

**THE POLITICAL ECONOMY OF TRANSITION:**

**Notes On Policy-Praxis-Research Interactions and Dialectics**

**By Reginald Herbold Green**

Pessimism of the intellect,  
Optimism of the will.

- Gramsci

Aye but a man's reach must exceed his grasp  
Or what's a heaven for?

- Barratt

We who are undefeated only because  
we have gone on trying.

- Eliot

Mega and Micro; Then, Now and Then

By its nature any brief paper on the interaction of policy, praxis and research in the context of Namibia's political economy of transition must be a tour d'horizon not an articulated agenda for action. The question is what type of tour (and what type of tour guide and tourist?).

There are several sets of interactions and tensions - probably best viewed as dialectical relationships and certainly as processes.

- a. Strategic issues (mega) and their elucidation/resolution are a precondition for setting up, articulating and following through on macro and micro ones. e.g. one cannot - beyond a point - define policies to be pursued relative to the EEC or to personpower development without facing what production structures and relationships are sought strategically.
- b. But without knowledge of what is and what is attainable at micro and sectoral level the mega (strategic) level will remain ill defined, full of gaps and non operational. e.g. the critique of existing historical materialist work on Namibia during the periods of pre-colonial neo-colonial and colonial occupations as too theoretical with inadequate empirical foundations and too speedily generalized from too few cases.
- c. To attempt to define and act on strategy or policy without knowing where one is and how one came to be there is rather like setting off for Windhoek without first determining whether one is east of Gobabis or south of Luderitz and whether one has to hand a functioning vehicle with petrol in the tank. Parallel considerations underly many of the issues dividing the 'dependency school' internally and from the other fractions of Marxian and/or structuralist analysis.
- d. On the other hand to concentrate solely on the present and its objective constraints - or on the historic process and the facetiousness of many past attempts to alter it by pure optimism of the intellect - can lead either to fatalism or to a view that only

very marginal willed change is ever possible. The 'new conventional wisdom' on Sub-Saharan Africa (Berg, Bank and Co. Ltd.) illustrates the second and the simpler variants of the Wallerstinian 'world systems' model the first.

Any practical resolution of these contradictions - which in both policy/praxis and research can be very antagonistic indeed - must be dialectic or iterative. Neither a simple Cartesian either/or (whether verbal, simple econometric or sophisticated computer modelled) nor a linear sequence (from any starting point) offers much hope of progress (in knowledge or of results) or at any rate not fast enough nor at a bearable enough cost to be anything less than appalling in the Namibian political economy of transition context. Academic and applied research mistakes like strategic and policy ones immiserize, repress and kill real human beings, and it is too easy for researchers and policy workers to overlook (or suppress in their own minds) this objective reality because they are rarely the victims most or most immediately affected.

#### Vantage Point

These notes are written from the vantage point of a generalist political economic policy and strategy advising and articulating civil servant albeit of one with (for better or for worse) an academic background and an analytical approach. Equally they are written from the point of view of a commitment to the political economic liberation of the people of Namibia and therefore to SWAPO.

This vantage point has - like any other - its limitations. Research with no very evident (or intended) operational significance cannot be central to agenda building from a strategy-policy-praxis viewpoint even if it is accepted that all additions to knowledge may have objective practical implications. If one believes that the prime purpose of understanding reality is to (know how one can) change it, research will tend to be priority ranked (often wrongly) in terms of its apparent direct relevance to achieving change - a system which raises problems in respect to at least some (clearly not all) historical research. Further research with claims to direct applicability which are clearly premature (e.g. a good deal of economic modelling) or objectively wrong (e.g. a good deal of "effective protection" and "(in)efficient import

substitution" analysis) arouse a degree of fury that relates not to their intellectual weaknesses (nor to how they might be overcome) but to the danger that their application will do very real harm. Research suggesting that nothing significant can be done at policy/strategy level (as opposed to identifying what can and what cannot) is - understandably - ignored or opposed, e.g. the variants of Nabudere's or Wallerstein's work suggesting that a Third World progressive policy analyst should move to the centre to cause change there and/or firmly avoid 'veritable transitory power' because it is premature to attempt serious change now and the attempt will in some objective sense 'make matters worse'. Finally research which is grounded in historic processes and present objective (including process) constraints, posits alternatives which are practicable once attained but notably fails to fill the gap between with much beyond 'optimism of the intellect' (e.g. much of Samir's work) has a fascination for the analytical strategy/policy worker but also arouses a sense of frustration more than challenge, because the gap is precisely in respect to that face of reality confronting him/her (albeit the political economic strategic and policy profession is, so male dominated him is almost literally accurate).

The importance of understanding this vantage point lies immediately in the fact that it informs this paper but more basically in that - usually not very explicitly put or reflected on - it informs relations (including clashes and non-communication) among the strategy-policy-praxis and academic research communities. (Whether all research is - or should be - academic and intellectual is a different topic. It does not appear to be on the agenda of this meeting which is clearly within the standard view of serious research as carried on by a self trained, defined and perpetuating elite or sub-class.)

#### Some Assumptions

Only strategy-policy-praxis agenda must be based on certain assumptions about present and future reality. Otherwise the number of variants expands so rapidly that it is neither intellectually, nor even physically, portable. Among the most important used here are:

1. Namibia will become independent within the foreseeable future (18-60 months). If not changes in objective reality will require a

complete rechecking of the agenda. e.g. the personpower situation is so different now from 1977 (in this case better) that certain constraints necessarily assumed then may not apply now;

2. There will be a negotiated - not a purely military transition. This is a necessary corollary of the first assumption but also has basic implications for the state of the productive base (material, personnel, institutions);
3. The government of independent Namibia will be formed by SWAPO. Or trivially I am not a logical discussant and much more fundamentally most of the assumed strategic goals need reexamination in terms of who the dominant coalition (whether in party, interest group or sub-class terms) is;
4. There will not be an active war on the Orange River border. The agenda for a siege economy with Oranjemund permanently non-operational would be quite different;
5. South Africa will continue to hold Walvis Bay after independence. Combined with the third assumption this puts creating (recreating) an alternative port at the top of the agenda (albeit the research implications do not flow primarily to that fraction of researchers - social science - represented here);
6. The territorial economy will be depressed but functioning. This follows from the first two assumptions and defines the broad productive constraints and possibilities - and to a degree priorities quite differently from those which would pertain were SWAPO to inherit either the 1977 colonial economy in full swing or an economic wasteland in which almost nothing was operating in a fashion amenable to normal economic analysis or management a la Uganda 1980);
7. There will be a rapid and significant exodus of Europeans especially among expatriates (as opposed to "Sud Westers"), Afrikaners and public sector personnel but not a near immediate tidal wave (i.e. qualitatively more, more rapidly than Zimbabwe

but less, less rapidly than Mozambique). The limits of what can and cannot be kept running and/or added and of what can/must be transformed (or abandoned) how and how fast depend crucially on this assumption.

8. The so-called 'external debt' issued by the AG will be repudiated. As the likely level of the paper issued will be at least equal to national product and the 'debt service' to half of exports and of domestic tax revenue this (or the reverse) assumption has massive external account and government budget 'room for manoeuvre' implications.
9. Relations with South Africa will be poor but with no immediate or total ruptive in economic relations. This would appear to be the necessary (on the poor) and probable (on the less than total break) result of assumptions 2, 3, 7 and 8.

The point about these assumptions is not whether I - or anyone else - likes them. It is that they are crucial to formulating a strategy/policy and an applied research agenda. If - to the extent - they are wrong, agenda revision is - will be - needed.

#### Action - Research: A Priority - Sequence View

The political economic transition in Namibia will be characterised by a very large number of actions which it is necessary to take - at some time - and even more which it would be desirable to take - at some time. It will be equally characterised by shortages of most resources - not least of data, analyses and analytical capacity and senior decision taken time - but not equal shortages of all and, indeed, surpluses of some (e.g. unskilled labour power in general, potentially at least, beef).

Therefore, identifying priorities and sequences will be critical as will be determining what scarce, relatively available and surplus resources an action will require. Top priority and urgency actions requiring scarce resources need to come first; scarce resource intensive actions which are critical but less urgent deferred; surplus resource using actions which raise relevant

production/incomes slotted in as early as their scarce resource demand component can be met.

Presumably similar considerations apply to research in its own right. However, the present concern is that to be informed action needs data and analytical bases. The costs of not having these vary widely. For many purposes the present territorial product (but not external accounts) data may be adequate as they are almost certainly as plausible and as plausibly estimated as those of a majority of SSA states (which may not be saying much!). The costs of using levelling, channelling and heavy machinery techniques on the Oshana country without prior geological, soil engineering and agro-economic research could be very different - hopelessly mixing good soil, sand and clay into an infertile conglomeration and cracking the hard pan over a salt lake creating a second Etosha Pan.

Five broad typological categories can be identified. The first is action which is both critical and urgent for which either adequate initial data to begin action exist or the most urgently needed research is comparative - not Namibian. Mining (once an analysis of personnel requirements, skill levels and present staff is completed) is an example of the first. Central Banking is an example of the second sub-category. Examination and analysis of what has been done, why, with what results in - say - Botswana, Zambia and Tanzania is more immediately action relevant than study and analysis of the history and present of the Reserve Bank of South Africa - let alone its Windhoek branch.

The second comprises action which is critical and likely to be urgent as soon as data and analysis are available to identify what can be organised and what sequences are practicable and time efficient. A key example of the first is historical and present marine and transport engineering research on Swakopmund and its vicinity oriented to identifying how, how fast, and at what cost an interim alternative main port can be put into operation (6 months or 16? \$25 or \$200 million?). A somewhat less critical area for research are the present irrigation programmes to see how they can be utilised which leads into the second sub-category in determining what expansion, transformation, modification and generalisation routes are both technically practicable and political economically consistent with SWAPO goals. A similar key area is urbanisation: how many households (however defined) will move to or be reunited where? What urban infrastructure (including housing) actions are

necessary? Socially acceptable? Political economically valid? Who should carry out which? How? Except for the harbour study these examples all involve both quite deep social science and quite specific technical research topics and are unlikely to be handled satisfactorily without an interactive alliance of both. (Research is not just social science research - as the makeup of this conference might suggest - nor is it plausible to suppose social science and technical research can be kept in separate boxes without very serious mismatch consequences.)

A third cluster include actions which cannot be defined properly for urgent implementation or cannot - however important - be scheduled until more data and analysis is available. The large ranching sector is an example of the first sub-category. Instant attention is needed but neither technically, organisationally nor in production relations terms can a practicable and desirable action programme be articulated on the basis of present data. The northern mixed farming zone (excluding, perhaps, the Okavango Valley) exemplifies the second sub-category. Far more data is needed before more than a quite limited range of actions on technology, productivity, production patterns and institutional/social relations topics can be defined (let alone taken) without high risk of catastrophic and potentially irretrievable mistakes. Of necessity action must be postponed but the same necessity argues that relevant research be begun yesterday to limit the delay before action can sensibly be taken (or sensible action be taken).

A fourth group comprises topics on which action is important but clearly must be deferred because other calls on the same scarce resources have higher priority. However, research - subject to the same caveat about greater sequential priority calls on capacity! - can and should begin earlier to improve the data and analysis base and to reduce the probability of mistakes when the sequential process puts these topics on the immediate action agenda. One example is commercial banking. Nationalism, Marxism and applied economic common sense all counsel against a system which is dominantly foreign, private and traditional. But the government budgetary process, the Central Bank and reconstruction of specialised public sector financial institutions will strain relevant decision taking, analytical and personnel capacity to (one may fear beyond) the breaking point. If these actions are botched, takeovers of or entries into commercial banking will be meaningless. If they succeed, the existing banks can - for a time - be controlled to some extent and more



permanent transformations determined and acted on later (say 3 to 7 years after independence). Tertiary education - except specialised research and training components which must be in Namibia, to produce Namibian specific data or skills, or to provide an interface with external (e.g. elsewhere in SADCC) research and training - is another such topic. The fact that a campus exists (the least critical input) is no reason to found a university at independence. As no SSA state has an even remotely satisfactory tertiary education sector (and the university proper is often the weakest component in terms of results but one dominating and corroding the sector) there is a strong case for research and reflection first quite apart from the personnel (student and staff) constraint.

The fifth category is one in which action is needed but nobody at present has any clear idea what it might be or even with what broad themes. An example is the status - role - participation - exploitation/subordination - liberation of women. Here the strategy-policy worker can, at most, put a very general agenda forward for research; the researcher (and a fortiori in the example the women) are likely to have much better intuitions and conceptualisations of both concrete and systemic/conceptual topics.

#### Action - Research: A Constraint-Requirement Optic

Action can also be typologized in terms of constraints and requirements viewed dialectically both in terms of their interaction at any one time and over time. This again requires research inputs. In the case of the Namibian political economy of transition in Namibia at least five such themes can be identified.

1. Keep the core of the economy (including public services) operating. Without a functioning Rossing, Oranjemund and Tsumeb there will not be the surplus, the tax base or the foreign exchange to do much of anything. Without much of the existing hospital/clinic network there will be no foundation from which to build comprehensive health services whether community centred or otherwise. Without a functioning power station and water purification plant in Windhoek the lights will go out and cholera come in. Without a functioning large scale ranching sector the livelihoods of up to 60,000

households will vanish.

Researchers - especially change oriented social science researchers - are often prone to forget or undervalue this constraint as are political leaders at (less so a few years after) independence. But those involved in action tend to draw the bounds of the core too widely and to pay too little attention to the longer term impact of means used, e.g. to keep European civil servants on present scales and to unify black salaries at those levels ensures either inability to finance necessary service expansion, near bankruptcy or traumatic cuts in citizen salaries a few years later (vide Zimbabwe's mix of all three). On these issues research is urgently needed in the Namibian context informed by comparative experience.

2. Restore key bits which have broken down. Much the same considerations apply as to the first theme.
  
3. Lay a basis for advance by initial additions to productive forces and services. The problem is not to devise a list but to select - "to plan is to choose" - a practicable initial short list. The author's would include personpower development (including national language, literacy and upgrading), water, urban family housing, crop production, the Trans Kalahari Railway, a mineral export company, import houses (realistically joint venture) with global sourcing capabilities.

Researchers are at least as good as strategy-policy workers at identifying areas for action and better at working through their implications. However, to be effective in influencing action they do need to pay more attention to priorities and sequences, e.g. not push rural electrification without explaining why they believe it ranks ahead of near completion of rural basic health - education - water - access to markets - production support services.

4. Identify key parametric (structural) constraints. A systematic, fairly simple model of an economy can allow identification first of what the constraints on growth, investment, external transactions, public expenditure are and - given reasonable assumptions - will

remain over a 3 to 7 year period. It can also help identify what sectoral disaggregations and questions need to be asked because they relate to key elements of overall constraints. A current exercise for Zimbabwe (involving a senior strategy-policy worker and a semi-outside researcher) has proved very interesting - not least in showing that certain seriously canvassed strategies/actions would be recipes for political economic suicide. A similar exercise for Namibia at or just after independence might be very valuable and would require a systematic research programme both as to data and as to analysis to allow a plausible model to be constructed or adapted.

The critical point about using this approach is to keep it dialectic. The constraints are the necessity that defines freedom of action in the short term - as researchers seem prone to underrecognize. But they are also the guide to the necessary action to ensure that there are greater degrees of freedom for years 8 - 15 viewed from year 7 than for years 1 - 7 viewed from year one, a point strategy-policy workers under heavy strain to avoid present breakdowns are prone to let "loose the name of action".

5. Identify a process/strategy of sequential change in an intended direction and how to keep it going

Strategy-policy workers and researchers alike are often too quick either to reject the Chinese proverb "Even the longest journey begins with the first step" or its implicit corollaries: the first step must be taken, and followed by more steps, overall in the right direction (even if probably not in a straight line) and with attention to seeing that early steps do not lead one to a point at which further steps are impossible. Removing de facto racial salary scales, setting new scales and using accelerated promotion constitute a good example of an area in which more thought, less blocages and even less haste, more speed apply and in which more case by case, conceptual and applied research is critical if Namibian decisions are to avoid a series of pitfalls.

In terms of processes and dynamics, researchers need to pay more

attention to what Chairman Mao called the "question of method". To analyze why it is critical to raise northern Namibian small agricultural unit productivity, incomes and control over production relations is a good deal easier (at some levels it is obvious) than it is to identify how. (That, in fact, is the basic flaw in Tanzanian agricultural strategy - and to all the alternatives proposed to date - especially in respect to productivity.) Strategy-policy workers need to pay more attention to the implications of means used to places reached - e.g. the present ranch support system in Namibia is remarkably technocratic, bureaucratic and paternalistic and, in the context of - say - worker self managed ranches less able to argue with it and technically more dependent on it than white ranchers, its maintenance and reinforcement on present lines would raise a series of basic questions more likely to occur to politicians and researchers and to be systematically addressed by the latter than by strategy-policy workers.

In dialectical terms the problem appears to be one of identifying immediate, critical, overcomable (secondary?) antagonistic contradictions and overcoming them in ways which raise (or make accessible) a further round of such contradictions and to do so in ways that do not pose too many or too basic contradictions before they can be overcome.

Most historical materialist research and discourse in Africa (whether by outsiders, semi-outsiders or Africans) has been singularly weak in respect to attention to the material basis of production, objective constraints, method and the process of change seen from a dialectic (rather than an apocalyptic or single leap) perspective.

#### Action - Research: Some (Sub) Class Approach Queries

Strategy-policy workers rarely formulate decisions and actions in articulated sub-class terms even if (as is usually not the case) they are acting on quite explicit political concerns/instructions and examination of alternatives/

results cast in those terms. The reasons appear to be twofold:

Tactically it is usually dangerous to specify a sub-class which is to be eroded or out-flanked unless it is isolated and weak (in which case the action may not be all that significant) or can be characterised (charicatured) so broadly and unfavourably that most members of the sub-class can deny (indeed often not percieve) they are targets. (e.g. Tanzania's erosion and eradication of the old co-op regional and national superstructure was basically an attack on the capitalist sub-class with the greatest potential to put together a serious capitalist road sub-class coalition. Precisely for that reason it was never publicly - and rarely privately - presented in those terms.)

Practically, research on sub-classes and their interests, perceptions and interactions is so patchy, problematic and arcane in form as to make even serious action analysis, much less public justification, in such terms very difficult at best and often impossible (e.g. Zimbabwe has systematically raised munimum wages to counter food subsidy reductions. But do "informal urban" incomes move parallel to the minimum wage as they appear to do in Tanzania but not in Kenya? Are the food deficit households in communal areas largely ones with an urban wage earner member or not? Until these questions can be answered the sub-class - as well as income distribution - implications of the present practice are obscure.)

From an action oriented point of view, the most immediately important research is not on the history/historic evolution - alteration of sub-classes in Namibia nor even in their present formations and interactions. There are likely to be very rapid changes - e.g. the present 40,000 odd black "salaried petty bourgeoisie" is too disparate in relation to production, attitudes to nationalism and relevance to independent Namibia and too recent a product of steps to "buy Bantustans", "placate nurses and teachers" and "buy off mine labour forces" for even a definitive present analysis to do more than form a base for ongoing research on its evolution, fragmentation, decomposition and alliance building after independence.

A number of "practical" questions relate to classes/sub-classes. For example what are the main types of household in the north, in urban areas, etc? Indeed have permanent households consisting of a woman and children become the

norm in some (or many) areas? What implications does this have for stated SWAPO, SWC and church policies on reuniting households? For urbanisation? For the status/liberation of women?

How do mixed "peasant/proletarian" households (assuming for the moment this is a sub-class of, rather than for, itself) perceive their interests? Why? What implications does this have for the - macro economically and, on the face of it, socially - logical strategy of seeking to have fewer, larger, more productive 'peasant' households on the one hand and reconstituted, complete wage earning households on the other? (In Zimbabwe this strategy is clearly meeting very substantial resistance on the part of the intended 'beneficiaries'.) What are the implications for land access rights? For creating production opportunities for women outside agriculture? For retirement security systems? For the relationships of women to production, decision taking and men more generally?

Strategy-policy workers simply will not (at least in the first years of Namibian independence) have the time to do the research necessary to answer these questions. (Nor will researchers if they insist on definitive answers before saying anything firm and policy oriented.) They very badly need (even if many, perhaps most, do not and will not realise it) collaboration with, and assistance by, researchers.

### Envoi

After 30 years of research (25 in or in relation to Sub-Saharan Africa, 9 in relation with Namibia - UNIN - SWAPO) and 20 of overlapping strategy-policy work I am not so foolish as to expect definitive solutions of the tensions let alone instant research work on all the Namibian topics and questions posed.

However, I do believe first steps can be taken and that it is imperative that they be taken now. Among those steps is a franker, more focussed dialogue among strategy-policy workers and researchers (as well as a broader and less monastic or elitist definition of who is a researcher and a clearer perception that human beings should be subjects not objects of research as of strategy, policy and praxis). Many present incomprehensions, lacks of fruitful interaction and mutual distrusts or denigrations (though by no means all)

could be reduced with more contact and dialogue and few can be in their absence.

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Reg Green has been a student of the political economy of Africa since 1959 and of that of Namibia since 1975, a strategy-policy practitioner on a semi-sustained basis since 1964 and a member or supporter of liberation support groups since 1955. While he is from time to time a consultant to SWAPO and to UNIN, the analysis presented and views expressed in this paper are his personal responsibility and are not necessarily those of SWAPO or of UNIN.