator (network packet cache) designed for disks with large capacities but low seek rates. By using HashCache's indexing, it can operate with a very low memory footprint, and the two systems can comfortably share the hardware of a low-end laptop (256MB RAM) with a USB-attached 1TB drive. Commercial WAN accelerators often advertise the fact that they do not store redundant content as a feature, since all content must be indexed (presumably in RAM). In contrast, with HashCache eliminating the RAM pressure from indexing, storing data redundantly on disk can reduce the number of seeks needed. Our WAN accelerator also provides a peering protocol, allowing content to be fetched from closer peers when possible.

For higher-performance environments, HashCache can use a different indexing scheme, which requires a larger RAM footprint. We intend to use SSDs in these environments, since current low-cost laptops use SSDs that offer performance and capacity between that of their RAM and external hard drives. For these environments, a low-end laptop (256MB RAM) using all of its SSD (typically 4GB) for indexing can provide performance comparable to larger servers.

Bandwidth-shifting is the term we use to describe moving bandwidth consumption from a high-cost location to a low-cost location. While Web caches are single-sided (i.e., the client knows when content is cacheable and can avoid contacting the server), WAN accelerators operate in pairs, with one end fetching the content from the server. Bandwidth in the developing world is often more expensive, even in absolute terms, than in high-bandwidth countries. We intend to exploit this difference by providing WAN acceleration endpoints in lower-cost regions – the WAN endpoint fetching from the server is located in the low-cost region, so most content is fetched at lower cost. Only the compressed data between the WAN accelerators enters the developing region. We intend to use our background in developing content distribution networks [7, 12] to address the geolocation and peer selection mechanisms needed to determine which low-cost region should be used when fetching content. We have some experience understanding the interaction between CDNs and localized content [6, 9], and intend to use this to minimize the possible disruption in choosing off-continent servers to fetch content.

While bandwidth-shifting moves bandwidth consumption in space, prefetching moves bandwidth consumption in time. The combination of a Web cache and a WAN accelerator also means that most Web content has some potential utility, either as a fully cacheable page, or as fragments of a page that can populate the WAN accelerator. Off-peak demand, especially for schools, can be near zero, and presents an opportunity to pre-load content for the next day. Traditional concerns regarding prefetching, such as self-interference on the network, are mitigated during off-peak hours. Even the question of utility of prefetched content becomes less important, since having several months of storage capacity makes it unlikely that a prefetch will evict anything recently-used. Even simple prefetching approaches, such as crawling news sites every morning, are likely to have a benefit in shifting bandwidth demand. More complicated approaches, such as analyzing the previous day's traffic logs, are also possible.

Snooping is an extension to prefetching, and involves using broadcast channels to populate peer caches. Users within wireless range of each other may opt to disable encryption to make their traffic cacheable to other users. Likewise, if multiple schools share the same satellite infrastructure, they may opt to let others populate their caches using the broadcast traffic already consuming bandwidth. This idea was used for static caches by the now-defunct commercial service Edgix many years ago, but was done without WAN acceleration, so it only benefited static content.

Transparent off-line access similarly builds around caching and WAN acceleration – when external connectivity is lost, the local Web cache can satisfy requests, but by itself, cannot provide the illusion of full connectivity. However, when combined with the WAN accelerator, it can be used to store multiple versions of dynamic content, such as keeping track of what page was last served to each user. Since WAN accelerators can identify the same content in multiple responses, it forms the basis of a deduplication system – for a dynamic page, one copy of the common content is stored, as well as one content of each set of per-user differences. For pages that are dynamically-generated but contain no indication of per-user customization, we may opt to provide them when they are not available. This approach has been used for several years by the Coral CDN [3] to offload flash crowds, reducing concerns about private information leakage.

Offline access can also be augmented by adding local search support, so that cached content can be searched when online search is unavailable or slow. Our intended model is a blend of Tek [11] and RuralCafe [2] – existing search results are presented when available, but if not, a local search is performed using an embedded search engine. This search engine does not have to perform as well as commercial search services to be useful, since the goal is to still have some content availability during disconnection, even if the specific ranking and presentation is not as polished.

3. REFERENCES

[1] A. Badam, K. Park, V. Pai, and L. Peterson. Hashcache: Cache storage for the next billion. In *Proceedings of NSDI'09*, 2009.

[2] J. Chen, L. Subramanian, and J. Li. RuralCafe: Enhancing web search in intermittent networks. In *Proceedings of WWW'09*, 2009.

[3] M. J. Freedman, E. Freudenthal, and D. Mazieres. Democratizing content publication with coral. In *Proceedings of NSDI'04*, 2004.

[4] Intel. Classmate PC, http://www.classmatepc.com/.

[5] One Laptop Per Child. `http://www.laptop.org/`.

[6] K. Park, V. Pai, L. Peterson, and Z. Wang. CoDNS: Improving dns performance and reliability via cooperative lookups. In *Proceedings of OSDI'04*, 2004.

[7] K. Park and V. S. Pai. Scale and performance in the CoBlitz large-file distribution service. In *Proceedings of NSDI'06*, 2006.

[8] R. Patra, S. Nedevschi, S. Surana, A. Sheth, L. Subramanian, and E. Brewer. Wildnet: Design and implementation of high performance wifi based long distance networks. In *Proceedings of NSDI'07*, 2007.

[9] L. Poole and V. Pai. ConfiDNS: leveraging scale and history to detect compromise. In *Proceedings of the USENIX ATC'08*, 2008.

[10] S. R. Sterling, J. W. O'Brien, and J. K. Bennett. Advancement through interactive radio. In *Proceedings of ICTD 2007*, 2007.

[11] W. Thies, J. Prevost, T. Mahtab, G. T. Cuevas, S. Shakhshir, A. Artola, B. D. Vo, Y. Litvak, S. Chan, S. Henderson, M. Halsey, L. Levison, and S. Amarasinghe. Searching the world wide web in low-connectivity communities. In *Proceedings of WWW'02*, 2002.

[12] L. Wang, K. Park, R. Pang, V. Pai, and L. Peterson. Reliability and security in the CoDeeN content distribution network. In *Proceedings of the USENIX ATC'04*, 2004.

Computer Science and Global Economic Development: Sounds Interesting, but is it Computer Science?

Tapan S. Parikh
UC Berkeley School of Information
Berkeley, CA, USA
parikh@berkeley.edu

OVERVIEW

Computer scientists have a long history of developing tools useful for advancing knowledge and practice in other disciplines. More than fifty years ago, Grace Hopper said the role of computers was "freeing mathematicians to do mathematics." [5] Fred Brooks referred to a computer scientist as a *toolsmith*, , making "things that do not themselves satisfy human needs, but which others use in making things that enrich human living." [4]. Computational biologists have applied algorithmic techniques to process and understand the deluge of data made possible by recent advances in molecular biology.

The proper way to approach this kind of research has never been clear within Computer Science. The refrain "Sounds interesting, but is it Computer Science?" is frequently heard. In this paper I argue that it is crucial take an expansive view of what Computer Science is. To do otherwise is to cede a great deal of exciting, high-impact research territory to others - research that is technically interesting and has the potential to impact many lives. I believe the tension felt by our community in conducting "technical research" for global economic development provides a critical opportunity to re-examine our conceptions of our discipline.

CS AND GLOBAL DEVELOPMENT

My students and I are focused on solving development problems through the innovative use of computing technologies. What gets us out of bed in the morning is the opportunity to have impact on individuals and communities living in poverty in the developing world, and on the institutions that serve them. This is not to say we are uninterested in technical problems — in fact, that is what we bring to the table — the ability to design, implement and deploy computing systems within constraints not often felt in other contexts — including limited user literacy, physical infrastructure and organizational capacity. However, for us, solving technical problems is a means to an end, not an end in itself. If we can solve the latter without contributing anything hugely novel to the former we would do (and have done) so.

Information and communication technologies (or, ICTs) carry great promise for development practitioners and researchers alike. Governments, NGOs and businesses see them as tools to communicate with target communities and their staff, to document and learn from their own interventions, and from those working in the same region or on similar issues. Recently, development economists have recognized the paucity of applicable theory, turning their focus on designing, identifying, and evaluating the impact of new interventions from the bottom up, usually applying experimental methods [1]. This includes prominent use of ICTs, both as the focus of new interventions (mobile phones for making markets more efficient [6], digital cameras to monitor teacher attendance [2]), and as tools for understanding their impact (PDAs and smartphones to conduct extensive in-field surveys [8]). It is a wonderfully timely moment for computer scientists to engage with the state-of-the-art in development research and practice.

WHY ACADEMIA? WHY CS?

Why do this work in academia, and within the discipline of Computer Science? There are several motivations. Academia allows us to be more free, and take greater risks, than other institutions. A commercial approach must (eventually, at least) be accountable to the tyranny of revenue and profits. A government approach is constrained by bureaucracy, and by an inability to take risks, including learning from (often, small-scale) experiments. In academia we are able to experiment with new ideas, without being guaranteed of their popularity or success, and without knowing a priori the long-term sustainability model.

The most compelling alternative is the non-profit sector — non-governmental organizations, or NGOs. NGOs probably have the best record of ICT innovation in support of rural economic development. The fact that NGOs have outdistanced academia in an area with such potential for important technological innovation, with just a fraction of the financial and especially human capital, stands as much as an indictment of our record, as a justification of theirs. However, there are important limitations to their approach as well. NGOs are usually not rewarded for nor capable of methodological and empirical rigor. It is important to develop tested theory and methods to inform technology design and implementation, and that allow us to generalize to new applications and operating contexts.

Why not do this work within other disciplines — such as Public Health, Economics or Education? After all, aren't we solving their problems? Simply put — researchers in these fields are most comfortable working with existing technologies, and not designing and implementing new ones. They have neither the skills nor the training to pursue that agenda, which could lead to entirely new opportunities and innovations. Moreover, CS students can learn a lot from doing this work, and are demanding opportunities to do so. Freedom, innovation, scientific rigor and opportunities for students — sounds like a good fit for academia!

BUT, IS IT COMPUTER SCIENCE?

It is time to return to that refrain "Sounds interesting, but is it Computer Science?" The ontological (what objects and phenomena we study) and epistemological (how we study them) bases of the field have been the subject of recent reflection. Ammon Eden distinguishes three paradigms of computer science [3]. Of fundamental importance is the phenomenon to be studied. The mathematics branch holds that the objects to be studied are algorithms — abstract mathematical objects, properties of which can be determined through deductive reasoning. The engineering branch holds that what we study are running computing programs, which are more complex, such that their properties can only be determined by seeing how they perform after the fact.

The final branch is the scientific one, best captured by Allen Newell and Herbert Simon — "Computer science is the study of the phenomena surrounding computers... an empirical discipline... an experimental science... like astronomy, economics, and geology." [7] These phenomena are not limited to algorithms, or even running programs (though they traditionally have been within mainstream CS), but can include the human, social and physical processes surrounding them. This interpretation is well-established within Human-Computer Interaction (HCI). By accepting it across Computer Science, especially as other areas are becoming increasingly user and application-driven, we bring a consistent philosophical basis to the field, including for engaging with other disciplines. In this paradigm, we advance hypotheses (often in the form of new computing technologies or applications, and/or their variants), and validate them using experimental methods. Note that it is not only the novelty of the technology that is relevant, but the importance and generalizability of the knowledge derived about them. CS research that advances the goals of global development, conducted in a scientific manner, for understanding the appropriate design, implementation, cost, impact, and usage of new computing technologies, clearly fits the bill.

One possible complaint is that if we stray too far from the *algorithm*, the theory will be insufficient for explaining the phenomena we study. I find this critique unsatisfying, and in fact already a fait accompli. Is there only one set of theories or concepts that underlies economics, biology or sociology? Naturally, the study of complex systems requires drawing upon multiple strands of theory, and drawing from other disciplines. The proper study of computing can (and does) draw upon theory from economics, sociology, psychology, cognitive science, neuroscience, and other areas. We should embrace this complexity, and learn from other disciplines, or doom ourselves to decreasing relevance by ignoring the panoply of interesting computing phenomena surrounding us.

BROADER RAMIFICATIONS

New Research Problems

We can create opportunities to attack a number of important, high-impact research problems for which we have the necessary expertise and tools.

New Publishing Models

Doing good science requires conducting studies that can take years to plan, implement and analyze. This is suitable for publishing in journal format, as opposed to the conference format common in Computer Science. By publishing more journal articles, we can improve our academic standing (for example, in tenure cases). By addressing problems important to other domains, we can publish articles in their conferences and journals also.

New Job Prospects

We can improve the job prospects of our students, both within academia and outside of it. For example, economics PhDs work in economics departments, business schools, schools of public policy, for NGOs, multi-lateral agencies, or governments; depending on their interests and the kind of dissertation they have published.

New Funding Opportunities

We can approach new funding opportunities, in collaboration with those disciplines that have experience with them. Computer scientists increasingly collaborate with medical researchers to apply for funding programs sponsored by NIH.

CONCLUSION

All these outcomes require a deep engagement with problems considered outside the realm of traditional Computer Science. It is pointless to go halfway — our new community must commit to the problems we are addressing, instead of trying to "squeeze" technical nuggets out of them. In this paper, I have argued that this shift is completely possible, by choosing a broader ontological and epistemological basis appropriate for the range of problems that Computer Science is now addressing. By taking this expansive view, we will not only contribute to other disciplines, but also to our understanding of computing, and to its advancement as a mature professional discipline.

REFERENCES

[1] A. V. Banerjee and E. Duflo. The Experimental Approach to Development Economics. *SSRN eLibrary*, 2008.

[2] E. Duflo, R. Hanna, and S. Ryan. Monitoring Works: Getting Teachers to Come to School. *SSRN eLibrary*, 2008.

[3] A. H. Eden. Three Paradigms of Computer Science. *Minds Mach.*, 17(2):135–167, 2007.

[4] J. Frederick P. Brooks. The computer scientist as toolsmith II. *Commun. ACM*, 39(3):61–68, 1996.

[5] G. M. Hopper. The education of a computer. In *ACM '52: Proceedings of the 1952 ACM national meeting (Pittsburgh)*, pages 243–249, New York, NY, USA, 1952. ACM.

[6] R. Jensen. The Digital Provide: Information (Technology), Market Performance, and Welfare in the South Indian Fisheries Sector. *Quarterly Journal of Economics*, 122(3):879–924, 2007.

[7] A. Newell and H. A. Simon. Computer science as empirical inquiry: symbols and search. *Commun. ACM*, 19(3):113–126, 1976.

[8] K. Shirima et al. The use of personal digital assistants for data entry at the point of collection in a large household survey in southern Tanzania. *Emerging Themes in Epidemiology*, 4(1):5, 2007.

Transportation of Bits and People: Lessons Learnt and Future Challenges

Bhaskaran Raman, Department of CSE, IIT Bombay

Abstract: In today's globalized economy, there are two grand challenges. (1) Transportation of bits; that is, communication and information access for everyone, and (2) Transportation of people; this is necessary since not all activities can be managed through remote communication.

What is the state of the art in terms of communication solutions in the developing world? What are the current stumbling blocks? These are some questions we comment on. Also importantly, how can communication be used to alleviate teething issues of physical transportation, especially in cities in developing regions? We take the position that the immense potential in this space is largely untapped, with solutions from the western world inapplicable for a variety of reasons.

While many of the observations may be applicable in a wider context, we specifically focus on **India**, since this is where the author's experience lies primarily.

1 The Communication Problem

The usage levels of current communication technology are quite low. As of 2007, cellular penetration in India was less than 20%, and Internet penetration was less than 7% [2]. There are two main reasons for the low penetration of current communication technology in much of the developing world: (1) high system cost, (2) high power consumption. We now comment on these aspects with respect to (a) cellular technology, which is well established, and (b) WiFi-based broadband connectivity, which has been proposed and deployed as a low-cost alternative [10, 4, 12].

System cost: Current cellular technology can provide coverage to places where the ARPU (Average Revenue Per User) is >$5 per month; and adding the next rung of 100 million customers will require a technology which is profitable with <$2 per month ARPU [8].

Several researchers have experimented with WiFi as a low cost technology alternative [3, 4]. WiFi spectrum is free too. However, commercial outdoor WiFi products are still substantially expensive since they have not reached economies of scale. Further, a long-distance WiFi network requires tall towers which are much more expensive than the communication equipment [11].

For instance, consider the Ashwini project, which used a long-distance WiFi network to enable video conferencing based services for several villages surrounding the town of Bhimavaram, in Andhra Pradesh, India [1]. The per-site communication cost was about $1.7K, much higher than the off-the-shelf WiFi radio itself (under $50).

There are also other hidden costs which are substantial. Here too, the Ashwini project reported over $1.5K each for (a) electrical fixtures required in a video conferencing room, and (b) power backup costs. Overall, the project setup cost was estimated to be over $0.5 million for 30+ villages, with plans to recover the amount (break-even) over a period of five years. Clearly, such a scale of operation with a break-even period of several years is not an attractive commercial venture.

Power consumption: The problem of poor availability of power in Indian villages is well known [3, 4]. However, the importance given to the issue in technical solutions is inadequate[1]. One of the main stumbling blocks in the deployment of cellular technology in several villages in India is that the power (backup) costs are too high[2]. Traditional cellular base-stations consume about 3.5-5 kW and also require air conditioning. The infrastructure cost for this, including the power backup for such a system can cost tens of thousands of dollars. Although low power base stations have been developed in the last couple of years, the power consumption still is around 0.5-1 kW.

In WiFi systems, the power consumption of the radio itself is low: a few Watts only. However, if video conferencing applications are to be used, as in the Ashwini project, the power backup for the PC and video conferencing equipment can be high [1].

Open technical issues in WiFi-based systems: WiFi appears to be a better option than cellular in terms of cost, power consumption, and data bandwidth, and several WiFi-based long-distance networks have been deployed. However, many open technical issues remain.

(1) *Tower cost minimization:* The foremost issue is that network topology creation to minimize cost. While [11] represents a beginning, the mechanism proposed does not scale beyond a few tens of nodes. Algorithmically, as well as in terms of practical validation, much remains to be done.

(2) *Maintainability:* One of the main reasons for the failure of several technology efforts is that maintainability is not factored into the original plan. There is lack of expertise, especially at low cost and/or at remote rural regions. With respect to WiFi networks, the work in [12] focuses on the important issue of node-level fault diagnosis. However, network-wide performance diagnosis is very much an open issue. Even in WiFi enterprise settings, the issue of problem diagnosis has not been adequately addressed. Lack of adequate performance/problem diagnosis is one of the reasons for the relative failure of WiFi-based community mesh networks. Before long-distance WiFi networks can be widely deployed, the issue of network problem diagnosis and manageability needs to be addressed.

(3) *Scalable protocol operation:* It is well understood that TDMA-based operation is needed [9, 6]. However, prototype implementations have not really been tested beyond a single-hop topology. The practicality and scalable operation of such protocols thus remain open issues.

IEEE 802.15.4: a possible choice: In addition to WiFi, a possible technological choice we are exploring is IEEE 802.15.4. In the Lo^3 project (**Lo**w-cost, **Lo**w-power, **Lo**cal communication), we seek to use a mesh network of

[1] For instance, the OLPC (`http://laptop.org/`) project largely addresses only the affordability issue and not the power issue.

[2] Personal communication, CTO, Tata Teleservices, India.

802.15.4 radios setup atop rooftops (thus avoiding tower costs), to provide a PBX-like local voice communication system within a village/region. 802.15.4 has limited radio capacity (250 Kbps), but has significant advantages in terms of low-cost (like WiFi) and very low power consumption (lower than WiFi).

We have a preliminary prototype of Lo^3, which shows a lot of promise. A Tmote Sky platform using the CC2420 802.15.4 radio consumes as little as under 100 mW, and also has good duty-cycling support. Such low power consumption has the potential to permit operation of network nodes on battery for several weeks/months. Our prototype has been able to achieve over 50 Kbps throughput over up to nine hops; this is sufficient to support 3-4 simultaneous voice calls.

We believe that such technology alternatives are important, given the heterogeneity of Indian villages. Some villages may have cellular coverage. In others, WiFi-based broadband video-conferencing services may make sense, like in the Ashwini project. In yet others, further scaling down the cost and power requirements would make sense, and Lo^3 would be applicable.

2 Alleviating Transportation Issues

City commute is the common case in people's transportation needs. Cities in developing nations face a severe problem of transportation management. The number of vehicles on Indian roads has been growing at an enormous rate of 10.16% for the last five years (`www.nhai.org`). In Bangalore, 5 million vehicles ply on barely 3000 km of road length.

In the western world, the notion of ITS (Intelligent Transportation Systems, `http://www.its.dot.gov/`) for improving road traffic is well developed. What are the techniques used? Can they be applied for Indian roads?

Broadly speaking, two kinds of sensors are used in current ITS solutions in the western world: static sensors, such as cameras or magnetic loop sensors embedded in the road. And mobile sensors, in the form of GPS-enabled probe vehicles. The sensed data is fed into various mathematical models for extracting information such as traffic density, estimated commute time, route planning, etc.

There are four broad reasons why we think these solutions will not apply in Indian conditions. (1) First and foremost is the high cost, especially that of static road-embedded sensors. Also related is the maintenance cost, which is an issue for impoverished and/or badly managed and/or corrupt city municipalities. (2) Many techniques used in western ITS solutions assume uniformity in vehicles. Indian roads have huge variability in terms of vehicle size, geometry and speeds. (3) The techniques also assume orderly traffic, in clearly separated lanes; this is rarely true for Indian roads. (4) Finally, most current solutions are for freeway traffic, which happens to be a significant case in the west; but regular city roads are the common case in India.

We thus believe that the application of communication technology to address traffic issues in India needs a completely different and radical approach. While there has been some early work, this problem domain is largely unexplored. The work in [5] has sought to exploit the (expected) widespread availability of smart, GPS-enabled phones on Indian roads. The work in [7] builds mathematical prediction models for Indian buses. A uniquely Indian feature, which we feel is worth exploiting, is the use of audio sensors and sound signal processing, to deduce various useful aspects of the current traffic conditions. A few early experiments have shown promise in terms of gauging vehicle speed based on the Doppler shift of horn sounds.

These represent early beginnings, and we believe that the potential for the application of communication technologies for alleviating traffic issues is largely unchartered territory.

3 Conclusion

In summary, we believe that there are two grand challenges in developing regions: (1) affordable communication for the masses, especially in rural areas, and (2) use of communication systems for better traffic planning and management, especially in cities. Open issues in both domains are aplenty, with much potential for research and development.

References

[1] Project Ashwini. `http://www.csdms.in/conflux/`, Oct 2005.

[2] ITU ICT Eye. `http://www.itu.int/ITU-D/ICTEYE/Indicators/Indicators.aspx`, 2009.

[3] Pravin Bhagwat, Bhaskaran Raman, and Dheeraj Sanghi. Turning 802.11 Inside-Out. In *HotNets-II*, Nov 2003.

[4] Eric Brewer, Michael Demmer, Bowei Du, Kevin Fall, Melissa Ho, Matthew Kam, Sergiu Nedevschi, Joyojeet Pal, Rabin Patra, and Sonesh Surana. The Case for Technology for Developing Regions. *IEEE Computer*, 38(6):25–38, June 2005.

[5] Prashanth Mohan, Venkata N. Padmanabhan, and Ramachandran Ramjee. Nericell: Rich Monitoring of Road and Traffic Conditions using Mobile Smartphones. In *SenSys*, Nov 2008.

[6] Rabin Patra, Sergiu Nedevschi, Sonesh Surana, Anmol Sheth, Lakshminarayanan Subramanian, and Eric Brewer. WiLDNet: Design and Implementation of High Performance WiFi Based Long Distance Networks. In *USENIX NSDI*, Apr 2007.

[7] Y. Ramakrishna, P. Ramakrishna, V. Lakshmanan, and R. Sivanandan. Use of GPS Probe Data and Passenger Data for Prediction of Bus Transit Travel Time. In *Transportation, Land Use, Planning and Air Quality Conference*, Jul 2007.

[8] Bhaskar Ramamurthy. Will India Witness a Cellular Broadband Revolution? WISARD 2007, Keynote talk, Jan 2007.

[9] Bhaskaran Raman and Kameswari Chebrolu. Design and Evaluation of a new MAC Protocol for Long-Distance 802.11 Mesh Networks. In *11th Annual International Conference on Mobile Computing and Networking paper (MOBICOM)*, Aug/Sep 2005.

[10] Bhaskaran Raman and Kameswari Chebrolu. Experiences in using WiFi for Rural Internet in India. *IEEE Communications Magazine, Special Issue on New Directions In Networking Technologies In Emerging Economies*, Jan 2007.

[11] Sayandeep Sen and Bhaskaran Raman. Long Distance Wireless Mesh Network Planning: Problem Formulation and Solution. In *WWW*, May 2007.

[12] Sonesh Surana, Rabin Patra, Sergiu Nedevschi, Manuel Ramos, Lakshminarayanan Subramanian, Yahel Ben-David, and Eric Brewer. Beyond Pilots: Keeping Rural Wireless Networks Alive. In *NSDI*, Apr 2008.

Some Grand Challenges for Computer Science In Service of Global Development

Roni Rosenfeld
School of Computer Science
Carnegie Mellon University
Pittsburgh, Pennsylvania, USA
+1-412-414-3701
roni@cs.cmu.edu

ABSTRACT

In what follows, I suggest a few challenges for discussion at the workshop and possible inclusion in a community-wide list of grand challenges for computer science in the service of global development. These are not meant to be exhaustive or even representative of the ICT4D field as a whole. Rather, they stem from challenges I have encountered or entertained in my own work.

Keywords
ICTD, ICT4D, Global Development, Grand Challenges.

1. EXPAND HUMAN COMPUTATION TO THE BOTTOM BILLION

Games With A Purpose (GWAP, [1]) has recently shown the potential of distributed, unskilled human information processing to produce value by suitably designed symbiosis with computers. Amazon's Mechanical Turk (www.mturk.com) is attempting to create a marketplace for Human Computation, allowing people to earn income in the process. Although some attempts are being made (e.g. txteagle.com), Human Computation has not yet been shown to be useful to the Bottom Billion. The challenge is then to invent, develop and test methods to harvest the enormous brain power and innate human skills of uneducated, unskilled (especially urban) people throughout the developing world to:

- Provide them with additional income (as is done for the rural poor by Polak [2]).
- Increase their future earning power by increasing their skills.
- Produce immediate value for their communities.

2. BRING THE BENEFITS OF TEXT-BASED COMMUNICATION TO LOW-LITERATE PEOPLE

The internet revolution brought human communication to a new level, allowing asynchronous, person-to-person communication as well as various forms of peer communication, multicast, broadcast, and collaboration. However, these benefits do not currently accrue to the billions of people who are not literate enough to partake in them. The challenge is then to develop speech technologies to provide the equivalent capabilities to illiterate and low-literate people in their native languages, e.g.:

- Person-to-person asynchronous communication (email ➔ v-mail).
- Closed group communication (mailing lists ➔ group v-mail).
- Open group communication (B-boards ➔ v-boards).
- Personal expression and subscription (Blogs ➔ vlogs; twitter ➔ v-twitter).
- Team content creation (wiki ➔ viki).

3. AUTOMATED TUTORING FOR THE BOTTOM BILLION

Automated tutoring is still a hard and unsolved problem when applied in the relative luxury of the developed world (e.g. learnlab.org). It requires solving problems in artificial intelligence, cognitive science, user interfaces, pedagogy and other related areas. Making it work for the Bottom Billion will require progress in many additional fields, including speech and language processing, content translation and adaptation, and a radical rethinking and redesigning of user interfaces for uneducated and less educated users. This grand challenge includes, as special cases, *language learning* [3] and *vocational training*.

An even more ambitious challenge is the development of technology for *inexpensive, standalone, mobile, bootstrap education*. Alexander the Great was tutored individually by Aristotle. Today, we should strive to provide such personalized education to every child in the world. The challenge is then to demonstrate an automated tutor that can continuously engage a young child in the developing world and, over a long period, provide them with culturally and economically appropriate education. Next, make this available in all languages and cultures.

4. USER INTERFACES FOR THE BOTTOM BILLION

The challenge is to develop a theory and practice of fail-safe interfaces that afford applications lower barriers to their *use*, to their *deployment* and to their *development*:

- *Application usability:* A commonly expressed standard for software usability is “my grandmother should be able to use it”. This should be expanded to “make it possible for the Bottom Billion to use it”. *HCI4D should drive much of ICT4D.* In fact, even the research methodology itself must be re-designed: user study techniques must be changed to fit developing world factors such as illiteracy, socio-economic disparities, cultural taboos, and linguistic barriers. Although the HCI literature has dealt with these issues, the work must be dramatically intensified and accelerated [4].
- *Application deployment:* systems must be designed to be maintainable using the level of skill readily available in the developing world.
- *Application Development:* Tools for application and content creation should be designed for the level of skill typically found in NGOs and other grass roots organizations.

For a recent example of how these challenges were tackled in spoken language interfaces, see [5].

5. DRAMATICALLY REDUCE THE COST OF MOBILE COMMUNICATION IN THE DEVELOPING WORLD

Air time in most developing countries is now significantly more expensive than it is in developed countries, especially when Purchasing Power parity (PPP) is used for comparison. This is a significant barrier to many ICT4D initiatives and renders many otherwise good ideas infeasible or unsustainable. Lowering this cost is a significant enabler of ICT4D and of development in general. Although the problem is at least partially political rather than technical, it may still be possible to solve it or finesse it with technical inventions.

6. REFERENCES

[1] Von Ahn, L. 2006. Games with a purpose. *Computer*, **39** (6), pp. 92—94. ISSN: 0018-9162

[2] Polak, P. 2008. *Out of Poverty*. Berrett-Koehler Publishers. ISBN 1576754499, 9781576754498.

[3] Kam, M., Kumar, A., Jain, S., Mathur, A., and Canny, J. 2009. Improving Literacy in Rural India: Cellphone Games in an After-School Program. *Proc. IEEE/ACM Conference on Information and Communication Technology and Development (ICTD ’09*), Doha, Qatar, April 17-19, 2009.

[4] *ITID* **5**(4), Special issue on HCID, expected December 2009.

[5] Sherwani, J. 2009. Speech Interfaces for Information Access by Low Literate Users. *Ph.D. thesis, Computer Science Department, Carnegie Mellon University*.

An ICTD Research Agenda from the Trenches

Umar Saif, LUMS Pakistan

Poor Man's Akamai and DonateBandwidth.net

Developing countries like Pakistan are going through an interesting dichotomy. On one hand, due to the lack of existing communication infrastructure, developing countries can often leap frog the developed countries in technologies such as high-speed WiMAX, Fiber backhaul and pervasive deployment of WLL. On the other hand, due to expensive and fault-prone undersea international links, monopoly of transit ISPs and poorly configured and maintained ISP routers and servers, access to the "Internet" is low-bandwidth, expensive and frequently intermittent.

Therefore, theoretically the Internet in the developing countries must appear like an island of rich, high-speed connectivity with few links connecting this island to an Internet hub in a developed country. By this logic, content within the country must be accessible over higher bandwidth links compared to content outside the country. Unfortunately, lack of proper peering points between ISPs in the developing-world prevents end-users from taking advantage of the rich intra-country communication infrastructure. This is because ISPs in the developing world typically have to sign transit agreements with upstream ISPs. There is typically no notion of a peering agreement between small regional ISPs. Smaller ISPs, therefore, purchase bandwidth at a cost derived from the price of an international Internet link, even when its traffic is destined for a host connected to an ISP in the same region. Even when an Internet Exchange Point (IXP) exists, it is typically owned by a large upstream ISP. Such an IXP is mostly a marketing term by a transit provider, offering a router configured for BGP4 packet-exchange and "sold" as a transit agreement to the Internet at international bandwidth rates. On the other hand, when no such IXP exists, traffic generated by a user in the developing world has to actually traverse international links even if the recipient is connected to an ISP in the same region.

A solution to this is a mushrooming of "star-shaped" ISPs that buy connections from different ISPs and start acting like regional peering points -- "selling" accelerated Internet bandwidth to local content and end-hosts. In a way, such an ISP provides the same service as a CDN like Akamai, but with a completely different purpose; where a CND redirects traffic to a nearby replica for "accelerated access" to content, such an ISP provides a direct, higher-bandwidth routing path between hosts connected to different ISPs.

Of course, a star-ISP can easily start caching data for content hosted on clients connected to different egress ISPs, further enhancing the bandwidth and reducing the latency in accessing local content. We envisage that such ISPs will naturally emerge with the growing need for locally hosted services and content to circumvent frequent outages on undersea International links.

We have also found an interesting service layered on top of such a star-ISPs, dubbed DonateBandwidth.net. Just as systems such as SETI@Home and OceanStore permit users to share their computing cycles and storage space with others on the Internet, DonateBandwidth.net permits sharing of unused Internet bandwidth (which is much more valuable in the developing-world compared to computing cycles or disk space). Our system, dubbed DonateBandwidth.net, has two key components: (1) An p2p cache that consolidates and enables sharing of data across users of different ISPs, and (2) a forward-caching architecture deployed at a star-ISP that enables users to donate their unused bandwidth by "forward populating" the ISP cache for other users. Forward caching is implemented by broadcasting a file download request to other users of the ISP willing to donate bandwidth. Donating nodes initiate download of non-overlapping chunks of the requested file such that the ISP cache is pre-populated with the file chunks by the time the low-bandwidth client starts downloading them. Viewed differently, forward-caching permits hosts connected to a star-ISP to collaborate with each other to speed-up downloads for each other. Once downloaded, the data is also cached by the participating clients such that future requests for download may be served locally by a p2p transfer across the star-ISP hosts.

Inverse Multiplexing of GPRS Connections

A lot of recent research in ICTD has focused on Delay Tolerant "sneaker-nets" (DTNs) or long-range WiFi links for connecting rural and semi-urban areas in the developing-world. However, while most rural areas in a country like Pakistan lack high-speed networking infrastructure, cellular wireless networks have grown at an exponential rate. For instance, it is rare for even a remote rural area in Pakistan to have no GSM and GPRS coverage. Of course, individual cell-phone connections are typically allocated a bandwidth of less than 20kb/sec, precluding applications such as high-speed Internet access, exchange of large email attachments, telemedicine and distance-education. However, cell-phone users in a village collectively have ample bandwidth to support applications likes telemedicine and distance-learning. For instance, 20 people with GSM cell-phones can collectively utilize more than 250kb/sec bandwidth. What is missing, however, is a system that can enable individual cell-phone users to opportunistically "coalesce" their bandwidth to forge a high-speed network connection.

Given the drastic fluctuations in bandwidth, latency and packet loss on GPRS connections, such inverse multiplexing of a multitude of GPRS connections is not without challenge. Especially for applications such as distance education or telemedicine, video players and servers, equipped with scalable coding and multiple description coding (MDC) must be developed. Furthermore, if the inverse-multiplexed links are used for real-time video download, the server must be made aware of multiple download channels. We are currently exploring the development of a streaming media servers that identifies multiple inverse multiplexed connections via a "cookie" and uses multiple descriptor coding to adaptively stripe data across multiple channels based on runtime properties of each GPRS connection. We liken this to *http range-queries* that we use for web-based downloads – albeit with appropriate segmentation and prioritization of video frames over multiple channels based on their runtime properties.

Finally, an important challenge is to develop a middleware in which an application can specify the degree of acceptable degradation in the face of bandwidth variations. The middleware should gracefully upgrade or degrade an application session depending on available connections and bandwidth. For instance in case of the telemedicine example, the application may let the video stream deteriorate but the physiological stream must be consistent for the correct operation of the application. As available mobile nodes (cell-phone) change, the middleware should adapt the video, audio and physiological streams according to application preferences.

Towards a Developing-world Teleputer

If you knew that only 5% of the population in Pakistan owns a "computer", you would be surprised at a claim that more than 40% of the population regularly uses a computing device; it is often easy to overlook the true potential of the pervasive adoption of cell-phones in the developing-world.

Equipped with a robust network connection, a general-purpose processor and on-board storage, a typical cell-phone can help "automate" many applications. However, in this paper we argue that while a cell-phone may have adequate computing resources to become the "developing-world computer", its input-output interfaces – the "I/O" interface – are fundamentally mismatched to the needs of a developing-world computer. Importantly, rather than proposing a mere "localization" of the alpha-numeric keyboard, we propose two fundamental changes to the I/O interfaces of a cell-phone:

1. **Sensing and Actuating**: In most rural and semi-urban applications, the need for computing often arises in the context of sensing and actuating – such as salinity detection sensors, telemetry equipment for water management, ImmunoSensor chips for early disease detection.
   Unfortunately, despite the emergence of standard sensor platforms like Motes, off-the-shelf sensors come with proprietary and often closed interfaces. We envisage a device where a villager simply attaches a sensor to a teleputer, "clicks" an animated icon representing the sensing application, and the teleputer verbally notifies the villager about the result e.g. level of salinity in soil. For this to happen, standardized sensing and actuating interfaces must be embedded in the cell-phone, while the cell-phone OS must support an interface for programming sensing and actuating devices.

2. **Text-free Operation**: Over the last few years, the user interfaces of cellphones have gone through a dramatic change. For instance, full keyboard devices such as blackberry make it simple to input text, while cellphones like the iPhone can accurately recognize human gestures. However, ironically, these increasingly sophisticated user interfaces often make cell-phones less useable by illiterate and semi-literate users in the developing-world.
   We believe that the key to turning a cell-phone into a "computing device" in the developing-world is the mainstreaming of speech-based dialogue interfaces in cell-phones. For instance, even the most advanced cell-phones do not include a text-to-speech module which other services can call using an open-interface to communicate with an illiterate user. An obvious example is a text-free SMS service for people who cannot read. On the sending side, the difficulty of open-ended speech recognition can be mitigated by a speech-based dialogue system that guides the user to compose a command or a message in the same vein as the T9 system guides typed messages.

Mobile Technology for Global Development in Africa: Current and Future Initiatives in Senegal

Christelle SCHARFF
Pace University, New York, NY, USA
Email: cscharff@pace.edu

Africa is the fastest growing mobile phone market in the world. Mobile phones can create lots of opportunities for social changes for the people in Africa – expected, unexpected and far beyond imagination. The literature showcases many examples in education, banking, health care and agriculture. But for the time being, the impact of mobile phones is still very limited by the number, difficulty of large distribution and scope of available mobile phone applications – in particular applications targeting the real needs of a diverse African population. Another important fact to mention is that most of the initiatives focusing on mobile technology in Africa – at the educational, non-profit and for-profit levels - are mostly taking place in English-speaking rather than French-speaking countries.

This abstract presents current and future plans for the introduction of mobile application development in the curriculum of universities in Senegal, a francophone country. It also showcases examples of mobile applications developed by Senegalese students and currently deployed in the country.

How to Train Skilled Software Engineers in Mobile Technology in Africa?

An important question to be asked is: Who will develop the mobile phone applications that will have a social impact on the populations of Africa? We believe that current and future African students will have to play this crucial role but need to be prepared appropriately as software developers, software engineers and entrepreneurs in mobile technology. Local African companies specialized in mobile begin to surface and cannot find the right workforce coming out of the African universities.

To address these issues, the University of Thiès in Senegal and Pace University and Stony Brook University in the US are currently collaborating together to enable undergraduate computer science students of the University of Thiès to work together on the development of mobile phone applications that will have social impact in their country. The model of working is based on providing the students with the required skills in mobile application development (including software engineering) and entrepreneurship through boot camps and immersion courses, applying their skills on "real projects" for "real clients" to develop "real solutions", deploying the solutions to trained clients, and working closely with the clients to monitor and evaluate the impact of the solutions. The collaboration has been extended to include other universities in Senegal such as the University Gaston Berger in Saint Louis and the Ecole Supérieure Polytechnique in Dakar. To include as many actors of the playing field as possible, links with local associations, NGOs and the industry in Senegal have been established. A training for Computer Science faculty of six of the main universities in Senegal took place at the Manobi headquarters; Manobi (http://www.manobi.net) is currently the only provider of mobile solutions and services in Senegal. Training faculty for this new and promising area of Computer Science is crucial for the replicability, scalability and sustainability of the project, but also for the development of the field at all levels in Senegal.

What Mobile Solutions for Africa?

The current portfolio of mobile solutions developed by the Senegalese students is composed of accounting applications to be used by artisans, educational games for young children, and applications to improve the quality of life of students on campus. The accounting applications permit artisans to control their sales and spending and make them distinguish private from business spending. Gathering data from these applications would permit to understand the economical role of the traditional craft sector in Senegalese economy. Educational games developed to date include games to be used in class by teachers to test pupils' knowledge in recognizing numbers and letters and doing basic arithmetic operations. The games are well perceived in classrooms of more than sixty pupils in rural areas and give pupils a first contact with a keyboard to be transferred to a computer. Students in universities are facing many administrative difficulties frequently leading to strikes; a new initiative is under-way to improve the conditions of life of students on campus using mobile technology.

The wiki of the project is available at: http://www.mobilesenegal.com.

Acknowledgement

This work is supported by two National Collegiate Inventors and Innovators Alliance Sustainable Vision grants, "Sustainable Technology-based Entrepreneurship for the Senegalese Market" and "Bulding a Global Network to Support Sustainable Information and Communication Technology Entrepreneurship in Senegal", and by Nokia. Thanks to: Anita Wasilewska and Jennifer Wong (Stony Brook University, US), Mamadou Bousso, Ibrahima Ndiaye, and Cheikh Sarr (University of Thiès, Senegal), Seydina Ndiaye (University Gaston Berger in Saint Louis, Senegal) and Alassane Bah (Ecole Supérieure Polytechnique in Dakar, Senegal).

Key Techniques in Building Effective ICT Solutions in the South African HIV/AIDS Sector

Kieran Sharpey-Schafer
Cell-Life
80 Roeland Street
Cape Town, South Africa, ZA8001
+ 27 21 469111
kieran@cell-life.org.za

ABSTRACT
Cell-Life is an organization providing innovative ICT solutions in South Africa's HIV/AIDS sector. Contexts such as resource constrained clinics provide practical challenges to projects such as large workloads and high staff turn-over. To cater for these more human factors, and unearth them early, Cell-Life have found a number of requirements gathering techniques along with Evolutionary Prototyping of the tools increase effectiveness of the tools in making a difference to stakeholders jobs.

Keywords
Developing context, Evolutionary prototyping, Non-Functional Requirements

1.INTRODUCTION

Cell-Life is a not-for-profit IT development organization based in South Africa. Its focus is on developing tools that will help aid the public health and civil sector in their efforts to address the HIV/AIDS pandemic.

South Africa is notorious for its levels of disparity in various fields of health, education and business[1]. The same is equally true for IT, where we can often find the latest most innovative solutions being used in the private sector, whilst public health and civil society still rely on manual and paper based processes. This is a problem as they are unable to leverage the various benefits that ICT can bring such as: security, scalability and time & cost savings.

Thus Cell-Life have attempted to build a number of tools dedicated to harnessing ITC benefits, namely:

intelligent Dispensing of Anti-Retroviral Therapy (iDART) is a dispensing tool aimed at increasing the capacity of low resource ART clinics, which are characterized by high patient volumes and staff turnover.

Emit is a mobile phone application that allows organizations to do data collection in the field using mobile phones. This has many uses in patient follow up or reports submission for community educators, and largely reduces paperwork.

Mobilsr is a web based mobile gateway that allows NGO's to use broadcast mobile services, and has uses from patient reminder messages, calls to action or public engagement.

2.CONTEXT

Much has been written or hypothesized regarding projects in developing contexts, whilst sadly not always having successful or sustainable outcomes.

On occasion the conversation on the details of a cutting edge technical solution can make up for a lack of education or specific IT skills in a particular project. Our experiences have often shown the contrary, that users are mostly quite capable of learning new systems and actually often have a more open-minded approach. Furthermore, the focus on technical issues often have less impact on the project outcomes than the more unique human factors in the context of deployment. These can be environmental, cultural or organizational issues or barriers that are foreign to the context that the software was often designed.

Cell-Life's experience has shown that a 'developing area' is rather characterized by resource or skill constraints that are more common in 'non-developed' environment. Pharmacies in the public health sector experience high patient volumes along with high staff turn over.

Furthermore, as Rosling [2] advocates, it can be dangerous to paint 'development' as a single environment. As whilst there may be commonalities in practice or job function, barriers to operation are often unique to each context.

3.KEY TECHNIQUES

There are a few techniques which Cell-Life have learnt that are critical to dealing with this context. Predominant in-the-field learning as well as outputs from research and industry practice, have been used to improve success rates of projects. Some key techniques include:.

3.1Requirements Elicitation

Requirements Elicitation is the pursuit and investigation of true requirements – not merely the collection of the most apparent requests taken at face value. True in most requirements efforts, the fact remains that many assumptions, organizational rules and cultural differences hinder the communication of the projects true requirements and operating environment. A few techniques to help with this include:

3.1.1Stakeholder Analysis

A project is often requested or planned by a few stakeholders. Often this can either be management or low-level users who monopolize the focus of the system impact. Rather all stakeholders need to be considered, including those external to the system or project but still have an interest in the outputs. How will the implementation change and /or benefit their daily work? What are their priorities and interests? What challenges do they foresee in operation. Interestingly, two stakeholders will seldom have same perception of the same event.

3.1.2Requirements Gathering & Participation

Joint Application Development (JAD) workshops, interviews, observation and document analysis, remain key ways of discerning the true needs & environmental constraints in a particular project [3]

3.1.3Identifying a Champion

A Champion [4] is defined by a local stakeholder who has a vested interest in driving the project internally. Through stakeholder analysis one can determine that this individual will gain grate benefit in their daily work from the system implantation. An example of a pharmacy manager indicates that iDART will reduce patient queues, reduce stock shortage and generate accurate Pharmacy reports. Importantly the Champion can be a great source of challenges and ideas about the context, and also maintain enthusiasm in the user team once the 'novelty factor' has worn off.

3.1.4Specific, Measurable Objectives

This means specifying detailed objectives of the project at the outset. For example instead of "The system will make the Pharmacy more efficient and be user-friendly", rather it should be "we aim to reduce dispensing time to less than 2 minutes for patients who exist in the system, and new users can be reliably trained in less than 15 minutes".

3.2Evolutionary Prototyping

Despite the many best practices no requirements gathering process or solution specification can uncover and cater for all project factors. This is especially true in the developing context where processes and procedures are not always well defined.

Here evolutionary prototyping of solutions becomes critical in handling the changing and ill defined context. Evolutionary prototyping is the practice of building software solutions incrementally, and most importantly, each increment is in use by stakeholders who are then engaged on the priorities for the next increment.

This means the priorities of the original feature list are constantly reviewed to uncover false assumptions and what is actually critical to the jobs of the stakeholders..Furthermore, this process can expose various operational challenges and barriers which were not considered in the initial design. This is often critical, especially in contexts where the actual organizational workflows are being developed in parallel with the system. At the same time the, this combination can also easily allow scope creep into the project if not managed carefully.

Importantly Evolutionary Prototyping also leads to great 'buy-in' from the users as they help 'build' the system[5].

3.3Non-Functional Requirements

A key analysis to be performed during both Stakeholder Analysis and Evolutionary Prototyping, is the establishment of the Non-Functional Requirements (NFR) of the Project. (Also sometimes known as Quality-of-Service requirements).

Whilst functional requirements are concerned with specifying what a system can do, the NFRs are meant to describe *how well* the system does it. There are many NFRs that a system can be evaluated on including: performance, security, flexibility, usability, cost, interoperability, standardization and many more. In fact a users preference for two systems that are identical in functionality often stems from a preference on a particular NFR. An example of this might be to mail clients, and users generally agree that one is faster and easier to use than the other. Furthermore this difference between solutions – another application might be favored as it is more secure and cheaper than the alternatives – despite having poor usability.

Interestingly, this prioritization of the NFRs of a solution have been found to differ between stakeholders in a project and thus needs to be carefully considered. Again using the example of the iDART, it was found that speed of dispensing and ease of training mattered most to the pharmacist, whilst clinic managers were mostly concerned with accuracy (of stock and clinic reports) and the ultimate cost of the system. Analysis needs to investigate any potential conflicts in these NFR priorities (e.g. accuracy and speed could potentially conflict), and focus on these can lead to making a measurable and effective impact in the stockholder's work.

5.CONCLUSION

Whilst Cell-Life's initial projects experienced mixed results, the incorporation of the discussed techniques at planning time, have increased our successful impact in aiding the sector. Specifically, the techniques have allowed the team to reliably deal with the change and uncertainty in the sector with greater agility.

6.ACKNOWLEDGMENTS

This paper was made possible by Cell-Life, its talented staff and our committed project partners on the ground. The three tools discussed are now implemented at over 30 health facilities, 50 NGOs and have more than 300 users.

7.REFERENCES

[1] United Nations Development Program, 2008, 2007/2008 Human Development Report South Africa , http://hdrstats.undp.org/countries/country_fact_sheets/cty_fs_ZAF.html

[2] Rosling, H. 2006, Myths About the Developing World, TED Conference 2006, http://www.ted.com/index.php/speakers/hans_rosling.html

[3] International Institute of Business Analysis, Business Analyst Body of Knowledge V1.6, http://www.theiiba.org

[4] Bridges.org, 12 Habits of Highly Effective ICT-Enabled Development Initiatives, http://www.bridges.org

[5] Brown, S. et Al., 2007, The Delivery of ARV Dispensing Software: Lessons Learnt from Four Pharmacies in South Africa, IST-Africa 2007 Conference Proceedings, ISBN: 1-905824-04-1

The Challenges of Graduate Research in ICTD

Jahanzeb Sherwani
School of Computer Science
Carnegie Mellon University
jsherwan@cs.cmu.edu

Divya Ramachandran
Department of Computer Science
University of California, Berkeley
divya@cs.berkeley.edu

INTRODUCTION

In this position paper, we would like to bring to a light a number of issues we feel are imperative for us as a research community to discuss, debate, and grapple with. These issues run through the heart of ICTD and Computer Science, and while they are rarely (if ever) discussed in the presentation halls of conferences, they are often the subject of discussion and tension among one of the key stakeholders in ICTD research: graduate students. It is our hope that by discussing these issues in the wider community, we can hope to achieve some resolution of these tensions.

TENSION: IS THIS COMPUTER SCIENCE?

Even though Eric Brewer did a series of groundbreaking talks in a number of US universities in 2006 arguing for the case for ICTD research to be regarded as valid CS research, there are still many issues to be resolved within the community (Anokwa, 2009). The eternal question remains: is this really Computer Science? This question is often left unanswered and undisturbed, while the rest of the research initiative is conducted around it. In this paper, we argue that we need to debate these issues openly in the community, as without doing so, we are giving rise to a number of problematic issues.

CS-Envy

First, there the issue of CS-envy – the attempt to make ICTD research seem more like CS, so that it is acceptable as valid CS research. There are often very valid and interesting ICTD research questions involved, but these are usually overlooked in the interest of focusing more on aspects that will seem worthy of a CS PhD. One graduate researcher says it best: "While the primary goal behind [CS] research is to make a contribution of knowledge to a greater community, the goal of [ICTD] work at large is to design appropriate technologies that can sustainably improve lives in the poorest communities in the world. As [ICTD] researchers we often find ourselves in conflict between achieving these two goals." (Ramachandran, 2009).

One graduate student primarily worked on understanding user interface design issues in the field, and on making explicit the implicit problems faced when conducting user studies. Yet he purposefully added a chapter to his thesis focusing on the technical results of a speech recognition approach, since he knew he had to "beef up" the technical aspects of the work, as some members of his committee had explicitly raised the question of whether the research could be regarded as "Computer Science" research.

The strategy of picking low hanging technical fruit to satisfy such requirements will only get harder as time goes on, and this will leave a larger burden for subsequent graduate students to do not only relevant and novel ICTD work, but also relevant and novel CS work, to complete their dissertations. We need to discuss this issue, and we need to establish what goals we as a field feel is important for us, so that efforts can be directed towards those goals, rather than the goals we've inherited from the field of CS.

Hard Science vs. Soft Science

One recurring issue is that of evaluation. How should we evaluate ICTD work? One graduate researcher relayed the following to us: "As a researcher working on interfaces for low-literate users, I was always looking for statistically significant measures that could show that my method was better than the alternative. The lesson we seem to implicitly learn as graduate students is that the ultimate test of value is whether p is less than 0.05 or not: this is where the best papers awards are, and where the work really becomes "scientific". Of course, my advisor was always encouraging me to see what broader lessons I could learn from the experience, and from the data, but at the end of the day, we both knew that unless there were hard numbers that validated the research, more work would be required." (Anonymous, personal communication)

PERHAPS THIS ISN'T COMPUTER SCIENCE!

One way to shed CS-envy is to not only accept the fact that we don't fit into the neat boundaries of traditional CS research, but to embrace it. If it is substantially different to conduct ICTD research than CS research, then can we agree that the same rules need not apply? If so, we need to look at exactly how this research is different.

The Challenges of ICTD Research

ICTD design research requires travel, and often the most important lessons learnt are in the field. Often, this means traveling across continents. The overheads of travel are significant, and more recently, graduate students have realized the need to stay "in-country" to do the field research than to stay at the University. The quality of ICTD research will only increase as a result of this, so it is a net positive in terms of ICTD research outcomes. But it also significantly increases the challenge of doing ICTD research for graduate students.

Living "in-country" is difficult for many reasons. First, it is difficult to keep up a work routine in a remote place, and the lack of an academic environment, and perhaps more problematically, the lack of a peer group, means that the graduate student is on their own in many ways. While remote interactions are possible, it is difficult to replace the high fidelity of face-to-face interactions. We have seen that when graduate students spend too much time in the field, it negatively impacts their ability to separate the broader questions from the mundane, day-to-day activities they need to keep doing in the field. It becomes harder to form research questions, much less implement plans to investigate them. It is easy for a graduate student to spend weeks in the field without making much headway, and in many cases, the cause for this are out of the student's control: for instance, if the partner NGO has a large donor meeting coming up, it could easily mean a delay of up to a month.

There are numerous other issues. It is much more likely that the graduate student will endure physical hardships, such as potentially life threatening illness, poor sanitation, and difficult field conditions, when living in the field. One graduate student conducted user studies in a sweltering room without electricity for 8 hours, followed by having his accommodations taken over by a visiting political dignitary right after the draining user study. When he was finally able to get in his room, the water supply had run out (no chance of a shower, other than with the bucket of water on which many dead ants were floating), and frogs were creeping into the room as well. At a different point in his research, he caught typhoid because of food he ate in the field. In a sense, graduate students are paying for their research choice with a fraction of their very lives.

Additionally, there are social adjustments as well – living in an often new environment for extended periods of time, difficult social situations (a vegetarian graduate student was offered a meal with meat, and refusing it would have been rude), and being far away from family and friends.

Even within the research, there are many layers of issues. There are cultural, linguistic, and class barriers for the ICTD researcher. There are partnerships with NGOs and end-users that need to be built, nurtured and grown by the ICTD researcher, which will fade unless she makes an effort to touch base with them every so often. And unlike other field-based disciplines (e.g. anthropology), CS doesn't have an array of courses to prepare you for the field. It's mostly a process of being thrown into the deep end. Even when the graduate student takes a class on a specific domain (e.g. maternal health) to prepare for upcoming research (working with maternal healthcare providers), it's only in the field that she realizes how the perspectives in the classroom are not applicable in the field (Ramachandran, 2009).

In another instance, when discussing subjective evaluations, a recent graduate student said that it's common knowledge that when one works in an ICTD context, the local community gets to know you and like you, and so when you ask subjective evaluation questions, they're going to be biased and give overtly positive feedback – yet, we need to report these statistics to get our papers published, so we do it. We clearly need to move beyond these approaches, but we currently aren't even discussing these issues openly, so many people don't even know these issue exist.

TENSIONS IN NAMING: CS OR ICTD?

Given these realities faced by graduate students in ICTD, it seems a tall order for them to have to deal with these issues, and at the same time, do the regular amount of work that traditional CS PhDs do. In a sense, they are paying thrice the price: first they have to deliver exactly what a regular CS PhD delivers, next they have to deal with hardships and uncertain conditions in the field for which they may be logistically as well as academically ill-prepared, and finally, they may be penalized for this choice of field financially when they enter the job market. So is there a way out? Perhaps a quick-fix solution would be rend apart CS and ICTD, and create a different set of rules for such graduate students, and confer a different degree when they complete their research: a PhD in ICTD?

Even though we've seemingly argued for exactly this, we will categorically state that this is a bad idea. Under the current job market, a PhD in ICTD would be meaningless, and potentially worthless. It is very much a blessing that graduate students are allowed to do such non-traditional research, and still graduate with CS degrees, because these degrees are a standardized "currency" in the job market, which can be used in both academia, industry and in the non-profit world.

CONCLUSION

There are clearly no easy answers to these tensions. There are good reasons for ICTD to try to leave the fold of CS to carve out its own space; yet there are good reasons to the contrary as well. We do not have the answers. However, we believe that we as a community need to engage with these issues openly and honestly, and at least acknowledge their existence, if we are to improve working conditions for the unsung heroes and heroines of ICTD: graduate students.

REFERENCES

1. Ramachandran, D. *Some Challenges in Graduate HCID Research.* Proc. HCCID workshop, CHI 2009, Boston.
2. Anokwa, Y. *A Young Researcher's Thoughts on ICTD Research*. Proc. Young Researcher's ICTD Workshop, ICTD 2009, Qatar.

Defining Hard Technical Challenges in the ICTD space

Lakshminarayanan Subramanian
Courant Institute of Mathematical Sciences
New York University
lakshmi@cs.nyu.edu

ABSTRACT

To make ICTD into a viable, sustainable and attractive research space within computer science, it is imperative that we address two fundamental issues: (a) identify hard technical challenges that define the space; (b) demonstrate the high impact factor for solutions that solve these hard challenges. I will articulate some technical challenges in this space.

1. INTRODUCTION

We, as computer science researchers working on ICTD are at crossroads simultaneously trying to serve two masters. On one hand, we are trying to convince core computer scientists on the value of ICTD research as a discipline within computer science. On the other end, we are trying to convince global development specialists on the value of new technological solutions and their potential to address pressing developmental problems. While the computer scientist wants to know "what are the challenging research problems", the development specialist wants to see "real-world impact and utility for addressing the problem". Not every developmental problem warrants a technically challenging solution and not every technical challenging problem is a real-world problem. One needs to carefully walk this fine line; especially, given our habit of trying to over-generalize problems which often quickly makes the operating assumptions unreal.

I believe there is a simple way out of this. ICTD as a field needs a unified vision around a set of well-defined "grand" challenges that satisfy the test of both the computer scientist and the developmental specialist. Any new emerging field is centered around a vision that specifies a set of hard challenging problems. For example, researchers working in Bioinformatics have a fairly well-defined goal that is coherent across the entire field. Same is the case with other new areas like sensor networks, quantum computing etc. In fact, most fields within the applied sciences have a well-defined vision that enables the community as a whole to present a unified pitch.

The problem with the current state of affairs in ICTD is that our vision is better articulated for the development specialist than for the computer scientist. While I definitely think that there are several significant research challenges within the ICTD space, we as a community have not clearly articulated these challenges to ourselves and to the CS and development communities at large. Worse still, many of us within the ICTD space are not sure about these grand challenges. This confused outlook is problematic and detrimental in the long run.

As one first step, I will articulate some of the interesting technical challenges in this space that I have worked on or come across (this list is by no means complete).

2. CHALLENGES

2.1 Low-cost connectivity solutions

Goal: Develop network connectivity solutions that can provide basic communications in developing regions at extremely low costs. Any such solution has to be economically viable and sustainable. Existing connectivity solutions are extremely expensive and not economically viable especially in rural areas.

Existing works: WiLD networks, Mesh networks, Mechanical backhauls, Delay Tolerant Networks (DTN).

Impact factor: Wireless solutions deployed in 15+ countries and used by millions. DTNs are also deployed in many countries.

Research contributions:

1. New wireless protocols for tailoring WiFi to achieve high performance over extremely long distances.
2. New protocols for routing, transport, naming and addressing in delay tolerant networks.
3. New network management solutions to ease the deployment and maintenance of rural wireless networks.
4. Building solutions for handling power fluctuations and lack of sustained power.

Possible research directions: Within the wireless space, we are currently investigating these directions:

1. Designing high performance multi-radio wireless networks.
2. Significantly enhancing performance of point-point links by leveraging better physical layer solutions such as MIMO, steerable antennas and Analog Network Coding.
3. Design a unified MAC layer for combinational wireless distribution networks comprising of point-point, point-to-multipoint and omni-directional links.
4. Building a new WiFi-based cellular architecture that consumes much lower power and incurs significantly lower cost.
5. Data over GSM: Achieve a data-connectivity layer on the cellular voice channel.

2.2 Extending the Web to Developing Regions

Goal: Improve the penetration of the World Wide Web in developing countries especially in areas with poor, limited or no affordable network connectivity. Even in places where good connectivity exists, the Internet connection is often shared across several users.

Existing works: TEK, RuralCafe, HashCache, EdgeXL, Contextual Search Portals, Riverbed Networks.

Impact: A few deployments in a few targeted locations. Riverbed networks is a large commercial entity.

Research Challenges and Contributions:

1. Network protocols require a complete redesign. HTTP and TCP work very poorly in low bandwidth networks and Web browsing is an extremely slow process. RuralCafe and TEK support an intermittent web browser interface to expose the intermittency to the user. We are currently building a new transport layer that can alleviate many of the transport problems in shared low-bandwidth networks.
2. How to build large Terabyte caches to support offline web access in low-bandwidth networks? We are currently building a contextual search engine that can fetch the vertical slice of the Web on any given topic and store this slice on a hard-disk. The idea is to perform offline content distribution and support offline web access by distributing hard disks and DVDs on specific contexts.
3. Wide-area network acceleration (EdgeXL, Riverbed) perform duplicate traffic elimination using block Rabin fingerprints to identify arbitrary length duplicates on the fly.
4. Existing web proxies do not scale for TB caches. HashCache presents a modified memory indexing system to make web proxies scale to support TB caches. We are also working on new algorithms to support fast local search on TB caches.
5. There are several works which have performed on-the-fly content adaptation to reduce the size of web pages to enhance web browsing experience in low bandwidth networks. Given the widespread prevalence of Web 2.0, existing web pages are no more plain HTML and there are many unaddressed challenges in content adaptation.

2.3 Authentication and Identity Management

Goal: Authentication is a fundamental problem that arises in various contexts in developing countries. We need low-cost usable authentication mechanisms appropriate for developing regions. Paper is often the common medium for transactions in these settings and paper forgery is among the leading causes of corruption in many developing regions. Our goal is to develop a low-cost authentication mechanism to verify the authenticity of any piece of paper.

Existing works: Paper authentication, Speech based authentication, biometrics, smart cards.

Research Contributions:

1. We have developed, PaperSpeckle, a low-cost, robust, tamper-resistant paper watermarking technique that extracts a unique watermark for any piece of paper based on the natural randomness present in the structure of the paper. We also show that this verification can be done using a simple mobile device. We show a theoretical result that establishes the hardness in forging the authentication mechanism.
2. We are working a combinational protocol of using speech-based authentication in conjunction with challenge-response based cryptographic techniques to develop a usable authentication mechanism for branchless banking.

Applications: Stamp paper verification, Drug counterfeiting, currency notes, supply chain management.

Identity Management: Closely related to authentication, the notion of identity in most application scenarios in developing countries is very weak and it is significantly hard to overhaul the entire system with a new identity management system. The goal is to develop a scalable and sustainable identity management system suited for developing regions. Cryptographic solutions are not the easiest to deploy since such solutions are not easily usable. Biometrics are possible options but they require a large manual setup process. We are investigating one solution based on leveraging SIM cards as a trusted computing base to bootstrap an identity management system for developing regions.

2.4 Mobile Apps for Developing Regions

Goal: Develop new viable mobile applications that can enable a new class of services in health care, financial services and other areas. While mobile devices are touted to solve a large number of problems in developing regions, there are some important limitations to understand. First, not every one has a smart phone or a phone with a data connection plan. If restricted to only voice and SMS, the functionality is extremely constrained. Second, cellphone usage rates are extremely high and the application needs to be extremely curtailed in bandwidth usage. Third, we are the mercy of the operator for mass adoption which is not always desirable.

Challenges and Contributions:

1. Given that most users have low-end phones, one of our thrust areas is SMS-based mobile applications. We have currently built solutions to four problems: (a) SMS-based health records; (b) Secure drug tracking; (c) Mobile Craigslist; (d) Rural ATMs. Key ideas to enable these applications include semantic compression of updates, a lightweight reliability layer for SMS, supporting aggregation of operations and lightweight privacy and security.
2. TCP/IP is unsuitable for intermittent environments. While the DTN research community, tries to provide a TCP/IP semantics over delay tolerant links, often hiding the intermittency from the application is not a good idea. Given that most applications operate in synchronous environments over TCP/IP, tailoring these applications to intermittent environments is often a difficult challenge. Some traditional synchronous applications like ATM machines need to be fundamentally redesigned when tailored for these asynchronous intermittent environments. To achieve this our rural ATM system supports offline authentication along with an approximate consensus protocol to support cash availability.
3. Mobile platforms need to be extensible and should support easy installation of new applications without operator need or involvement. One possibility we are investigating is to build a class of applications on a common platform, where the application themselves are instantiations of the platform specified as configuration parameters to the underlying platform (like creating a web page). We are currently working towards developing an extensible platform to support a wide range of SMS-based apps.
4. As an alternate to GPRS, we are developing a transport layer for Data over GSM to achieve 2-2.5 Kbps on the voice channel. We can currently achieve close to 2.5 Kbps with a 10% loss rate on the voice channel. This represents a low-bandwidth and low-cost alternative to GPRS.

2.5 Other Important Research Challenges

To enumerate a few other important challenges:

1. **Appropriate user interface design:** A large fraction of the development-targeted population is either semi-literate or illiterate. The user interface is the single deciding factor on whether a system is deemed usable and is widely used by local populations. There are several HCI and UI design challenges in the ICTD space that warrant careful consideration

and there is a sub-community within ICTD studying this space.

2. **Local language challenges:** Each developing region has their own local language with its own spoken and written dialects. This opens up many research challenges to both the speech and NLP research communities. Two of the biggest challenges in local language are speech recognition and content generation, translation and presentation. Addressing either or both of these issues is critical to make systems usable in the local language.
3. **Low, unreliable and intermittent power:** Systems should be reliable and robust in the face of unreliable and intermittent power. Designing low-power computing systems is currently receiving a lot of attention within the CS systems community (HotPower workshop) but much of their focus is on power issues in the developed world where they assume the presence of a sustained reliable power source. Power challenges in the developing world are technically harder to address with lack of reliable power sources and significant power fluctuations. We need to draw this low-power research community to address power design challenges in the developing world.
4. **Computer vision challenges:** We recently developed an automated diabetic retinopathy system that uses machine learning techniques to detect diabetic retinopathy disorders in retinal images. This turns out to be fairly hard image processing and vision problem and existing solutions do a poor job of solving this problem. Our system has high specificity with low false positives and false negatives and improves over prior detection systems. We are deploying our system in Aravind Eye Hospitals where we believe this solution can significantly alleviate their diagnosis burden on 2 million patients tested for diabetic retinopathy every year.

3. SUMMARY

To end on a positive note, there is a wealth of really hard technical challenges in the ICTD space that the CS community should jump on. The beauty is the breadth of challenges in this space covering different disciplines within computer science with significant depth in each area. We, as a community, should be able to better articulate these challenges to both the CS and the development communities at large. I have tried my best to begin articulating a few of these technical challenges in greater detail.

Why it is Hard to Identify Technical Research Problems in ICT4D and How to Make it Easier

William Thies
Microsoft Research India
thies@microsoft.com

ABSTRACT

My position is that a shortage of detailed and compelling problem statements is the primary bottleneck that prevents most computer scientists from conducting research in ICT4D. While interesting problems exist, they are usually discovered via months of fieldwork, and there is little incentive to formalize and disseminate problems for the benefit of other researchers. To address this bottleneck, I argue that we should create a prestigious venue for publishing problem descriptions, rather than problem solutions. I also propose that we establish problem-exchange websites to solicit problems from practitioners; organize structured design contests that aggregate knowledge in a problem area; and leverage the domain knowledge of funding agencies in defining technical research problems.

1. INTRODUCTION

I believe that the primary barrier to the growth of ICT4D as an academic discipline within the field of computer science is that the burden on researchers is too high: they need to not only devise an innovative technical system, but also to understand a social problem to a depth that is not easily attainable. To lower the entry barrier to ICT4D research, I propose four mechanisms by which we can create and share detailed problem descriptions that computer scientists can use as a starting point for their research: 1) a publication venue that is devoted to problems rather than solutions, 2) a website that solicits problem descriptions from practitioners, 3) structured design contests that engage students and researchers in building knowledge around a problem, and 4) relationships with funding agencies that leverage their problem-specific knowledge.

My position stems from the observation that there is no shortage of computer scientists that are interested in working on technical problems that could impact global development. Almost everyone supports the mission and desires to get involved; however, they do not know what they could contribute within the realm of their technical expertise. Those that are skeptical of the research area are not (as a general rule) doubting the technical prowess of the researchers involved, but rather the technical depth and potential impact of the problems that they are working on.

2. WHY IT IS HARD

While formulating an interesting research problem is challenging in any field, it is unusually challenging in ICT4D because the problems addressed are not ones that are seen on a daily basis by researchers in high-income countries. In ICT4D, researchers are often trying to understand the problems that affect a very different population: one that lives in a different place, embraces a different culture, speaks a different language, and is subject to different economic, social, and technical constraints. While many problems in computer science are inspired by our daily experience in high-income countries – or at least are informed by colleagues elsewhere on campus (e.g., the department of biology) – the problems in ICT4D may never come up in our daily lives, or even in our newspapers or Internet browsers. More often, they are discovered and understood via experience on the ground.

Currently, researchers in ICT4D face two basic options for discovering problems of interest: they can visit the field themselves, or they can dialogue with a community partner. Of course, they can also follow their own intuitions regarding the relevance of their technologies; however, such strategies can lead (at best) to their own wasted efforts, as their solution is not relevant or adopted, and (at worst) to bad outcomes, as donors follow mis-guided enthusiasm to scale-up inappropriate solutions.

Researchers who have the flexibility and commitment to spend time in developing regions have the advantage of seeing the ground realities for themselves, and for relying on their own instincts and abilities for uncovering interesting research problems. However, there are also many drawbacks. It is costly and logistically difficult for most academics to make regular trips to developing regions. It is also difficult to expose the underlying realities during short visits, both due to time constraints and due to local bias (showing important visitors the best side of every coin). Moreover, the social skills required for effective ethnography and fieldwork often have little overlap with the quiet competencies required of a computer scientist. Inability to communicate in the local language may also pose significant barriers.

The second option is to rely on a community partner to relay information to the researcher (perhaps in addition to their field visits). This has the advantages of leveraging more experience than researchers could ever accumulate themselves; it also offers benefits beyond problem identification, such as providing feedback on prototypes and possible deployment of solutions. However, there are also drawbacks of relying heavily on a community partner. It is rare to find strong partners who understand the scope and potential of computer science research, and are interested in thinking on long-term time scales that are necessary for fundamental research. Researchers may also be at the mercy of the judgment and insights of the partner; if partners are imprecise in formulating the problem, then the research may also suffer. Finally, it may not be sustainable for researchers to collaborate with community partners indefinitely, as partners can potentially lose interest after investing time with researchers who did not have the capacity (or good luck) to provide a working solution to their problems.

To compound the concerns above, even when one does succeed in identifying a technical research problem in ICT4D, it is often outside one's own area of technical expertise. For example, if a networking expert spends months in the field, they may discover a research opportunity, but in speech technologies rather than networking. This phenomenon also represents an opportunity, as researchers that do not plan to pursue a discovered problem may be more willing to share that problem with colleagues. All that remains is to incentivize researchers to invest the effort needed to rigorously define and share the problems that they discover – a goal we address in the next section.

3. HOW TO MAKE IT EASIER

I propose four new approaches for fostering the identification and sharing of compelling research problems in ICT4D.

1. Reward problem statements with publication. I propose that we solicit detailed, contextualized, and unbiased statements of technical problems in developing regions for publication in conferences and workshops. These can be invited both as full papers in existing conferences (ICTD, NSDR, etc.) or perhaps as a new workshop or online journal. The crucial role of such a venue is to reward researchers for disseminating the insights gleaned from fieldwork (and conversations with partners), whether or not they have an innovative technical solution to match.

For example, I could envision a paper that defines the problem of medication adherence: what are the reasons for non-adherence, what are the constraints of delivering and consuming medication in rural areas, and what is the role of stigma, incentives, costs, families, politics, geography, and other factors as they relate to possible technical interventions to improve adherence. Such a detailed description of the problem is more than enough to fill a paper, but yet is often the minimum knowledge needed to undertake a technical research agenda in this area.

Researchers will reap rich returns from publishing such knowledge, as follow-up work on actual solutions is likely to cite their problem statement. If the authors have also developed solutions to the problem, then the solution is still eligible for publication elsewhere. If the problem is beyond their expertise, then the authors still get credit for defining it. Devoting an entire paper to the problem itself also encourages a rigorous an unbiased formulation, with less incentive to skew the description to match a particular solution. Other researchers – both inside and outside of ICT4D – would benefit greatly by having a single resource to consult for a breakdown of interesting problems in the field.

Publication of problem descriptions in ICT4D is analogous to publication of benchmark descriptions in computer architecture. Conferences such as the IEEE International Symposium on Workload Characterization are devoted to the subject.

2. Maintain a website of open problems. This website would provide a more informal and evolving portal to the same information contained in the publications above. However, as opposed to descriptions from researchers, the website would also solicit entries from practitioners, who often understand a problem deeply but are unable to cast it in terms that are interesting and appropriate for academic computer scientists. Such entries would be constructed with the assistance of a group of moderators, which would be drawn from respected researchers in the field.

As an example, consider a problem posed to us by Operation ASHA, a highly effective tuberculosis program that operates in New Delhi, India. They are seeking a means to reliably authenticate that a health worker and patient interacted at a given time. The solution must be low cost, as reliable as biometrics, and must offer timely notification (within a few hours) of each interaction. There are many additional parameters and constraints that deepens the problem definition. A range of solutions are possible, and are the subject of upcoming research.

In addition to the problem statement, the site would include updates regarding technical progress made on each challenge, as well as new demands and constraints from partners in the field. Such a resource would serve not only computer scientists who are looking for problems, but also for development agencies that are looking for solutions. A discussion board between problem solvers and solution seekers may also prove beneficial.

[1] http://maven.smith.edu/~orourke/TOPP/

This site would be analogous to the Open Problems Project[1] for computational geometry. It also bears some similarity to ThinkCycle.org (no longer available online), though with an emphasis on research problems rather than engineering design projects.

3. Organize structured design contests for students and researchers. I consider a *structured* design contest to be one in which all participants work on the same problem, and the organizers provide rich background materials that describe the context and constraints. In a classroom setting, a structured design contest has a place in any class that designs computing systems for the developing world. Such classes typically assign each student group to a different project, involving a large number of community partners and also burdening the students with finding a meaningful problem. However, there are many benefits to assigning the same problem to all of the students. The staff can invest deeply in building a knowledge base around the real constraints of the problem, including multiple perspectives from guest lecturers or from different community partners. Students can benefit by seeing others' approach to the problem. Also, partners benefit by choosing the best solution from the class, rather than bearing the risk of working with a single student team who might fail to deliver a working solution.

An example of a structured design contest is the Yunus Challenge to Alleviate Poverty, which is held at the Massachusetts Institute of Technology. Every year, students across campus can submit solutions to a single problem, in consultation with community partners, scientific experts, and other resources that are made available by the staff. Topics to date have included affordable small-scale energy storage systems, improving indoor cooking stoves, and improving adherence to TB medications. The competition has led to several spinoffs, including a non-profit co-founded by the author (Innovators In Health).

Structured design contests also have a role in the research community. Examples in other fields include the Multimedia Grand Challenges[2], the Supercomputing Challenge[3], and the CHI Student Design Contest. I think the time is right for a design contest in the ICT4D space, with a dedicated track at a premier conference.

4. Leverage the expertise of funding agencies. In other areas of computer science, researchers leverage the applications expertise of funding agencies to ground and direct their research (e.g., the DARPA Grand Challenge). The potential benefits are perhaps higher in the ICT4D domain, as global foundations have rich resources and knowledge bases in areas of international development that can be used to identify interesting research problems.

An example of this direction comes in the form of open, structured design contests that have recently been organized by leading foundations. Last year, the Rockefeller Foundation partnered with Innocentive to offer a $40,000 award to the best design for a solar power device that reduces the risk of malaria. While funding at this level may not sustain an academic research program, the foundation also provided open guidance in the form of literature reviews and other pointers which could direct ongoing research in the area. Other challenges on the site include "improving banking processes in the developing world" and "solar-powered wireless routers".

Engaging funding agencies with respect to specific research challenges could not only help to identify relevant problems, but perhaps also increase the chances of sustained funding.

Acknowledgments. I thank Saman Amarasinghe, David Hutchful, Indrani Medhi, Thomas Smyth, Kentaro Toyama, and Julie Weber for helpful conversations.

[2] http://www.scils.rutgers.edu/conferences/mmchallenge/

[3] http://sc09.supercomputing.org/?pg=challenges.html

Finding the Right Analogy for ICT4D as a Subfield of Computer Science

Kentaro Toyama

***Abstract*—**In the quest to establish ICT4D as a recognized sub-area of research within technical computer science, one question that arises is what precedent best applies.

I argue that the best analogies are those applied sub-areas of computer science where computer science is clearly in service of another discipline, with bioinformatics being a key example. It is suggested that the technical ICT4D community try to understand how bioinformatics gained a respectable reputation within computer science, as a way to light the path for ICT4D. Among the issues to consider are faculty focus, publication outlets, unique technical challenges, and general public awareness.

On the other hand, while human-computer interaction research has a lot of kinship with ICT4D, there seems to be a critical reason why HCI is a weaker analogue for ICT4D: HCI work naturally applies to most other sub-areas of computer science, whereas ICT4D's own contributions are directed outwards with respect to the CS community.

I. Introduction

Research in "information and communication technologies for development" (ICT4D) has two characteristics that confound attempts to establish it as a legitimate, discrete field of academic inquiry. First, it is thoroughly interdisciplinary, and interdisciplinarity is often viewed suspiciously by academic disciplines that believe themselves to be pursuing "pure" research in a well-circumscribed field. Second, it is driven by application as the "4" in "ICT4D" clearly indicates, and in the hierarchy of the ivory tower, applied subjects are relegated to the lower levels. With these two counts against it, it's only natural that the field finds it difficult to gain a foothold within some departments.

Ironically, ICT4D suffers from this difficulty most severely within a field that has itself had a history of struggling to establish itself, namely computer science (CS). With its roots in Goedel's Incompleteness Theorem and the Church-Turing Thesis, computer science was for many decades considered a sub-area of mathematics, and applied mathematics, at that. Howard Aiken, inventor of the Mark I computer that is considered by many to be the first universal computer, faced tremendous challenges at Harvard University in establishing a department of computer science [1]. No doubt, detractors felt that computer science was too applied and too interdisciplinary to be elevated to its own field of study. Computer science came into its own only after a rise in the number of exceptional thinkers in the area, as well as with the dramatic growth in its own relevance to society in general.

The analogy to computer science is apt, but perhaps not one that computer-science departments themselves are willing to accept as a rationale for ICT4D. So, instead, it's worth looking for other analogies that are more relevant.

I propose that the most relevant analogies are those of areas such as bioinformatics or computational physics, where computer science is in service of the goals of another discipline. Bioinformatics, in particular, appears to have had much success becoming an "official" CS sub-area (For example, one book lists it as among 12 sub-areas of CS ; bioinformatics stands out as an applied CS area). These areas are interdisciplinary by definition, and they're also applied computer science. I'll also specifically suggest that while there are many parallels with the human-computer interaction (HCI) community, ICT4D is qualitatively different as a sub-area, and requires a different approach to acceptance.

II. Components of Success of Applied Areas in CS

There are several components to the success of applied fields in establishing themselves as viable sub-areas of computer science.

Faculty Focus on ICT4D

In the university context, what is ultimately required for a field to become viable is simply enough faculty conducting research in the area. With faculty involvement, there will be research, papers, workshops and conferences, support for graduate work (or at least, a determined effort to get it)… and with critical mass comes more faculty involvement, as faculty influence hiring decisions and departmental direction.

But, faculty focus is not only the end goal, but also one of the paths to legitimacy as an area. That is, with more faculty who declare themselves to be working on ICT4D, the goal of ICT4D as a mainstream computer-science research activity becomes closer.

What does it take to achieve this? Actually, it seems very simple: We want to encourage (1) more existing CS faculty to engage in ICT4D research; (2) more existing CS faculty to declare ICT4D as their only or primary area of research; and (3) more new faculty in ICT4D to be hired into CS departments.

Of these, (1) seems easiest to achieve through collaborative engagement. If more of us doing research in ICT4D engage other CS faculty, we can increase the number of faculty interested in the effort. Bioinformatics appears to have benefited greatly from significant collaborative pressure coming from the application domain: biologists wanted more input from computer scientists.

(2) would be great to see, especially among tenured faculty. In computational biology, tenured faculty like Martin Tompa at University of Washington, for example, effectively left behind their previous fields (e.g., algorithms and theory) to start research in what was still then a nascent field. In ICT4D, Eric Brewer has effectively done this at UC Berkeley, while

Kentaro Toyama is with Microsoft Research India, Bangalore, India. kentoy@microsoft.com

maintaining a foot in research in databases and Internet systems. The more tenured faculty "defect" to ICT4D, the more the field gains validity.

Hiring is perhaps the area in which we have the least control, because there's a chicken-and-egg challenge: Without the critical mass of ICT4D-sympathetic faculty, ICT4D faculty hires are less likely. To make progress, we have to go outside of universities…

Publication Outlets for ICT4D

For a subfield to be recognized, a close second to faculty involvement is a quality publication outlet for research articles. Technical ICT4D lacks a flagship publication (*Information Technologies for International Development*, perhaps the closest possibility, doesn't publish highly technical papers), and this is something that would be relatively easy to change.

Similarly, a high-caliber conference would also help, and again, there is no technical ICT4D conference. It's not entirely clear whether there is a critical mass of strong research to merit a conference, but workshops such as NSDR [4] have been successful, and special tracks at WWW [5] and CHI [6] have shown that there is interest in other CS communities.

Unique, Challenging Technical Research Problems

On the one hand, ICT4D offers a range of interesting, very unique research challenges. On the other hand, it's surprisingly difficult to find hard, technical problems that are unique to ICT4D – very often the technical challenges are generic CS research problems (e.g., better speech recognition), while the portion that is relevant for ICT4D is limited to a bit of adaptation (e.g., what's the best way to train speech recognition engines quickly in local dialects)? It's not that challenging technical problems don't exist in ICT4D, it's that they seem relatively few and far between, and they're often not very obvious.

Much of bioinformatics consists of string-matching, machine learning, and indexing problems that are well within the domain of other CS sub-areas. It's not obvious at first glance that there are unique problems posed by biology as a domain. Yet, perhaps because these problems are acknowledged to be difficult technical problems, the area has gained acceptance. It would be helpful to understand how bioinformatics was first perceived by conservative CS faculty in its early days.

Increased General Interest in ICT4D

There is a direct correlation between public awareness, industry growth, increased funding, curious students, and general interest with university research. Certainly this was true for both the growth of computer science and bioinformatics as research pursuits. In effect, if the need for applied research is great enough, universities are willing to meet it.

For ICT4D, it seems like we need a combination of ongoing PR about the field, to highlight successes in a way that it registers in the public mind.

Patience

Many of us feel the challenges of conducting ICT4D research today. However, technical ICT4D as a research area is still very young – in fact, apart from a handful of isolated projects, it's hard to argue that technical ICT4D research has been happening for more than six-seven years. Given such a short history, it might just be patience that is required to see the field gain recognition in CS departments.

According to Wikipedia, "bioinformatics" as a term was coined as early as 1978 [2], yet it hadn't blossomed as a field of research until relatively recently, with the mid-1990's just beginning to see widerspread interest. That's nearly twenty years from early conception to established research area.

III. Why ICT4D Differs from HCI

HCI is often invoked as a field that is similar to ICT4D and has also struggled to gain acceptance as a subfield of CS. HCI is certainly both interdisciplinary and applied, and there are many methodological similarities between the two fields. There is one crucial reason, however, why I don't believe HCI serves as a useful analogy for ICT4D.

The core issue is that HCI is applied back to computer science, and not necessarily to another discipline. It has relevance to most other subfields of computer science, since most subfields of computer science – including some very hard, technical areas – touch human users at some point. Software engineering benefits from an understanding of software developers. Database research considers issues of the database administrator. Compression performance is typically tied to human perception. And, so on – in fact, it's hard to think of a sub-area of computer science that couldn't benefit from HCI work and methodology. This kind of inward applicability isn't sufficient for acceptance as a sub-area, but it certainly adds to the case.

In contrast, ICT4D is defined by its domain, and its impact is generally restricted to the domain. Although new CS problems might be identified in ICT4D, ICT4D as a whole doesn't impact CS understanding as a whole. Machine learning might have valuable application to ICT4D, and might even find new research problems in development contexts, but it seems very unlikely that ICT4D research results will ever inform machine learning as a research endeavor.

References

[1] Cohen, I. B., and G. W. Welch (eds.). *Makin' Numbers: Howard Aiken and the Computer*. MIT Press. 1999.

[2] Dale, N. B., J. Lewis. *Computer Science Illuminated*. Pp. 25. Jones & Bartlett. 2006.

[3] Wikipedia article on Bioinformatics. http://en.wikipedia.org/wiki/Bioinformatics, May 2009.

[4] Workshop on Networked Systems for Developed Regions. http://www.dritte.org/nsdr08/, 2008.

[5] International World Wide Web Conference. http://www2007.org/prog-Papers.php, 2007.

[6] CHI 2009. http://www.chi2009.org/Attending/AdvanceProgram/wednesday.html, 2009.

CS Focus on Informal Economies

Anita Wasilewska
Stony Brook University
1428 Computer Science
Stony Brook, NY 11794
anita@cs.sunysb.edu

Jennifer Wong
Stony Brook University
1432 Computer Science
Stony Brook, NY 11794
jwong@cs.sunysb.edu

1. INTRODUCTION

Computer Science research for global and social development can be carried out at two levels of the economy on opposite ends of the spectrum. At the top of the spectrum are developed countries with access to state-of-the-art technology. Research here focuses on projects for using available resources to aid disabled members of the society. For example, the development of web access techniques for the blind [3]. At the other end of the spectrum are undeveloped countries with limited, if any, technology. The most dominant form of technology in these places is typically mobile phones. While it is socially important and research worth to focus innovation at the top end of the spectrum, we believe that focusing on the bottom of the spectrum, at informal economies in developing countries, poses larger computer science research challenges and will have much greater impact on global social development. Informal economies are predominantly associated with developing countries, however can also refer to immigrant communities who work in developed countries "off-the-books" or selling contraband. Incubator and mircoloan programs have been the predominate form of developmental and technical support for these communities. While many of these programs have proven to be beneficial they face two major drawbacks: typically there is no evaluation or feedback information obtained on the effectiveness or impact of the programs; and these programs only operate for limited time frames and provide no sustainability.

We believe the focus needs to be placed primarily at the lowest level, on the informal economies in developing countries. In these communities the technology and the knowledge of the users is at a much lower level, which creates larger computer science challenges. If these challenges can be addressed at this level, then the solutions, along with improved features, can be easily adapted to advanced informal economies with more technology resources and knowledge, such as immigrant communities in highly developed countries [5, 1]. We envision two "Grand Challenges" for informal economies. The first is social development using existing and limited resource mobile phone technology. The second challenge is the development of new algorithms and techniques for evaluation on existing and limited resource mobile phone technology. In addition, we believe that a number of computer science areas play important roles in enabling informal economy development at all levels of the spectrum.

2. MOBILES AND INFORMAL ECONOMIES

There are over 3.5 billion mobile phones in the world and they are proliferating at astounding rates across socio-economic and cultural boundaries and provide unprecedented opportunities for enabling social impact and technical activism. To most of the people in informal economies, the mobile phone is the dominate computing resource which they have access to and limited income to support. Knowledge of using the functionality of the mobile phones however must often be taught to the users. For example, Tostan ([4]) a US nongovernmental organization based in Senegal, West Africa dedicated to educating and empowering Africans who have had little or no access to formal schooling developed methodologies how to teach the rural communities how to use mobile phones and send SMS messages. Rafael Anta of IDB (Inter-American Development Bank, [2]) directs a project in South America that uses SMS translation hub to facilitate non-Spanish speaking rural population give access to only Spanish speaking doctors. While the mobile phone technology will see great advances in functionality in developed countries year to year, there is limited likelihood of these trickling to the informal economies at the same rate. In addition, even if these mobile phones were to be available the lack of infrastructure and user understanding/knowledge prevents their wide spread adaption. Also, many of the existing application on these platforms would reach beyond the needs and knowledge of the users. Based on this understanding, we believe that the community should focus research efforts on the development of algorithms and applications which can be supported and applied to the technology already within the communities. An additional supporting reason for this is the sustainability of the applications and technology. One of the main limitations of incubators is their lack of sustainability within the community. They bring technology, such as computers, and teach members to use it, however when the incubator term expires the technology leaves with it. By focusing on developing special mobile applications for existing mobile technology the users will continue to have access to the devices after assistance has come and gone. One of the main focuses for mobile application development should on the development of a uniform framework which enables the applications to be cross cultural and portable across languages and countries. Applications should have ease of portability between languages and more importantly target all levels of literacy. One possible approach is the development of natural language independent applications. In this way the application itself is applicable across countries, dialects, etc. The diversity of mobile platforms adds a level of complexity for developers, therefore the framework should facilitate ease of code portability and customization.

3. EVALUATION & FEEDBACK

Computer science is a major enabling unit in evaluation of technology and social impact of funded programs, incubators, and micro-loans. The structure, methods, technology and effectiveness of all social programs need to be well documented for funding agencies and for effectiveness evaluation of the global and social impact of the programs. These evaluation methods need to be incorporated into technology which will remain within the communities to ensure long term evaluation. Since mobiles are the main technology platform which is i) already in place, ii) already used and understood, iii) can remain after programs have completed, and iv) promote sustainability, they are an ideal platform for user feedback, data collection, and monitoring. Data mining is an area of research which is crucial for evaluation and feedback. We discuss the ideas and main challenges for data mining in the next section. Sustainability within the communities is also a key concern. Many programs last for only a pre-determined length. By performing on the ground education (methodology of teaching locals, train basic development and maintenance skills in the field) and by continuing communication and interaction with the users to gain trust and feedback, the programs can become sustainable over long-term. Additionally, by bringing skills and interaction to the communities we are furthering the social impact and advancement of the informal economy.

4. COMPUTER SCIENCE INVOLVEMENT

There are two major challenges for the CS community: to bring technology and advancements in all fields to the poorest people and to gain insights how to do everything we do on limited resources (mobile phones). Some of the main areas that we see as factors are data mining, human computer interaction, visualization and natural language processing to/from images. We briefly touch on two of these areas.

Data Mining Techniques can be used to learn patterns and rules governing the smallest and poorest of all informal economies. It can be performed on two different levels: individual (personal) and global (community). Applications on both levels represent a challenge. On the individual level, on standby device, the challenge is how to fully automatize the data mining process in order to build a fully automatic decision support system for the mobile user advising about patterns and rules of behavior emerging from their applications use (business, education etc). And it all has to be performed on limited hardware capacity. On the global (community) level the challenge is to find effective ways to collect the data from the users (via SMS?) encode Databases, develop methods for automatic categorization of the data for sending to central server, etc. On this level, as much as on the individual level data mining the process should be at least partially automatized. Applications can not be web-based due to limited Internet infrastructure, lack of computers, and lack of computer skills. And they can not be standard models already in use as thy may need to be developed to the specific needs and goals of informal economies.

Another main issue is the multiplicity of national dialects as well as duality of spoken and education languages, the low rates of literacy, and lack of schooling in underdeveloped and for immigrants in highly developed countries. It is hence important to consider technology development which works around these challenges. One approach is the development of visualization based natural language independent user interfaces. Visualization provides a key for creation of easy comprehensible icon based language for mobile applications. Such language makes possible for quite sophisticated applications to be used by people who are partially or fully illiterate, or can't understand an official language of the country. Such created iconic sets for languages and societies can also be applied to other fields of projects. With this idea comes a new challenges, such as how to define generic images for usage terms which are not common concepts or concepts in which the technology is trying to teach. For example, for a financial application what type of icons would describe the concepts of savings balance, income, and savings goal.

4.1 CS Design Problems

There are a number of major computer science design problems which must be addressed when working and developing technology and applications for mobile phones in low level informal economies. While this is an new area of research, to develop the area one must rethink and redesign all classical notions to function on limited resources and hardware of mobile phone. Moreover, because of specificity of the applications, new algorithms must also be invented and implemented. In essence, one of the problems is to explore and develop algorithms which address the concepts and functionalities we use today on hardware which has the capabilities of standard hardware from 5-10 years ago.

When considering data mining, for example, the limited resources raises a number of issues. For example, how to format collected information, what information to store, and how to collect it. SMS is currently the standard for collecting data from mobile phones, however it can be very expensive in developing countries. With this limitation, optimization approaches must be developed to efficiently collect relevant data. The main research question concerning data mining is how to effectively implement data mining algorithms on hardware with limited resources on independent phones. In addition, standard data mining techniques typically involve pre-processing steps performed with interaction from the designer. In this case, it is not be feasible. It becomes an open research question on how to do the entire data mining process effectively while being completely automated.

5. REFERENCES

[1] European microfinance network (EMN). `http://www.european-microfinance.org`.

[2] R. Anta. IDB inter-american developement bank. `http://www.bicusa.org`.

[3] Y. Borodin, J. Mahmud, I. V. Ramakrishnan, and A. Stent. The hearsay non-visual web browser. In *International cross-disciplinary conference on Web accessibility*, pages 128–129, 2007.

[4] G. Gillespie. TOSTAN. `http://www.tostan.org`.

[5] M. Nowak. *On ne prête pas qu'aux riches*. Editions J.-C. Lattès, 2005.