

# **Democracy, Labour And Politics in Africa and Asia: Essays in honour of Bjorn Beckman**



Edited by  
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## OF CONCERNED SCHOLARSHIP

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What is concerned scholarship? We should start to formulate an answer ourselves - if only from a personal point of view, which is all I can do here. The honouring of Bjorn Beckman is an appropriate and challenging occasion for trying to get on with this task.

Bjorn was involved, from the outset, in *AKUT*, the Working-Group for the Study of Development Strategies, which grew out of a post-graduate seminar at the Uppsala Department of Political Science in 1969. It dissolved in 1993, when the Seminar for Development Studies was established in Uppsala and Bjorn Beckman started PODSU (The Politics of Development Group, Stockholm University)

The 'concerned scholarship' to which we were committed has generally been characterized by its critics. One way of reflecting upon the work of *AKUT* is to take our opponents claims as our point of departure. *AKUT* grew out of a concern with third world realities, but also out of a reaction against the dominating academic culture-the character of which was revealed at a seminar in Uppsala in 1976, following Bjorn Beckman's defence of his thesis.

The almost legendary Africanist, Professor Thomas I Hodgkin of Oxford, the opponent of Bjorn's thesis, had been invited to give a seminar. The hall was full of expectant members of one of the World's oldest and, in its own view, most prestigious departments of 'Government'. The appearance of the asthmatic professor in dishevelled gilled clothes was a bit odd, "but wasn't his wife a Nobel prize winner?"

**1. The following is only my own personal reading of some of the experiences related to our joint *AKUT* work. Inga Brandell and Lars Rudebeck have offered valuable comments. Yet I am sure that they as well as other friends have a lot to add, and revise!**

**2. For some basic documents on the *AKUT* Group, see *The AKUT-series* Nos. 1, 23, 27, 29, 37, 42, listed in the references, and the contributions to Rudebeck (1992).**

With the announcement of the theme of the talk – “When do scholars become revolutionary?” senior faculty members started to wriggle. And as Hodgkin moved on to the illustrative and exciting empirical case of nineteenth century Sudan, some simply left the hall.

This exemplified the lack of courtesy, blunt arrogance, and ignorant philistinism of the dominant academic culture with which we had to live at the time. Apparently, it applied not only to local dissidents and junior fellows but also to foreign celebrities. My doubts about the need for us to work separately withered away. We had to be autonomous. Not in order to build a new discipline and a new department with positions and professorships but simply, and perhaps naively, to be able to raise important but inopportune questions, even questions like when scholars become revolutionary, and to pay serious attention to 'strange' empirical cases - such as nineteenth century Sudan.

So *AKUT* was consolidated. While outlining our common field of interest, we did not formulate anything like a platform for concerned scholarship. That would have taken us into endless debates. Rather, we gave priority to our somewhat related individual projects and to joint seminars. It was left to our opponents to define us in more ideological terms.

## **IN DEFENCE OF *AKUT***

What did our opponents have to say? I recently had to go through personal archives from more than two decades with *AKUT*. As far I could see, the critics put forward five major allegations: (1) the guerrilla analogy; (2) the politicizing of academic positions; (3) the failure to separate scientific research and political sympathies; (4) not being in the mainstream of political science; (5) imposing a radical school of thought.

### **The Guerrilla Analogy**

Many critics, including friendly ones, repeatedly argued that, by forming *AKUT* as a semi-autonomous research group with its own external finance, we acted as extremist guerrillas, unnecessarily abandoning the people in the plains for the mountains. In this way, we even helped to sustain the conflicts and partially to isolate ourselves.

There is some truth to the latter points, which is why in 1993, when we were in a stronger position, we dissolved AKUT as a research group, while maintaining the network, and gave priority to work within established institutes. Looking back over the documents, it is clear that, in the mid-1970s, we either had to concede and retreat or to continue on our own. Individually we would probably have survived as black sheep within the same academic culture that gave Thomas Hodgkin such an illustrative reception. But that would have called for so many compromises and required such a lot of time that we would not have had space and money to read and discuss the texts, invite the colleagues, and carry out the empirically oriented fieldwork that formed the basis for our dissident work in the first place. By 1976, moreover, several years of mutual supervision had already created a rewarding mix of individual writings and joint work on the basis of equality that few of us were prepared to abandon for a career within mainstream hierarchies. To appropriate Virginia Woolf's metaphor, concerned scholarship, like women's writing, calls for a room of one's own.

### **The Politicization of Academic Positions**

Critics claimed that we were not really academics at all but used the university for political purposes and organized seminars and groups along political lines.

This is an illustrative misunderstanding. We were against the separation of professional academic work, from discussions about what subjects such work should focus upon. I shall return to that in the next section. But we were actually more anxious to uphold academic freedom than most of our opponents.

Of course there were some exceptions, from the notes of hundreds of seminars, I recall three in particular: a Philippine scholar cum rebel leader, José Maria Sison (whose scrupulousness I should have known about) was invited to give an intellectual seminar but turned it into a political lecture: and a certain Beckman turned parts of two seminars into eloquent political attacks against some friendly participants' ways of "indirectly supporting" structural adjustment and the regime of Daniel Arap Moi in Kenya respectively.

Usually it was the other way around. None of us would have been interested in academic positions if they had not come with an important degree of freedom. We not only supported academic freedom in principle but, being in a politically and economically weak position ourselves, also tried to make use of that freedom against the dominant political and economic powers and to establish the widest possible democratic space.

Politicization on our part we would have been devastating to us. Rather we had to be able to turn the accusation of politicization against the dominant academic groups, ridiculing colleagues' entrepreneurial ways of making use of positions, connections, fashionable theories, and timely Fields of interests. Gossiping about these practices with friends helped us to shape a wider common identity.

Not only where the arguments important, so was the way of spreading them. From the late-70's, I can still hear Bjorn Beckman carefully informing his children that their sorting and stapling on the kitchen table of piles of pages from a text of ours would not only render them some extra pocket money but also help us to oppose the commercialization of Swedish development aid.

Usually: it was less fun: like resisting in vain, in the late - 1970's and early 1980's, the increasing lack of independent critical analysis for the monthly solidarity-with-the-third-world magazine *Kommentar*; or later on criticizing the attempts to run the Nordic Institute of Asian Studies as an expanding country shop, adapting scholarly Scandinavian interest in Asia to political and economic markets in general and the European Community in particular.

For several of us, the liberal idea of academic freedom and the strategy of really taking it seriously was even a way of distancing ourselves from the organized radical politics that we otherwise supported. For instance, we enabled the Chilean Christian Left leader Licenciado Bosco Parra to continue (after Allende) his work with us in the most productive way he could think of, which was 'simply' concerned scholarship. I fondly remember the two of us (Catholic and agnostic) discovering, after many years, similar hidden passions for monasteries - as seemingly more serene and definitely more tastefully arranged and decorated places for reflection, concentration, and the upholding of ones integrity then their modern equivalents, the universities.

### **The Failure To Separate Scientific Research And Political Sympathies**

A common way of characterizing our work was that we were incapable of holding our scientific studies apart from our political sympathies. To some extent this was true and even deliberate. During the first and most important part of any intellectual process, that very mix was even the main point for us. In our view, for instance, distant colleague Bo Gustafsson's principle of separating his role as 'scientific' historian and Maoist-Stalinist activist was manipulative and unacceptable. Rather, we said, neither should be separated from the other! Maoism-Stalinism in particular should be criticized within social science. And

as long as priority was given to relevant problems and points of view there could be no such thing as 'pure science', only, at best, principled intellectual debate on what to give priority. One might say that we opposed the 'proletcult' thesis (proletarian or people's culture) and instead studied Peter Weiss' *Die Ästhetik des Widerstands* [The Aesthetic of Resistance] (1975, 1978. 1980)

We agreed. in other words, with mail\, mainstream political science colleagues about the primacy of formulating a clear and relevant question before turning to appropriate theories, methods and facts - only adding that the selection of a relevant question is a matter not only of what previous scientific results should be followed up but also of what is socially and politically important. Therefore, the selection of socially and politically relevant questions, must be subject to as much intellectual scrutiny as the identification of the previous scientific results. Just as the issue of what is scientifically important calls for free and open debate among the scholars, the problem of social and political relevance also calls for a serious, and even more free and open, intellectual debate on the basis of the primacy of the argument-if one is not to give free hands to politicians, administrators, and businessmen to dictate agenda. The identification of the relevant questions, is fundamental when one has to identify what theories and methods are appropriate and what materials one should enquire into. One should start with the questions, not with the tools, and turn to the people and the material in the field, not only to the libraries.

Hence, I still think that we were right to devote our 1983 conference among Swedish development researchers to the issue of social and political, not just scientific, relevance. At the time, proponents of conventional neo-classical economics and 'proper' political science tried to impose their own definition of 'pure' scientific quality against 'partisan and sloppy' third world studies. During the conference the researchers themselves - as well as the officers of the sponsoring Swedish Agency for Research Co-operation with Developing Countries (SAREC) - got involved in serious, and open debates on what were those labels all about, on what were the most important problems in the third world, on the capacity within the Swedish scholarly community to study them, and to which issues priority should be given.

**3. Prof. Bo Gustafsson himself later on seemed to have abandoned the idea, adding that he, as nouveau social democrat, thus felt more cast in one piece than before. See the interview with him, by Anu-Mai Köll (1980).**

*AKUT* itself was entirely dependent on funding from SAREC. All of us, not least Bjorn Beckman, nevertheless straightforwardly criticized SAREC for not paying sufficient attention to whether various applications for funds were relevant or not - on the basis of the politically-decided fundamental principles for Swedish development aid *and* intellectually concerned analyses by the scholars themselves of the context where the research would be carried out. Neo-classical economists, among others, and some leading officials with SAREC (and perhaps elsewhere) were not too happy with this. I know this, since I edited the 1983 conference report, which SAREC had commissioned and which, when finished, they acknowledged to be a correct account of the proceedings, but which they nevertheless refused to print and distribute. So we did it ourselves (*AKUT-Series* No. 29, 1984).

I am not arguing that all the analyses produced within the framework of *AKUT* were internationally pioneering or of the highest quality. They varied, of course. But this is an attempt to reflect on our way of working, not of reviewing the production. And once having settled on relevant questions, theories and facts, we were as 'boring' as anyone else when it came to strict and detailed application of the appropriate 'conventional' tools. In terms of basic quality there was not much too worry about as long as we were subject to assessment by established academics who branded us as 'radical leftists' and by radical third world practitioners who regarded us as 'too critical academics'.

### **Not being in the mainstream of political science**

Another half-truth is the allegation that we isolated ourselves from mainstream political science in terms of its meetings, magazines and debates.

Yes, to some extent we did. But there was no deliberate isolation. We tried to attend seminars and to follow relevant journals like *World Politics* or *Comparative Politics*. But when did the critics participate in politics and development-oriented seminars? When did they have a look at important journals like *Economic and Political Weekly*, *Review of African Political Economy* or *Third World Quarterly*? And when did they follow daily papers in one or two countries in addition to their own?

We did not give priority to the dominant version of political science, or even to its sub-discipline 'comparative politics'; instead, we tried to broaden it to do relevant, serious, and competent research on politics and third world development. In addition to 'ordinary' theories of politics and public policy, we need, then, to relate to theories of development and to acquire extensive empirical knowledge of various third world contexts. We also need to improve our understanding, through time-consuming co-operation with

colleagues in those settings and to gather information through real fieldwork and not just visits to conferences, offices, and hotels. So our mainstream critics will have to excuse us, but we have no reason to take seriously their allegation of us being improper political scientists.

### **The imposition of a radical school of thought**

On a superficial level this accusation is a bit flattering: that from a position of being almost thrown out of Uppsala University, we would have become influential enough to develop a dominant school of thought, monopolize seminars, let this influence our teaching and supervision, and build a Scandinavian and international network with closed-shop tendencies.

To begin with we have never been in a position to dominate. Our limited influence was primarily because others, till recently, did not do much serious work on politics and development. Our networks were developed in response to our lack of institutional support and the experiences of entrepreneurial colleagues, who made careers by setting up agreements between institutes around the world. We opted instead for personal contacts between equal scholars and practitioners with similar intellectual orientation and interests. And this was hardly a basis for dominance, only productive studies and friendship.

There were also inbuilt risks in this strategy of isolation and inbreeding. When Yusuf Bangura left a visiting fellowship with us for UNRISD [United Nations Research Institute for Social Development] in late 1989, he affectionately characterized *AKUT* as being more of a family than anything else. Probably he was right - and that was our main weakness. We turned into a generational network and an extended family more than an institutionalized research group and seminar. There was so much informal trust that formalized democracy did not always seem necessary. Only a few new members were recruited.

We tried to counter this already in 1982 when we formed a separate open seminar for supervision and discussion of current research. Between 1987 and 1993 we took full responsibility for the seminar ourselves - supplementing university money with our own external finding and free labour. The *AKUT* research group was separated from a new, regular and open *AKUT* seminar for discussion of ongoing work, supplementary supervision, and advanced courses.

The long list of the seminars and courses given does not signal any attempt to dominate the scene. Aside from discussing our own work in seminars with titles such as Bjorn Beckman's 'Whose democracy? Bourgeois versus popular democracy in Africa', we also examined those of critical colleagues', like Hans



Blomkvist's 'How to explain corruption and patron-client relationships in the third world?', penetrated the general discourse with invited lecturers such as Stefan de Vylder on 'Biases and distortions in socialist economic policies - some lessons from the third world', and enabled exciting third world scholars to present their work, like the late Said Chikhi on 'The role of labour and social movements in the process of democratisation in Algeria' or Partha N. Mukhetji on 'New social movements in India: the dynamic of class and ethnicity in nation building'. The courses stretched from different perspectives on 'Development, state and democracy' to more focused ones on 'Peasants and the political economy of agriculture' and 'Wage labour, workers, and industrialization'. But we gave it all a certain character. Opponents could keep on branding us unreliable rebels. It was only in the early 1990s, as we began to dissolve *AKUT* that new, and dynamic seminars could develop in Uppsala and Stockholm incorporating the research interests of younger scholars with the work of senior academics (now including ourselves).

The genuine problems of intellectual inbreeding arose only because the group turned into an extended family with too much 'social capital'. We did not develop a dominating school of thought that shaped our teaching and supervision.

It is true that Bjorn Beckman, many years ago, once wrote in a brochure that there was an '*AKUT* school of thought'. But he was terribly wrong! Several other group members energetically opposed this formulation, from very different points of view - unconsciously making it extra clear that we could not even agree on how to oppose what we were not.

I know from younger colleagues that this is the most difficult point to accept that there was no attempt within the group, let alone in external relations, to establish or propagate a unified theory, perspective or specific thematic or geographical focus. Even if somebody wanted to, he or she could not. We were individualists who generally did not write major pieces together, who applied different theories and methodologies, who focused on related but separate problems and who worked in very different geographical settings. And perhaps even more important: this in turn called for a kind of joint work which allowed for the diversity; a principled pluralism on the basis of mutual respect, primacy of the argument, comparative perspectives theoretically, thematically, and empirically- and almost always consensual decisions.

A senior research officer at SAREC once told me that the weakness of *AKUT* was proven by the fact that we never established a new school of thought in terms of a new, pioneering, grand theory. He was both right and

wrong. In terms of pioneering, scientific quality the group might have generated better products if we had been more focused. But then we would have had to impose theses, perspectives, and themes upon each other - and run the risk of disintegration. Our strength was rather manifested by the fact that our joint efforts survived for so many years, which was because we did not even try to establish such a school, or even, as a group boost a specific theory. We just tried to support each other's work and stimulate students by co-ordinating parts of our individual projects and practising concerned scholarship.

Nor did we try to propagate a certain school of thought in our teaching and supervision. Most courses were characterised by extensive reviews of the academic discourses. Some students complained; they would rather have liked us to tell 'the one and only true story'. Even some of our critics accused us of paying too much attention to different perspectives while themselves turning directly to their mainstream favourites. Some of us might also have preferred this: we would have liked in-depth studies of entire books rather than collections of extracted pieces to illustrate different perspectives. But this kind of pluralism was not only productive when approaching and introducing a theme, it was also necessary for us to be able to critically review mainstream perspectives *and* to present what we thought were more exciting ones, without being accused of trying to dominate.

### **ON REFLECTION**

It is currently popular to regret what one did in the 1960s and beyond. Having gone through the documentation related to our joint *AKUT* work, I do not feel we have to. (Our individual products will have to be evaluated separately.)

Sometimes we weathered the storms by merely pitching in rough seas - as an extended closed family with too much 'social capital' and too little institutionalised democracy. But usually, and more frequently over the years, we were reefing and beating away from the lee shores, light on the helm and with fair steerageway.

It was our way of doing things that mattered, not the forming and propagating of a particular school of thought. We were individualists with different theories, projects and empirical cases. What we had in common, was our reaction against the arrogance and ignorance of the dominant academic culture *and* a preoccupation with concerned scholarship in terms of focusing on questions which were not just scientifically but also socially and politically relevant - in their empirical contexts.

Most of the rest were only logical and practical consequences! We needed a room of our own, from where we could broaden our perspectives, establish our own network of equal scholars with similar interests, and give due importance to close empirical fieldwork. To handle our own differences of perspective, it was necessary to develop mutual respect, and make decisions by consensus. To both criticize mainstream approaches and to be able to present more exciting ones without being accused of trying to dominate, it was vital to be pluralist both in theory and in practice, and to review the whole range of relevant discourses in our teaching and supervision. To establish the widest possible democratic space, it was important to support the liberal ideology of academic freedom in principle and also to use it against the dominating political and economic powers and the radical politics of which we ourselves were otherwise supportive. To prevent politicians, administrators, and businessmen from deciding on their own what was politically and not just scientifically important to analyze, it was necessary to call for free and open intellectual debate on the basis of the primacy of the argument. There is still much to it.

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