

The 'agrarian question' in the rear view mirror – some personal reflections¹

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Göran Djurfeldt, Lund University

After a long career in studying agricultural and rural development, in the following paper I take the opportunity to reflect on these issues in the rear view mirror. The advantage of this is of course that my biographical time span now covers a considerable historical one, starting with the apocalyptic alarms over the population bomb at the end of the 60s, especially in Asia, and the widespread fears of all-encompassing famines in India and elsewhere. To many of us who were then in development studies, Malthus, Ehrlich and the Club of Rome were soon overshadowed by the renaissance of Marxism and by the 'dependistas', in whose perspective development was a sham, and for whom the capitalist or imperialist peripheries were doomed to stagnation or even regression into increasing poverty.

Alongside with the radicalisation of development studies came the rediscovery of Chayanov by Shanin and others (Chayanov 1966). Thus the 'peasant' entered the scene, although I personally shied away from using the term, given its pejorative connotations, preferring instead to talk of smallholders or 'farmers'. The latter sounds very strange to American ears, where farms denote huge units. In the Marxist tradition of course, the agrarian question, first asked by Kautsky (1899), refers to same question as the one asked by Chayanov: How come 'peasants' seem so resilient in the confrontation with capitalist agriculture? Their persistence threw doubt upon the prognosis, primarily associated with Lenin, that capitalism in agriculture would imply big farms run by capitalists and manned by (landless) agricultural labourers, or *factories in the fields*.

In parallel, in agricultural economics, the peasant was rediscovered by scholars such as Schultz, Hayami and Ruttan, Johnston and Kilby and others (Schultz 1964, 1983; Hayami and Ruttan 1971). In a neo-classical framework, the 'peasant' stood out as a puzzle, because in his (or her) person were fused both a producer, a consumer and often also a worker, which created havoc for the neoclassical theory of farm production (Ellis 1988).

Postmodernists entered the scene only later, in the 80s, with their radical denial of the very concepts of 'development', 'modernity' and 'progress'. At that time those of us who worked on Asia became aware of what were and are much greater challenges to Euro- or Western-centric social science than postmodernism and, in the view of this author, its equally Western-centric offshoot, postcolonialism. Development did not exactly take the course foreseen by these schools of thought: So-called export-led growth pursued first by the Asian tigers, Taiwan, South Korea, Malaysia and Indonesia threw doubt, especially on the dependency school. When the tigers later were joined by the Chinese dragon and the Indian elephant and the centre of the world economy shifted to Asia and the Pacific Rim, dependency theories needed hard-handed massage to be made to fit the new

¹ This paper is dedicated to Staffan Lindberg my old friend and companion in development studies with whom I have shared not only field work but also theoretical concerns over a whole career. He is not responsible for my personal views in this paper, and I am sure we would not agree on a number of points I make here.

realities. Many of us, drew the conclusion, like Castells did, that new global realities needed new theories (especially Volume III of Castells 2000).

Many missed the essential point that structural transformation of agrarian society went before or in parallel with industrialisation in Asia. Structural transformation is the *alias* of the *Green Revolution*, radically misinterpreted by many students of development. The fact that the Green Revolution helped avert famine and once again postpone the Malthusian apocalypse threw far-reaching distrust not only upon the malthusians, but also on the 'dependistas'. Agrarian and industrial development in Asia rephrased the questions of development, agrarian as well as industrial, rural as well as urban, and recast them in a new global perspective.

In the following I will reflect on this, but from a personal and quite narrow angle: In the rear view, what is the significance of Kautsky's 'Die Agrarfrage'? Is it still worth asking the old questions about the peasantry, or is the agrarian question long ago resolved? Or perhaps, we need to rephrase the question? In working towards an answer, I will be shamelessly personal.

Encountering the world in Thaiyur

We landed in Thaiyur village, in Tamil Nadu, in August 1969. We like to think that we learnt to see the world from below during the ten months that we lived and worked in Thaiyur. We moved daily among the poorest of the poor, the landless coolies and salt-pan workers, the undignified *Paraiyan*, whose very name has given us the word *pariah*. Most of them led miserable lives, with daily wages hardly enough to provide them and their families with a daily ration of rice, and definitely not enough for luxuries like vegetables, or fruit, not to speak of eggs, milk or meat. We estimated the infant mortality during that year to 290 per 1000 born. Dismal reality!

After a year we returned to our privileged lives in a cooler place, preparing a career out of what we had learnt from the wretched Paraiyan and their masters, the *Vellala* and *Mudaliyar* of Tamil Nadu. We wrote two Ph.D theses together, one dealing with health and medical systems (Djurfeldt and Lindberg 1975b) and the other with the 'social formation' of Thaiyur (Djurfeldt and Lindberg 1975a). The latter gave much space to the analysis of rice production and how paddy and its cash equivalents were appropriated from the Paraiyan smallholders and went to feed landlords, usurers and merchants, as well siphoning of a surplus to feed the urban population. To our immense pride both books became mini-classics of Indian sociology.

When you land in a place like we did at Thaiyur, even if you are there long enough to learn the ins and outs of the place, you are not there long enough to observe or document *change*, even if that's what you came to study. Your study thus essentially becomes *cross-sectional*: it deals with a point or short span in time, while the wave-length of social change is usually longer than that. The *cross-sectional bias*, if we term it like that, is that you are tempted to generalise, not only in space from your little universe, *but also in time*. You can try to overcome the cross-sectional straitjacket by immersing yourself in historical studies to give depth to your explanation of why things *are like they are*. But that strategy in a way only reinforces the cross-sectional bias: The cross-section now appears as the outcome of a long process of historical change. Even with a measure of historical depth, you have no clue about what is to come; why the temptation is to freeze the cross-section and declare it as eternal. That's the great paradox of development studies: It studies change based

on cross-sectional data. Rigorous studies of change require pooled cross-sections or longitudinal data.

'Behind Poverty' failed in foreseeing the mechanisms of change making our 'ethnographic present', 1969-70, less longlived than we could anticipate. Imprisoned in the cross-sectional design and despite an inspired historical analysis, its concluding chapter declared 'development' a theatrical show and ended with the resigned assertion: "... despite all poverty and suffering, Thaiyur is a coherent and even harmonious formation: There are few dissidents, and few conflicting world views. This conclusion tallies well with our most frustrating experience in the field: There is so little which augurs revolt, or even a protest." (Djurfeldt and Lindberg 1975a, p. 316). True to the mood of the late 60s and the 70s, we saw only one mechanism of change: revolt or protest. Thus we missed the most important processes: the Green Revolution, the structural transformation and the various State interventions and human development they led to. One-eyed theories (and blue-eyed theorists) have to bear the blame for that.

Misconstruing the Green Revolution

We witnessed the farcical introduction of the Green Revolution in Chingleput District in Tamil Nadu in 1970. It was a top-down campaign, driven by an authoritarian administrative machine, insensitive to the feedback from the 'riceroots'. The latter was unequivocal: this new IR-8 rice is not acceptable to local farmers! The press reported complete success, but hardly anything was sown on the ground.

Susceptible both to over-generalise from a single case and victims of the cross-sectional bias, this experience made us join the critics. We saw the Green Revolution as a sham, at most beneficial to big farmers and landlords, with zero or negative trickle-down effects; its main effects were to speed up the proletarianisation of the smallholders, swell the number of landless and depress agricultural wages.

In this we were part of a movement. It was hip to be against the Green Revolution: for social reasons in those days, like for environmental ones today. Freebairn has an original take on this: He let his students in methodology classes go through and code more than 300 studies on the Green Revolution published between 1970 and 1989 (Freebairn 1995). In the only article published drawing on this material, he investigated how the conclusions on the distributional consequences of the Green Revolution co-varied with the regional origin of authors, location of the study area, methodology followed, and the geographic extension of the study area, from micro studies like our own in Thaiyur to regional studies of several Asian countries. To quote him: "the authors' conclusions on the question of whether income concentration increased depended on such structural and methodological characteristics as the regional origin of authors, location of the study area, methodology followed, and the geographic extension of the study area. For example, studies done by Western developed-country authors, those employing an essay approach, and those looking at a multicountry region are most likely to conclude that income inequalities increased. By contrast, work done by Asian-origin authors, with study areas located in India or the Philippines, and using the case method are more likely to conclude that increasing inequality is not associated with the new technology" (Freebairn 1995).

From the viewpoint of the sociology of knowledge, this is interesting, but if we are interested in the distributional consequences of the Green Revolution, and if we agree on some version of realist

epistemology, it is obviously unsatisfactory that research results depend upon factors like the geographical origin of the author, or on the type of method used. Since dissension among scientists cannot be resolved by voting, the fact that around 80 per cent of the studies coded by Freebairn and his students concluded that the distributional consequences were negative is a weak pointer to what prevailed on the ground. On the contrary, it points to the importance of ideological as well as methodological bias.

My own conclusion from the above is that development and agrarian studies need to get more sophisticated, methodologically as well as theoretically. Studies which aim at generalisation need to rely, either on population statistics, or on representative samples. If they aim to catch processes of change and their effects, they further need longitudinal data, pooled cross-sections or preferably panels.

Empirically testing Marxist hypotheses

In the rear view mirror it is thus easy to see how as a young scholar I and my colleague fell prey both to the fallacy of overgeneralising from a single case and to cross-sectional bias. Our next major study partly helped us to correct for that, as well as leading us into new traps.

Ten years after Thaiyur we did another major field work, in Tiruchy District, also in Tamil Nadu. The title of the project brought home the essential ideas: *Production Relations in Indian Agriculture*. We set ourselves the task to empirically test the Marxist ideas, current in the Indian debate in the 70s, about agricultural development. The main hypothesis was that production relations determine patterns of agricultural development. We drew much on the Indian debate and authors like Banaji (1977), Patnaik (1990) and others. More specifically we wanted to investigate the roles of landlordism and 'merchant-usurious capital' (Banaji's term) as barriers to the development of capitalist agriculture and as the key culprits preventing any benefits from agricultural development trickling down to the labouring classes: the poor peasants and the agricultural labourers.

We put relatively little emphasis on family farmers. Partly this is due to the peculiar Indian context. It was a striking fact that all farmers, including the smallest ones, i.e. the smallholders who were themselves dependent on working as coolies for others, hired labour to help out on their own farms. We demonstrated that this was a peculiar form of labour exchange, with the labour market as a clearance house. In other words, smallholders who themselves hired labourers, also worked as such. In an exchange labour system, working for others entail an obligation for them to work for you. If a similar exchange is cleared via a labour market, and at current wages, there is no obligation to return a favour. This makes it difficult to spot the family farmers in an Indian context, because they too hire in labour.

The results of the Tiruchy field work did not exactly corroborate our hypotheses; on the contrary they were severely contradicted by the empirical evidence. The Green Revolution had indeed taken roots in the District. The new varieties were grown on large parts of the rice area, especially in the canal-irrigated tracts. Rice yields had gone up dramatically. So had inputs of inorganic fertilizer and pesticides. While DDT had been blacklisted, we noted that its more persistent relative Endrine and the more acutely toxic Parathion were widely used (Athreya, Djurfeldt et al. 1990a, p. 87).

Green Revolution, yes indeed! But its social consequences were not as expected. When we looked at the distribution of operated area we found that inequality had gone down rather than increased.

Similarly, the real value of daily wages had gone up. Although our data did not allow us to infer anything about the impact of the Green Revolution on the number of labour days per agricultural labourer, we could conclude that if the number of days had not substantially decreased, the real income of labourers would have increased. This was demonstrated for another district in Tamil Nadu by Hazell and Ramasamy (1991).

What about the supposed barriers to this development, landlordism and merchant-usurious capital? Land reforms in Tamil Nadu, as in most Indian states, had led to little redistribution of land. However, in our area there had been a *de facto* redistribution of land, to a large extent caused by pre-emptive sales by landlords opting to move out of agriculture. The land market – the vicious market! - had thus achieved much of what the land reforms were designed to do, but apparently failed to achieve.

We used Banaji's term *merchant-usurious capital* to refer to a phenomenon noted already by Chayanov, i.e. that merchants lend money to smallholders in return for a future contract to buy his crop. If the implicit interest is usurious, this is obviously not beneficial to the smallholder. Usurers were also prolific in the Tiruchy villages and although they lent primarily for consumption, they also had a role in production, which was not exactly progressive. Thus there was a lot of usury, but as we could demonstrate, less of merchant-usurious capital. We collected mountains of data on merchants and their role in production via credit, but we failed to substantiate the hypothesis about the crippling effect on the development of production and productivity.

Thus to put it squarely, landlordism and merchant-usurious capital seemed to have retreated and they were definitely not the barriers to development that we had expected them to be: The barriers were broken, to cite the title of our book. What had broken them? A Marxist, or for that matter a rigorous neo-classical economist would have looked for an economic explanation to this. We found the explanation elsewhere.

The Green Revolution in Tiruchy District was certainly *conditioned by* production relations, but *it was not determined by them. The driving force was state intervention.* The government was driving the various programmes designed to implement the Green Revolution; its land reforms had enough teeth to indirectly bring about redistribution of land; and the expansion (and nationalisation) of banks was vigorous enough to drive down interest rates for loans, even in the private market (Athreya, Djurfeldt et al. 1990a). Thus the barriers were superseded, at least temporarily as we would later learn.

Some of the above conclusions were established by adding a longitudinal dimension to our cross-sectional data, for example by retrospective questions on landownership, or by comparing historical wages with current ones. The real test of these findings was made much later when we got genuine longitudinal data. These are superior to cross-sectional ones when it comes to distinguishing correlation from causality, and disentangling the exact effects and causes of the development we had documented (Djurfeldt, Athreya et al. 2008a; Djurfeldt, Athreya et al. 2008b).

The discovery of how our cherished theory failed to predict important traits in agricultural development in South India, led to my questioning, not only the theory as such – because theories can always be molded *post factum* and can be massaged to fit the findings from an empirical study. I concluded that the problem lay in how we were trained in the Marxist tradition to look upon the

role of theory in empirical research. This also led me to doubt the fruitfulness of some of the core concepts in this theory.

The resemblance between Marxism and neoclassical economics

This will provoke many Marxist-leaning participants in this workshop: I claim that there is a *non-coincidental resemblance between Marxist political economy and neoclassical economics!* Both theories aspire to formulate a universal theory which would explain the structure and development of all market economies. The main difference is to the advantage of neoclassical theory: It can pride itself in working with stringently formulated assumptions and a mathematically formalised way of developing the corpus of the theory. *It is a pity that the theory does not apply to any real existing economy!*

Let's face the truth: Marxist political economy has no strictly formulated assumptions and no formalised way of developing theory and hypotheses! Normative assumptions enter instead of axioms, dialectics and other 19th century philosophical debris come instead of rules of reasoning. Empirical evidence is used *ad hoc* or anecdotally, where it fits theory. Again in contrast to the economists, whose methodological awareness and sophistication is much greater, thanks among other things to econometrics.

I believe that it is a mistake to strive for a universal theory of the kind mentioned. This can be easily seen by looking at neoclassical theory. It achieves its rigour only by making some very strange assumptions, for example that the economic "actor" has perfect information and that he (always a "he") is perfectly rational. Only by assuming this, economic action becomes predictable, but at the price of assuming away some of the most fundamental universalities of human existence, i.e. that since reality is not transparent we never have full information,² and although human beings have the faculty of rational reasoning and action, we only occasionally use it. Since action, economic or otherwise, is in principle unpredictable, there is no way of developing a universal theory of the kind aspired to in these traditions. Moreover, actors are not only individuals, as assumed in neoclassical economics but also organisations or institutions of various sorts, from families and households, over firms³ to governments and international bodies. In recognising corporate or collective action, Marxism is more realistic, although its assumption about classes as corporate actors is a major and tragic folly. Corporate action, like individual action, is in principle unpredictable. Therefore universal theories predicting the development of societies, or of the global society, are in principle discredited.

Returning to Tiruchy district and its Green Revolution, it was a valuable chastisement to get one's pet theories falsified by a rigorous empirical study, but the lessons to be learnt are more fundamental than that: Factors (or actors) exogenous to the agrarian economy and society are indispensable to an explanation of how this came about. You cannot explain the retreat of landlordism without invoking the State and the land reform policies pursued, although thanks to their apparent toothlessness these did not radically confront the interests of the landlords. Likewise, to account for the retreat of usurious capital requires recourse to government action, more specifically the nationalisation of banks. The later return of the usurers, likewise could not be predicted (Djurfeldt and Lindberg 2007).

² Joseph Stiglitz earned his Nobel prize working out some of the consequences of this.

³ Oliver Williamson earned his Nobel prize for making the theory of the firm more realistic, partly by loosening the assumption about rational action on its part.

The above is not to say that human or corporate action cannot be explained. Of course it can, but only *post factum*, when it has occurred and with all the traps and temptations implied by historical explanation. On the other hand, that is the maximum that social science can aspire to.

Middle range theory?

We need to distinguish, I believe, between at least three levels of theory: universal, middle range and local and time-specific theory, or explanation. At the first level there is an analytical toolbox, providing us with general or analytical concepts or theories, which achieve their universal applicability largely by being empirically empty. Here belong both methodological tools, like statistics or formal logic and universal concepts like action, institution, organisation, production relations or what have you.

Middle range theories⁴ have a range of application which, as the name implies, is less than universal, for example in applying to “peasant societies”, if indeed the latter category is empirically and theoretically meaningful. Within this framework, provisional generalisations can be made, but only to the time- and place-specific referent of those theories.

We should perhaps avoid the term ‘time- and place-specific theory’, because it adds to confusion about what theory is. It is preferable to talk of time- and place-specific explanation, for example of the structure and process of a given ‘peasant society’ or of the Green Revolution in India. In working out such explanation we draw on both the universal toolbox and on middle-range theory.

The persistence of the family farm

Returning at last to the theme of this workshop, but rephrasing it in my terms, it is topical to come back to the persistence of *the family farm*. I had a stint in the 90s when I worked with European and American agriculture. I wrote about the sociology of agriculture, unfortunately in Swedish (Djurfeldt 1994) and led a survey of social conditions in Swedish agriculture, which inspired some work on the concept of a family farmer (Djurfeldt 1996b; Djurfeldt and Waldenström 1996).

I had written on this already in the early 80s (Djurfeldt 1981) and now returned to the theme of how family farms could continue to be the dominant form of agricultural production in highly capitalist societies. I also did a study of landlordism in Andalusia in Southern Spain and realized that big landed property in most historical cases have not been established and generally do not prevail because they are efficient, but because they receive political protection and hidden or overt subsidies (Djurfeldt 1993). The *latifundios* of Andalusia is a good example. They were established already in the 16th century, during the *Reconquista*, the conquering of Southern Spain by *los reyes católicos*, Ferdinand and Isabella, who rewarded their warlords with huge tracts of land in the conquered territory. This story is very similar to the history of landlordism in Latin America. It applies as well to settler systems in Zimbabwe, Kenya and other African countries. The story recurs today in sub-Saharan Africa when investors are granted huge tracts of land in permanent lease or ownership by corrupt officials, or governments misguided by the myth of *big is beautiful* (Djurfeldt 2010).

⁴ The term is due to Merton Merton, R. K. (1968). Social theory and social structure. New York Free press.

It remains true in many circumstances that family farmers are able to compete with capitalist farmers.⁵ This is not only because there often are small or negligible advantages of scale in farm production, but also because of what I used to call *the Chayanovian rule*, which refers to the advantages family farms have in relying on family labour for its operations. Family labour is less demanding in terms of payment, if necessary they can work for very small remuneration, and they are usually much more motivated to toil on the land. Hired labourers on the other hand need fixed wages, piece rates or contracts; they won't work more than they are paid for; they need supervision and may idle when the supervisor turns his back to them; they may even be inclined to use 'the weapons of the weak' (Scott 1985) and sabotage production if they get fed up with the employer.

In order to use the Chayanovian rule in empirical research you need a definition of a family farmer. In retrospect, I can feel that maybe I spent too much effort on taxonomy (Djurfeldt and Lindberg 1987; Athreya, Djurfeldt et al. 1990b; Djurfeldt 1996b; Djurfeldt and Waldenström 1996). On the other hand, a taxonomy building on analytical categories is a must if you want to arrive at any theoretically meaningful conclusions from empirical research. Otherwise you will see no more of reality than what you want to see or what people tell you that they see. This statement obviously implies a realist ontology and that there are layers of reality which are beyond people's perceptions, and which it is meaningful to do research on (Djurfeldt 1996a; Djurfeldt 2000).

While a rigorous definition of *family farms* is thus necessary (Djurfeldt 1996b), it might not be necessary to make the operationalization of the concept too complicated, as we did in our 1990 book (Athreya, Djurfeldt et al. 1990a). In a forthcoming work we use statistical techniques to capture the family farmers (Djurfeldt, Lindberg et al. forthcoming 2012). The database we are working with is a sample of the agrarian population in a large number of Indian villages, in fact a panel, the ARIS REDS database with three waves (1969-71, 1982 and 1999) designed to trace effects of the Indian Green Revolution.

The dataset does not contain the data needed to operationalize our own quite demanding definition of a family farm (Djurfeldt and Waldenström 1996). Instead we use statistical techniques for dividing the cultivators into three groups: (i) those who work as agricultural labourers and (ii) the 5% biggest landowners in the district sample, leaving (iii) a residual category which we believe contains many family farmers. The rationale is simple: The first category captures the 'poor peasants', i.e. those with too small farms to feed them, which in *a purely agrarian context* forces them to work for others. The second group contains big farmers who are highly likely to depend on hired labourers and whose family labour is not used on the land, more than perhaps in supervision or management. The residual category (iii) above should thus contain more family farms than the others. Based on this you can compare for example the propensity for poor peasants or big farmers to have been upwardly or downwardly mobile in size-class of operated area, and indirectly get the propensity for family farmers to be mobile.

A major advantage of the ARIS REDS database, besides it being representative of rural India, is that it also contains data on non-farm and non-agrarian sources of income. Thus it captures *pluriactivity* and its growing role in Indian agriculture, like in the rest of the developing world. Pluriactivity adds

⁵ For contemporary sub-Saharan, this has been demonstrated in an interesting World Bank study: World Bank (2009). Awakening Africa's Sleeping Giant: Prospects for commercial Agriculture in the Guinea Savannah Zone and Beyond. Washington, D.C., The Bank.

to the complexity of studying 'classes' in agrarian societies, because it is a cross-cutting dimension. In my view, it throws doubt on the concept of agrarian class. In statistical terms, you get a situation where a *latent*⁶ nominal variable is associated with another latent variable which is a scale, i.e. the degree of pluriactivity. Thus, not only the mean rate of pluriactivity but the form of its distribution may vary within each class, creating havoc for the whole classification effort.

An increasing importance of the non-farm and non-agrarian sector is of course to be expected in a fast growing middle-level income country like India and in many other countries in the developing world. It is an index of the structural transformation which these countries are undergoing. It is this transformation which prompts the reposing of the agrarian question, which for Marxist-inspired researchers implies a new scrutiny of old assumptions. I will come back to the challenges that this implies.

Green Revolution in sub-Saharan Africa

The above is the baggage I brought with me to sub-Saharan Africa. When I was first drawn into a small project on population and food security in sub-Saharan Africa (Djurfeldt, Egerö et al. 1996), I couldn't help remembering the world as it looked in Thaiyur in the early 70s: basic food insecurity, high level of child mortality and lousy nutritional levels. So the idea came by itself: If Tamil Nadu and India could have its Green Revolution, why not sub-Saharan Africa?

This was at the height of Afro-pessimism in the late 90s, a pessimism which I also recognised from the 70s and how people then looked at India and Asia. I remember vividly the first time I presented the idea about an African Green Revolution at a Sida-seminar in the late 90s in Stockholm: People were shaking their heads, thinking: "This guy is crazy"!

Fortunately, there were more people around the world who were afflicted by the same psychiatric diagnosis, because over a roughly a decade the pendulum swung, as indicated by the World Development Report 2008 (World Bank 2007), which presented a smallholder oriented programme for poverty alleviation and agricultural development in sub-Saharan Africa and elsewhere. Our own book, "The African Food Crisis - Lessons from the Asian Green Revolution" (Djurfeldt, Holmén et al. 2005) contained both an overview of the Green Revolution in various Asian countries and attempt to diagnose the state of agricultural development in the sub-continent. The case-studies of various African countries in that book were of course limited by the cross-sectional database, but it laid the ground for the further work of the *Afrint group*, which produced a panel database of over hundred villages in nine African countries and a book building on the longitudinal data (Djurfeldt, Aryeetey et al. 2011).

The Agrarian Question is unresolved

Some of the points I have tried to make above, are crucial when reposing the agrarian question today. This goes firstly for methodology: development studies in general cannot continue to rely on theories pretending to be universal, but lacking the rigour required for empirical application. We cannot continue relying on cross-sectional data, but need longitudinal and preferably panel data, and thus research designs that are more sophisticated than for example the majority of the studies

⁶ In statistics, a latent variable refers to a variable for which there are no direct and reliable indicators. That 'agrarian class' is such a variable, is testified to by the long discussion on how to operationalize agrarian class, i.e. get indicators for it.

scrutinized by Freebairn (op.cit.). We further need middle-range theories which can embed the 'peasant' or the family farmer in a social and cultural context. This does not only imply unpacking the family or household to lay bare gender and generational relations, but it also requires insight into the wider familial networks into which the family/household is inserted. Furthermore, the family farms needs to be studied in the context of structural transformation, bringing out the growing role of pluriactivity and social mobility which individuals and households are involved in, currently and over a life-span.

Finally, and as illustrated by our Tiruchy study, Marxist-inspired scholars need to give up the assumption of class rule, which is deeply *economistic* and effectively denies the autonomy of the political sphere and the role of governance (another similarity with neo-classical economics!).

Family farmers and smallholders today, especially in Africa, face a growing hunger for land on the part of big investors, supported by governments enthralled by the myth of 'big is beautiful', believing that big farms are superior to small ones – a belief that is contradicted by much research in development studies and agricultural economics. They are also confronted by *supermarketisation*, which is a new form of commercial capital – not Banaji's merchant-usurious capital. There is no doubt, I believe, that the supermarket chains, other things equal, prefer to deal with large producers who can guarantee timely deliveries, in bulk and with qualities demanded by exacting consumers, not only in the West, but in the growing urban middle classes in India, Mexico and other countries.

Here the old distinction by Chayanov, between horizontal and vertical integration comes to mind. With the former, he referred the concentration of land in big units, i.e. the development of capitalist agriculture as here defined, while with vertical integration he referred the integration of a multitude of small producers with agro-industrial buyers. His preferred form of vertical integration was the co-operative one, which came to prevail in much of Western Europe. This would also be my preference for the supermarketization in the developing world.

However, the smallholders in Africa, Asia and Latin America do not possess the resources needed to organise in co-operatives, especially given the history of a perverted cooperative movement in these countries since the 60s. International institutions, like the World Bank and the various UN organisations, as well as NGOs of various sorts and national governments need to push for the vertical integration of smallholders into the growing agriculture and food markets in the developing world. Otherwise the agrarian question will remain unsolved.

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