

How to define a research agenda in CS combining social usefulness and scientific relevance? Elements of thought

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I present here a personal perspective on how to possibly position one's research directions in Computer Science (CS) as a public researcher by best combining social usefulness and scientific relevance. I intend social usefulness as contributing to issues faced by certain groups in the society. I will relate an ongoing personal journey to address such a joint objective in three specific sections. In Sec. 1, I discuss main influencing factors – often left implicit, when one devises their research plans in early career stage. Sec. 2 discusses the main tensions we face when attempting to set a research agenda with non-epistemic values. Sec. 3 presents research projects aiming to realize the sought fruitful combination, and how ethical purposes can raise new scientific questions deemed worth tackling in CS research. I conclude by questioning how to structure groups around such research purposes. Along the way, I will refer to works in philosophy of sciences, including feminist philosophy of sciences, artificial intelligence and ethics.

1 What are the factors influencing our choices of research topics when working in a public institution?

The (possibly relative) freedom to choose our research topics is one distinctive property of working in a public university as a civil servant, even the more so in France under a legacy tenured status. Academic freedom is one cherished pillar of public research in universities. Yet, in my limited experience, this notion is more called upon in Social Sciences and Humanities (SSH) than in STEM disciplines. In early-stage career, the factors determining one's involvement in various CS research projects are often left implicit, and hence out of the entire control of the researcher: injunctions to work on industry-driven applications in the case of applied science, to contribute only in our field of methodological expertise (even if in hindsight many contributions seem incremental and not impacting), importance to seize collaboration opportunities likely to be quickly conducive of publications. Surely these factors are not free choices on the researcher's side, but are instead implicitly and externally structured and imposed, and partly internalized. This raises the two following questions.

Why do these factors generally remain implicit? Consequence of leaving them implicit, hence seemingly preventing the researcher to comprehend them as objects of thought, may lead to a sense of lack of agency in one's research trajectory, hence questioning one's usefulness as a researcher, with all possible mental health consequences we know. Reasons for leaving them implicit may not stem from a deliberate stance of the institutions, but rather from the consideration of a lack of scientific substance and usefulness of such questioning. Conducting such questioning scientifically – as this unconventional conference does – would indeed involve referring to works outside STEM, namely in social sciences in humanities (including philosophy and epistemology), and may be discounted as subjective and outside our entitled perimeter of thought, conveying a persistent and impoverishing hierarchy of knowledge and disciplinary exclusion analyzed by Raji et al. [1].

What is considered a legitimate choice factor in CS? For the institution in its broader sense (funding agencies, research community, evaluation instances), whose point of view is conveyed more or less explicitly, some driving factors to set our research agenda may be more legitimate than others. Specifically in the applied sciences, when reflecting on the end goals, one may perceive that applications are ranked according to untold values: working to improve industrial applications is not questioned in the same way as working as a CS researcher with SSH, where actual CS contributions are more prone to be questioned.

2 What is doing good research in CS?

There is fortunately not a single answer to this vast question, but this is in the end the question one may arrive at after a few years of research career, wondering why it has not been the question they started from. First, let us

mention that determining what are valuable applications or purposes of our research is fraught with non-epistemic (moral or economic) values, a number of which being person-dependent but in my point of view, important to make explicit, at least to oneself and possibly in one's contributions as well. Second, whether science must be carried out independently from societal purposes to be useful has been synthetically discussed by Ruphy [2]: purity and objectivity of science are a long-gone idea in philosophy and sociology of sciences. As summarized by Ruphy [3] discussing works by Helen Longino on how the feminist philosophy of sciences can help build a perspective on the roles of values in science, the role of non-epistemic values on social or natural sciences should be made explicit, chosen and analyzed to unveil ideology and biases (such as sexist biases) in the scientific method, but also to identify an emancipating potential in science for, e.g., feminist ends.

Therefore, if we consider as Ruphy [2] that defining our scientific priorities according to internal disciplinary considerations only would be unlikely to respond effectively to societal expectations, and hence that expressing moral values to set our CS research purposes is necessary, then we are faced with a major question: *what precise social purpose to pursue, where our work could make a difference, and which raises scientific challenges for CS (and hence would not be deemed a mere application)?*

This is a question even more difficult to answer that it requires exiting one's comfort zone on two levels: (i) being most relevant to social problems we care about may often require to gain expertise in a wider set of sub-fields of CS; (ii) it requires almost systematically involving other sciences, experts of the field of the chosen purpose: being it sociologists or psychologists for, e.g., feminist purposes, or also geographers or environment scientists for environmental purposes. It therefore requires humility towards other sciences, external knowledge and will to envision the potential but also the limits of the computational approaches we can contribute. Approaching such ambitious goals hence requires thoughtful planning, which is sometimes alluded to (e.g., [4]). Negotiating possibly contradicting values (like working on model debiasing at a high environmental cost) is another challenge.

3 Examples of CS projects designed for social usefulness and scientific ambition

This required multidisciplinary has been discussed in the field of AI, for example by Raji et al. [1] who draw the parallel with the climate change crisis, which, as a global and complex problem, requires plural solutions. Similarly, as CS and AI have an increasing global impact on our lives, plural perspectives must be considered to avoid socio-technical blindness. Questions of how to position efficient approaches in CS research in regards of the climate crisis has been discussed in [5]. In AI, several recent works such as [6, 7, 8] have exhibited the political acts often left implicit in making a dataset. Part of the AI community has been therefore calling for rigorously documenting the creation process of datasets and models to make all motivating values and conditions of design and production explicit, and hence progress towards artifact auditability. Making the motivating values explicit in AI has been specifically carried by T. Gebru and E. Denton, stating that technology is inherently political [9].

In this context, fundamental scientific challenges raised by ethical objectives are, e.g., how to address the encoding of human biases in current AI models [10], or tackling important inequalities problems and identify the limits of current data-driven approaches [11]. The French ANR project TRACTIVE [12] has a strong social motivation, that of exposing gender biases in film, yielding the scientific objective to characterize and quantify gender representation and women objectification in films. The scientific strategy brings together 3 laboratories in CS and 3 laboratories in SSH, working jointly to contribute to scientific challenges in each domain.

4 Conclusion

In this abstract, I wanted to discuss some main existential questions for a public researcher aiming to contribute directly to deliberately chosen social problems. Devising a rigorous and efficient approach to realize the sought alignment between scientific relevance and social usefulness is not trivial. Also, an important parameter is professional age: focusing one's energy on less conventional purposes is more challenging for young colleagues submitted to classical evaluation criteria. Such approaches may remain limited in the number of persons involved (individual research agenda) or limited in time (ANR projects). To make them durable, it is important to structure group activities around them (see, e.g., [13]), which however questions more deeply the acceptability of the approach in institutional scientific policy often geared towards more economic innovations in CS.

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