Social Engineering and Self-reflexivity. Rejoinder to Arne Tostensen

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In *Forum for Development Studies* 1998: 2, I wrote an article entitled 'Some Notes on Development Research and Ethics'. In this number (1999: 1) Arne Tostensen, former Director at Chr. Michelsen Institute (CMI), Norway's biggest and most prominent institute for commissioned research on development issues, presents a response. This is an overdue and most welcome discussion.

I will use Tostensen's article as a point of departure to try to elaborate further on the more general analysis presented in my article. I will therefore leave it to the reader to judge whether he is right when he asserts that I, in the article, pose as a 'purist', being opposed to aid institutions funding research; that I dismiss concepts like 'Western ideas'; that it contains 'sweeping generalisations'; that I think evaluation studies are dirty businesses and that most of the institute sector is in the pocket of the powers that be. Instead of doing what is very tempting indeed – to discuss how such misreadings are possible – I will focus on those of Tostensen's arguments that deal with issues of more principal interest in this connection.

I will argue that they represent and express conventional and unfruitful views on researchers' roles and ethical challenges. They are based on outdated assumptions about the *a priori* existence of clear and rigid *role* differences between researchers on the one hand and aid bureaucrats and politicians on the other hand. I think that Tostensen's views as presented in the response are precisely those which impede that kind of self-reflexivity I think is a precondition for making social science possible at all.[1]

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Tostensen agrees with my main empirical observation: there has been and is very little 'ethical reflection within the development research *community* about the whole issue of the relationship between science and politics and power' (p. 213, my italics). Or to be more precise: he agrees there has been little reflection about this issue in *written form*. He is consoled by the fact that, according to his experience, the individual researcher in his dealings with bureaucrats and politicians wage a 'continuous struggle over the professional autonomy exercised by researchers'. I do not quite understand what he means by the expression 'professional autonomy exercised by researchers',

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1. The sociologist Bourdieu makes this point for social science in general and sociology in particular. See, for example Bourdieu (1990) and Wacquant (1987). Due to the very short history of development research as compared to the history of social science in general, and due to the field's comparatively rapid institutional growth, it is even more important to historicise the rationality and the conceptual traditions of this research field.

since autonomy does not exist as a fixed category and cannot therefore simply be exercised as if it is an attribute to being a researcher. But if he wants to argue that many individual researchers fight to maintain different forms of autonomy *vis-à-vis* aid bureaucracies, he might be correct. Nobody knows, and it will be very difficult to ascertain in any precise way. What we can explore, however, and what is easy to study and where assumptions can be falsified, is what goes on within the research community.

I maintain my proposition: There has been virtually no debate, whether written or oral, in the research community on the issues raised in my article. Few articles, reports or working papers have discussed the issue during the last two decades. And very few seminars or discussions have been held on the issue. When the Norwegian Association for Development Research in 1998 organised a conference on Ethics and Development, a number of important ethical issues were brought up in the invitation to the seminar, but mostly issues belonging to the world of politics. The most crucial one for researchers – what are and how to maintain the core value codes that one might say distinguish the research system from other systems and activities – was neglected. That was the immediate background for my article, which originally was given as a paper at the conference. I do not think that Tostensen can dispute my assertion based on personal experiences either. I would suggest that there have been close to a thousand seminars at the CMI while Tostensen has been working there, but very few, if any, about these issues. This is typical for the situation in general, and it is also very easy to understand, since very few researchers have been concerned with this topic. Since a research community fruitfully can be understood as the communication that constitutes and maintains it, the communication among researchers and between the research system and other systems it interacts with, is *the* important issue. The character of this communication, or discourse if you like, is I think also of importance when it comes to the individual researcher's ability to exercise self-reflection and autonomy. It is not difficult to envisage practical Utopias, or research communities, that are very different from this situation.

Distinctive ethical values or different roles

The crux of the matter as seen from my point of view is revealed unwittingly by Tostensen. He states that researchers are involved in 'knowledge production'. When they 'go beyond this vocation, they enter politics and cease to be researchers' (p. 136). Although this way of thinking is popular and influences research policy and research policy debates, it is not fruitful – if the aim is to maintain autonomy and freedom or to stimulate self-reflexivity in the context of individual research efforts.

1. Researchers are not simply involved in some sort of value-free 'knowledge production'. The never-ending discussions among researchers about 'The Fall of the Roman Empire', the 'Background to the Industrial Revolution' or how to define 'the state', 'rights', etc., demonstrate this. Development researchers suggesting development strategies, assessing how democratic institutions are built or how non-

governmental organisations work definitely do not deal with value-free knowledge production (just ask the governments in Zambia and Korea, or the poor peasants in Bangladesh or the Shilluk in Southern Sudan).[2] Tostensen suggests a way out. If two researchers writing the same report take opposing policy positions with respect to design or implementation of an aid project, then the 'aggregate effect may render the multitude of positions neutral', he argues (p. 138). This will imply that since many of the articles and reports produced by Tostensen (for example: 'Review of NORAD's research support', for NORAD, or 'The political economy of poverty reduction in Kenya', for SIDA) have been written by more than one researcher, his final reports are neutral. Within this 'neutrality' perspective, the need for self-reflexivity on how different communicative situations and various relations to funders and the topic affect the researcher and the research will be irrelevant.

2.   Politicians and aid bureaucrats are also involved in 'knowledge production'. Ministers of education initiate nationwide plans for knowledge production and knowledge dissemination. Aid bureaucrats and aid activists also deal with 'knowledge production'. They implement projects, based on former experiences or copying experiences of others, while hoping to contribute to practical knowledge of project implementation. Since value-free knowledge production does not exist, and also other social systems deal with knowledge production (the journalistic system, the political system, the football system; David Beckham performing his crosses, etc.), this cannot be the criteria on which the distinction between what is research and what is not research ought to be drawn.

Not only does Tostensen argue that the researcher *is* different because he is a 'knowledge producer' unlike other professions in society. His quotation from Alexander George suggests that he thinks that there exist other and very definite important role distinctions between researchers and non-researchers. George focuses on conventional arguments about the role differences between researchers and policy-makers, i.e. that 'the academic can operate in a more relaxed time frame' while the policy-maker 'must nearly always act with imperfect information, before a fully satisfactory analysis is complete'. As a general description of the research system and the world of politics or aid administration this is empirically incorrect and substantially uninteresting. Just to mention a few examples: In modern voluntary aid organisations there are, for example, employees with PhDs, who may concentrate their work and thoughts on the same region for years, dealing with the same few projects. Their time frame is often much more relaxed, and

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2.   I have elsewhere showed how many researchers were state activists in the 1950s and 1960s and NGO- and civil society activists in the 1980s and 1990s. The research tradition on NGOs in aid is a tradition in which fundamental concepts have not been thought through, and where, I suggest, hundreds of evaluation reports have reproduced the idea about the 'comparative advantages' of NGOs – at face value (see Tvedt, 1998).

their knowledge about both local setting and individual projects will generally be much more in-depth than that of a researcher 'passing by' (whether with a PhD or not), be it for a quick-and-dirty consultancy job lasting some few weeks or a research project requiring field studies over a year or two. And I am sure that also Tostensen, who has produced about one evaluation study per year for the last decade must have felt, like myself, that he (now and then, at least) put the final word to a report or an article, before a 'fully satisfactory analysis was complete'. There are of course differences between researchers and practitioners in this regard, or between aid organisations and research institutions, but such role differences are not so rigid as conceived (just think of the rapid growth in consultancy firms), and they do not provide a reason for the complacency on behalf of the researchers as Tostensen suggests.

If one accepts the existence of such rigid role differences, it is also natural that one will argue, as Tostensen does, that researchers by definition have relative comparative advantages in 'the policy making process'. A useful distinction, Tostensen argues, is that between 'diagnosis and prescription in policy making'. He wants to reinstate the researcher as a kind of Master of Diagnosis, i.e. as the crucial actor in the policy process. The researcher is, according to Tostensen, particularly skilled at performing 'policy analysis and diagnosis'. It would have been nice for us if he was right, but I do not believe that the implied systematic differences exist between different professions and I do not believe that this skill is automatically linked to the title of researcher (also Tostensen must have come across some of the very many people who call themselves and are called researchers by others, who operate as advocates for a particular product, be it for a certain medicine, or the usefulness of eating potatoes instead of pizza, to drink milk instead of beer, wine instead of cola, etc., or who send out prophesies every other day about how the economy is going up and down, how the stock market is developing, etc.). And neither do I believe that to conceive these role differences as real or having a practical and decisive impact on behaviour, or to maintain them as if they are important, are conducive to research.

To maintain these mechanical distinctions will not only make it more difficult to analyse and reflect on the complex research environment of today. It will also, if taken literally and seriously, lead to criticism of those researchers who, like Tostensen himself, do different things. I think it is worthwhile and ethically fully justifiable to move between different systems and between different types of research, and have tried to explore these issues both theoretically and methodologically in published articles, as Tostensen will know. For a researcher to turn politician may even be good for politics. And it may be good for the person's research, if he or she later returns to a research career. I have many places argued in favour of the fruitfulness of shifting positions, as a way to gain new insights and to detect more clearly the limits of one's own perspectives and to enable oneself to draw a distinct line between what is research and what is not research. The problem is not that people do different things. The problem is a research community where many researchers act more or less as politicians while they at the same time claim to be researchers, or believe that they do research when they do what

practitioners could have done just as well (if they had been asked). This practice disregards what should be the research system's distinctive core values – the concern about what is true/not true and what is good research/bad research, a concern that we as researchers universally can agree on and maintain as core values, provided there are some autonomy and freedom, without never coming to an agreement about what is true/not true or good research/bad research.

I think it is problematic when Tostensen legitimates development research by its usefulness for the aid authorities. I am not saying that development research should not be useful for aid authorities, or for others, for that matter, and that individual researchers may well have this as a motive for doing research. But if the same researcher is interested in acting ethically as a researcher, he or she ought to be concerned with how this affects the distinctive core values of research activities as compared to those values being distinctive for other activities in society. Let me take one example: Seen in a long historical perspective, and, let us say, from the standpoint of an African village or a bazaar in Islamabad, there can be no doubt that the development research community and the aid bureaucracy have cooperated to improve aid efforts and to make aid more efficient and beneficial. This does not, of course, imply that everybody agrees with each other or with the dominant aid policies at every point in time. My point was not that researchers take 'all cues from the aid authorities', as Tostensen asserts. Fredrik Barth, Johan Galtung, Gunnar Håland, Arne Martin Klausen, Tore Linné Eriksen, Olav Stokke, Kristi Anne Stølen, Mariken Vaa and Tostensen himself, just to mention a few, show that researchers dealing with development issues in Norway have been critical to both aid bureaucracies and to concrete development projects. My point is that to be a development researcher implies situating oneself in certain structural power relationships, belonging, for example, to a segment of the dominant civilisation in this civilisation's effort to change the world according to its script. These relations cannot be wished away. It becomes a problem for research ethics first when the potential and structural impact of these relations on one's own studies is not realised. The problems I raised were those which develop when researchers dealing with this type of 'policy analysis' and 'policy descriptions' are disinterested in or unable to reflect on the implications this structural situation will have on research ethics and autonomy – and if this kind of activity dominates the research community as a whole.

Aware of and critical towards that self-confidence which the assumption about inherent role differences will produce and reproduce, I tried to focus on a set of conflicting core values that should be the concern of all researchers, within, what certainly is a very heterogeneous community in most other respects. This is the more important since Tostensen legitimises development research as such according to the relevance the research has to the aid authorities.

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A central question or perhaps the most central question of normative ethics is to determine how basic moral standards are arrived at and justified. By focussing on a particular set of core values being constitutive for the research system as such, it might be possible to overcome a not very useful dichotomy in our context: that between theories that do appeal to value considerations in establishing ethical standards, and theories considering the goodness or value brought into being by actions as the principal criterion of their ethical value. My focus on ethical issues does not call for doing certain things on principle or because they are inherently right, and neither do I suggest that certain kinds of actions are right because of the goodness of their consequences. I think like this: A researcher does not carry a definite, rigid role, ascribing him definite abilities or characteristics. He may, as researcher, do (and ought to do) different things, like what is normally called basic research, commissioned studies, evaluations, etc. But these terms are problematic, not primarily because they are difficult to define, but because they are terms that are not directly linked to the individual research process, but basically external to it. They may be helpful terms within a political-bureaucratic system, but not within a research system, because they imply that there are different criteria about truth/not truth or good/bad research for different research activities.[3] The questions about what is right/wrong and what is good/bad research) should be just as important when carrying out an evaluation study as when doing basic research and vice versa, not because these values are good or universal (they are not), or because they have good consequences (bad or untrue research does not have good consequences), but because these are the questions we as researchers should ask if we want to maintain research as something else than politics, aid administration, etc. The situation now is that everybody seems to accept that one should be more 'liberal' when it comes to commissioned studies, because it is short-term, 'quick-and-dirty', etc. (I should like, one day, to do a historical analysis of concepts, methods and how ethical issues have been handled in such evaluation studies).[4] Instead of roles and different types of research, I will rather talk about ethical values and different communicative situations. I think this will be conducive for establishing what there is a dire need for: what may be called an ethics of resistance against the tendency for research to become politicised and bureaucratised and in very subtle ways affected by the conceptual power of the powers of the day, at the same time as we can continue to do all kinds of research activities. My suggestion in the article in

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3. Tostensen argues that I am 'spending a lot of effort in breaking down open doors. For instance, few development researchers would take issue with him when he, by way of introduction, states emphatically, that note must be taken of variations in moral systems across time and space' (p. 133). But Tostensen's article shows that he does not agree with me. My point was precisely what Tostensen neglects; the need to be aware of such differences within the same society. His article does not contain reflection on distinctive values of the the research community, only differences in roles. The door is not wide open. Tostensen's article argues that it should not be unlocked.

4. For a more thorough theoretical perspective on the history of Norwegian aid, see Tvedt (1999).

Forum was based upon the idea that development research as a distinct activity in society is becoming impossible if the research system's distinctive core values are not defended and discussed repeatedly, since this by implication will also mean stressing the need for relative autonomy and self-reflexivity, also for people acting as both researchers and social engineers.

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