

Introduction: Contextualizing and Interrogating the GKI Case for Redistributive Land Reform

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The GKI case for redistributive land reform is first contextualized historically: by considering its broad historical context that extends back to ancient times, and its more recent context, that of the effort to secure 'development' in poor countries in the post-1945 era. Two broad forms are briefly considered: tenurial reform and redistributive reform. The decline of land reform in policy agendas from the late 1960s onwards, and its recent reappearance are noted. That reappearance has included 'market friendly' reform, strongly pushed by the World Bank. It is in these contexts that the bold and radical GKI case for redistributive land reform has been made. A brief résumé of that case is provided. Thereafter, the nature of the interrogation of GKI by contributors to the special issue is outlined, this including treatment of the GKI methodological/ideological context. This interrogation covers a questioning of: the nature of their neo-populist/neo-classical logic and the theoretical problems associated therewith; the existence of an inverse relationship between land productivity and land size; the supposed impact on agricultural growth; the postulated effects of urban bias; and the ignoring of the 'real politics of land'. This encompasses treatment of Japan, Taiwan, China, former Soviet bloc countries, Southern Africa (with a focus on South Africa and Zimbabwe) and Bangladesh.

Keywords: land reform, redistributive reform, tenurial reform, neo-populism, neo-classical approaches to land reform.

LAND REFORM: ITS CONTEXTS AND ITS VARIANTS

Historical and Contemporary Contexts

As has been observed: "The history of land reform is as long as the history of the world, extending back into medieval, ancient and biblical times" (Tuma 1965, 3). Indeed, one particular tenurial form/mode of surplus appropriation, share tenancy, whose elimination or moderation has often figured in land reform

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programmes, or in agrarian struggle, in the past, and does so down to the present, 'is as old as recorded history' (Byres 1983, 2).¹

Our historical context cannot be so broad, although we do well to note land reform's ancient lineage, and its relationship to often momentous and disruptive agrarian change in the past.²

Our concern, rather, is with the contemporary preoccupation with land reform – the here and now. That, indeed, must be seen in its own historical context: that of the effort to secure 'development' in poor countries in the post-1945 era. That encompassed, in the initial decades, both socialist and non-socialist countries, the latter via some form of capitalism, with the socialist state and socialist planning matched by the so-called 'developmentalist' state and non-socialist planning. At the dawn of this era, it was the received wisdom, as expressed in an important United Nations document of the time, *Land Reforms. Defects in Agrarian Structure as Obstacles to Economic Development*, that:

for many countries the agrarian structure, and in particular systems of land tenure, prevent a rise in the standard of living of small farmers and agricultural labourers and impede economic development, both by preventing the expansion of the food supply and by causing agriculture – usually the major economic activity – to stagnate. (United Nations 1951, 89)

That is to say, there was a powerful case for land reform, both for reasons associated with agriculture itself and for inter-sectoral reasons.

Tenurial Reform and Redistributive Reform

There are, indeed, different kinds of land reform. Abstracting from, for example, government-sponsored settlement schemes, and distinguishing collectivization (the socialist attempt to resolve the agrarian question) from land reform (more usually a non-socialist initiative, and sometimes a moment in the development of capitalist agriculture), two basic types have received attention in the past: *tenurial reform* and *redistributive reform*. Both, in the post-1945 era, have been the initiatives, or potential initiatives, of 'developmentalist' states: top-down, state-sponsored land reform, but land reform for which the way was cleared, almost

¹ It is found in ancient China in the Spring and Autumn Period (722–481 BC), in ancient Greece at the time of Solon in the sixth century BC and in ancient India in the fourth century BC. On these early manifestations of sharecropping see Byres (1983, 7–11). It is likely, indeed, to have predated those first recorded instances.

² In his *Twenty-six Centuries of Agrarian Reform. A Comparative Analysis*, Tuma goes back to the Greek reforms of Solon and Pisistratus of the sixth century BC and the Roman reforms of the Gracchi in the second century BC. He then covers the medieval English reforms (commutation) and the English enclosures; the reforms that accompanied the French revolution; the three Russian reforms of 1861 (emancipation of the serfs), 1906–11 (the Stolypin reforms) and 1917 (the Soviet reforms); the Mexican reforms after the 1910 revolution; the Japanese reform after 1945; and the Egyptian reforms that started in 1952.

always, by some form of prior peasant struggle. Land reform did not reach policy agendas in a political vacuum.³

Tenurial reform concerns the terms on which the operational holding is held and worked, and seeks to eliminate those aspects of the tenurial relationship, or the form of operational holdings, that are held to dull incentives, reduce the wherewithal to invest and impede efficiency, and so prevent the emergence of an efficient, dynamic and growing agriculture. In the ideal, tenancy itself may be abolished, with a policy of 'land to the tiller'. In that instance, all (or almost all) the tenurial obstacles to a dynamic agriculture would be swept away. Where tenancy remains, such land reform has included the following strands: the abolition of absentee landlordism, held to be inferior to resident landlordship, on the grounds that the absentee has no interest in productive investment in the land; the eradication of a particular tenancy form, sharecropping, held to be especially pernicious, because of its effects on incentives and its heavy appropriation of surplus (so removing from the producer the means to invest); the introduction of fixed money rents and 'fair' rents (i.e. controlled rents), which commercializes agriculture and controls the rent burden; the elimination of insecure tenure, since it is a disincentive to that investment whose benefits accrue in the longer term. We may also include the rooting out of land fragmentation (where an operational holding is held in several physically separate pieces), via a programme of consolidation of holdings, although owned as well as tenanted holdings may be fragmented. Such fragmentation is held to lead to an inefficient use of land, labour and capital. In theory, such tenurial reform, in its various manifestations, could proceed without any change in the distribution of operational holdings, with both large and small holdings (rich, middle and poor peasants) benefiting. In practice, the most likely beneficiaries have been larger/richer peasants.

Redistributive land reform is, in principle, more radical, and seeks to redistribute operational holdings, taking land from those with large operational holdings and transferring it either to those with no land at all (landless peasants and wage labourers) or those with tiny holdings (poor peasants), and imposing ceilings on the size of operational holding. A crucial part of its *economic* rationale has been that there is an inverse relationship between land productivity and size of holding, with a gap of as much as 50–60 per cent between the largest and the smallest holdings. This has sometimes been held to show that small holdings are more 'efficient' than large; that, certainly, redistribution would bring about a significant once-and-for-all rise in output; and probably that redistribution would bring a more quickly growing agriculture. It has been further argued that such redistributive land reform would reduce the incidence of widespread rural poverty, by providing land, the 'means of subsistence and a security to borrow with' (Krishna 1959, 302) and the source of employment, to those without it or

³ The account given in this sub-section derives from reading over many years in relation to my own research and to my teaching of courses on 'Agriculture and Development' at the School of Oriental and African Studies. Detailed treatment may be seen in my own writing. The relevant sources (including my own writing) are too diverse to cite here. But a useful compilation that may be quarried with profit is Parsons et al. (1956).

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with inadequate amounts. It may also be deemed to be *politically* necessary, where peasant struggle has had redistribution as one of its central demands. In one formulation, ‘it is the essential token of status in rural society’ (Krishna, 1959, 302). But, more potently, it is as a means of security and livelihood that it becomes the object of struggle. Redistribution, where it has been pursued, has required ceiling legislation, itself a matter of considerable controversy, over what the size of a ‘viable’ holding might be and what the ‘optimum’ size of holding is.⁴ To be successful it has been assumed to require simultaneous tenurial reform, so that the operational holdings will be worked to maximum effect, with the necessary incentives in place. It is *redistributive land reform* that is the focus of this special issue.

The Prominence, Eclipse and Reappearance of Land Reform on Policy Agendas

Land reform was prominent on the development agendas of the 1950s and 1960s, in both socialist and non-socialist states, with both tenurial reform and redistributive land reform figuring in the land reform programmes of a variety of poor countries. In socialist countries it proved to be a prelude to collectivization, and disappeared with the advent of collectives.⁵ In non-socialist countries it achieved limited success, with tenurial reform pursued more robustly, and with greater success than redistributive land reform, and largely to the benefit of richer peasants.⁶ In those countries land reform was swept away as a major policy initiative, by the late 1960s, in the wake of the ‘new technology’ and its ‘betting on the strong’ philosophy, in the belief that much-needed agricultural growth, in countries in which growth had slowed to very low levels, could only thus be secured. That ‘new technology’ was often adopted by those larger peasants who had benefited from tenurial reform. The adoption of that ‘new technology’ produced increasingly unequal structures. Land reform receded into the background from the late 1960s onwards, although it never quite disappeared. From the late 1970s, it was ‘out of policy agendas’ (Borras 2003, 367) more or less completely.

In the meantime, the demise of the ‘developmentalist’ state has been signalled, with its replacement by supposedly leaner and meaner neo-liberal states in poor countries. Land reform, after the eclipse of its earlier prominence in a variety of poor countries, has recently returned to policy agendas, but often now in a form more attuned to the market. The World Bank, for example, has been urging ‘market friendly land reform’, altogether more appropriate for such neo-liberal

⁴ For a classic work on this, see Khusro (1973).

⁵ For an excellent early treatment of land reform in China, between 1947 and 1952, as a prelude to collectivization, see Nolan (1976).

⁶ There is a large literature, of uneven quality, some of it general and some relating to specific countries, on the experience of non-socialist countries. For a flavour, at least, of land reform programmes in the 1950s see, for example, United Nations (1954, 1956) and for the nature of land reform programmes and something of the experience in some countries in the 1950s and 1960s, Warriner (1969) and Lehmann (1974).

states, which involves creating incentives and providing the means for large landowners to sell part of their holding. It is an approach that is hostile to state-led agrarian reform, with its supposed large bureaucracy and top-down methods, and postulated attendant high costs, corruption and distortion of land markets.⁷ This reappearance of land reform as an issue has been true of both non-socialist and former socialist countries.

But it is not only advocacy of a particular kind of land reform by the World Bank that has brought land reform back on to the policy agenda. There have been powerful movements from below – land invasions and occupations – in, for example, Latin America, in which an effort to secure redistribution has been made.⁸ Indeed, the Bank's programme may even be a response to these popular struggles over land: an attempt to defuse them and bring order. There has also been a particular form of redistributive land reform in Zimbabwe, very different to the market-mediated land reform advocated by the World Bank, that has attracted unfavourable attention in the West. So land reform, indeed, is very much of the moment, and land reform that is decidedly redistributive.

THE GKI INTERVENTION

This special issue is a response to the article by Keith Griffin, Azizur Rahman Khan and Amy Ickowitz, 'Poverty and Distribution of Land', published in an earlier issue of the *Journal of Agrarian Change* (Griffin et al. 2002). There, a theoretical case is made for truly radical redistributive land reform, which would take land from large landowners and vest it in small holdings, with those who would work it as owner-operated, family-based farms. The case for tenurial reform is dismissed, on the grounds that it cannot produce a superior outcome, and might even worsen matters. They note the World Bank's 'market friendly', 'negotiated' land reform. This they reject as a realistic solution to problems of inequality, poverty and growth, because

the financial cost to the government of a 'market friendly', full compensation land reform is bound to be onerous and the government is likely to feel compelled to shift as much of the financial burden as possible on to the beneficiaries. (Griffin et al. 2002, 321)

It is too expensive and could only have very limited impact. They argue:

The inescapable conclusion is that a major redistributive land reform is impossible if land transfers are based on free market prices; either government

⁷ See Borras (2003) for an account of World Bank so-called Market-led Agrarian Reform and its operation in Brazil, Colombia and South Africa. There the critique of the state-led agrarian reform model and the logic of market-led agrarian reform, as stated by its two major exponents, Klaus Deininger and Hans Binswanger, are considered. See Binswanger (1996a, 1996b), Binswanger and Deininger (1996, 1997), Deininger (1999), Deininger and Binswanger (1999), van Zyl et al. (1996).

⁸ Bernstein draws attention to this in his contribution to this special issue. He refers us to Petras (1997, 1998) and Veltmeyer (forthcoming) on Latin America. The most powerful manifestations of these movements have been in Latin America.

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must act to depress land prices or there must be outright confiscation of some kind. (Griffin et al. 2002, 321)

It is a remarkably bold policy recommendation, its startling boldness not quite captured in their warning: ‘This is a painful nettle to grasp, but it is unavoidable if there is any hope of success’ (Griffin et al. 2002, 321).

The land reform advocated, it is argued, would produce a more efficient and a more dynamic agriculture and would soon eliminate rural poverty. That case is made theoretically: concisely, rigorously and cogently, and with great clarity, within a neo-classical framework. And, in its treatment of the ‘power of landlords’ to mobilize and organize labour to ‘extract effort from their workers and tenants’ (Griffin et al. 2002, 287, 288) it seems to embrace a political economy that transcends usual neo-classical treatment. In the very broadest terms, in the pre-reform situation, monopsonistic landowners confront rural labour, in circumstances of significant inequality. Fragmented factor markets (for land, labour and capital) and powerful systems of labour control generate underemployment and pervasive rural poverty. Because of these fragmented factor markets, small farmers (for whom labour is relatively cheap and land and finance capital relatively expensive) adopt lower capital–labour ratios/higher labour–land ratios and large farmers (for whom labour is relatively expensive and land and capital relatively cheap) adopt higher capital–labour ratios/lower labour–land ratios; and it is clearly the case that small farms produce significantly higher output per acre – are more efficient – than large farms. A redistributive land reform would, therefore, increase agricultural output significantly and would accelerate agricultural growth. It would also, concomitantly, get rid of rural poverty.

In addition, something of ‘the rich experience of individual countries and regions’ (Griffin et al. 2002, 292), that bears upon their case, is considered. That falls into two separate treatments: the first of certain ‘varieties of regional experience’ where their redistributive logic needs to be applied, which covers sub-Saharan Africa, Latin America and the ex-socialist countries of the former Soviet Union, although not, one notes, South Asia; and the second of a range of ‘land reform successes’, which covers Taiwan, South Korea, Japan, post-collective China and post-collective Vietnam. The foregoing is geared, one might say, to the microeconomic context. There is a section, also, on ‘Land Reform in a Macroeconomic Context’, where it is argued that redistributive land reform will lead to an acceleration of *overall* economic growth. This will be so, however, only if certain conditions are met. The pernicious influence of ‘urban bias’ is argued and it is insisted that ‘the removal of urban bias is . . . a necessary condition for a successful redistributive land reform’ (Griffin et al. 2002, 317) and, therefore, for the postulated more rapid economic growth. So, too, must what GKI refer to as ‘landlord bias’ be removed. Although South Asia receives little attention in previous sections, Bangladesh makes an appearance in this section, as it does in the Appendix on ‘How Should Land Concentration Be Measured?’. Bangladesh is seen as a prime case for redistributive land reform.

The GKI paper has already had a considerable impact. It is an important statement of the case for redistributive land reform, which seems dramatically radical and may be very attractive for those in the development field normally hostile to neo-classical approaches (among them academics, intellectuals, students, NGOs – if not representatives of governments). Its considerable appeal lies in its appearing to hold out the promise – if it could be implemented – of eradicating rural poverty and accelerating both agricultural and overall growth; and doing so with a case that is rigorously constructed and confidently presented. In its very boldness – its dramatic boldness – it seems to embrace a potent iconoclasm that is powerfully at variance with the recommended land reform policies of the World Bank and clearly on the side of the poor. Moreover, it has a political economy content that neo-classical texts usually lack and which seems to give it a relevance that appears absent from more purely technical exercises.

There is no need to rehearse the full details of the GKI treatment here. Interested readers will consult the original article. Moreover, the different aspects of the GKI argument are presented, in all their details, in the papers that follow. In those papers, the relevant theoretical and analytical aspects of the argument are addressed critically and some of the differing regional experiences are considered, at length and critically. We may highlight, however, the themes that are pursued in the various contributions to the special issue.

INTERROGATING THE GKI ARGUMENT: FURTHER CONTEXTS AND THE BROAD THEMES

Ideology and Theory

GKI – and more precisely, Keith Griffin himself – are located ideologically, in the Byres paper, as a crucial part of an important body of thought, ‘neo-classical neo-populism’, that began with the famous Russian A. V. Chayanov, and that includes, more recently, most notably, the work of Michael Lipton. Both Griffin and Lipton made their major initial contributions, within this tradition, in the 1970s (Griffin 1974; Lipton 1977), and, indeed, the GKI argument is to be found, in its essentials, in the Griffin 1974 book. They are similarly identified, whether as ‘populist’ or ‘neo-populist’, by Bernstein, by Sender and Johnston and by Karshenas. Khan, indeed, focuses upon their neo-classical methodology, stressing that it is one of two competing neo-classical approaches to understanding the constraints in the agricultural sector, the other being that of the World Bank. Here, then, is their methodological/ideological context.

The theoretical problems associated with such an approach are subjected to close, detailed scrutiny by Khan, who considers GKI’s neo-classical approach, and to broader examination by Byres, who looks at both the neo-populist and the neo-classical levels of their analysis, and their contradictory co-existence. Both Khan and Byres argue that the GKI model is defective in the essentially ahistorical nature of its approach and, crucially, in the ignoring of the processes associated with capitalist transformation in the countryside, i.e. agrarian transition.

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Dyer, Sender and Johnston and Bernstein, in their different ways, consider the implications of the ignoring of capitalism by GKI.

For Khan, the GKI model is logically flawed inasmuch as although they argue (along with World Bank economists) that reallocation of land fails to come about because of market failure and hold (unlike World Bank writers, who follow a high transaction costs approach) that this is because large farms enjoy a monopsony in the labour market, in fact it turns out to be a high transaction costs explanation (hidden high transaction costs). It collapses, then, into a World Bank position. Khan rejects the transaction costs argument emphatically. Concentrating on Bangladesh, he argues that, in fundamental contrast to GKI, what is necessary for understanding the dynamics and the constraints of agrarian transitions is a placing of class and power at the centre of an analysis of structure and change in the agrarian economy; and he focuses on the distribution of power that prevents active processes of primitive accumulation (defined as 'the non-market reallocation of land') leading to a capitalist transformation. I comment on his argument further, below.

For Byres, an essential problems lies in their pursuit of the twin objectives of equity (the neo-populist goal) and efficiency (the neo-classical desideratum). Byres argues that the contradictions inherent in this pursuit are never satisfactorily resolved: that GKI ignore the structural inequality that is an essential part of capitalism, failing to capture the existence of differentiated peasantries and thus ignoring the class structure of the countryside. It is further argued that its logical foundation is the neo-classical construct of perfect competition, which is without historical basis. The rejection by GKI of the case for tenurial reform is contested by Byres.

The Inverse Relationship

As Dyer stresses, the inverse relationship lies 'at the heart of the GKI theoretical framework'. This central GKI postulate is questioned by Byres, on the grounds that while such an inverse relationship undoubtedly exists in the circumstances that obtain in the countryside before a full capitalist transformation, and may have existed for centuries, it does not survive such a transformation. To maintain its continuing existence is to pursue a static approach in a dynamic context. It is also questioned by Khan for Bangladesh, by Bramall in relation to China, and, in a detailed scrutiny of the available evidence for Africa, by Sender and Johnston.

It is, however, most comprehensively considered by Dyer in a searching critique, at once theoretical, methodological and empirical, of the central supports of the inverse relationship argument, the work of Berry and Cline (1979) and Cornia (1985). Berry and Cline's work, in particular, is regarded by many as the definitive text on the inverse relationship, and is cited by GKI as such. Dyer argues, and seeks to demonstrate, that the empirical evidence used by Berry and Cline is seriously flawed and cannot be employed to test the hypotheses proposed by the authors. He considers cross-country analysis and evidence for Brazil, West Pakistan and India. He holds that an inverse relationship does, indeed, exist, but does so only where pre-capitalist social relationships dominate. In the dynamic context of capitalist development, however, there are powerful

forces that cause the breakdown of the inverse relationship. Most notably, large (capitalist) farms now have access to a ‘new technology’ that embodies clear economies of scale. Dyer argues that the essentially static neo-classical approach to the inverse relationship, pursued within a marginalist framework, embraced by Berry and Cline and inherent in GKI, is seriously defective. He provides an alternative, dynamic political economy approach – a class-theoretic approach – to understanding the inverse relationship, that undermines, he suggests, the use of the latter as the central rationale for redistributive land reform.

Some GKI Exemplars Scrutinized: and a Questioning of the Growth Effect and of Urban Bias

GKI’s exemplars are Japan, Taiwan, South Korea, post-collectivized China and post-collectivized Vietnam. Among them, Taiwan emerges as *primus inter pares*, the classic case that supports the GKI argument completely. Taiwan was the prime example when Griffin first made his case in 1974. It continues to be pre-eminent, although all of the cases noted are now regarded seriously by GKI. Of these, the first four are treated in this special issue.

We may start with China. Bramall, in a comprehensive and rigorous treatment, deploys all of the available data, and often data that have only recently become available. He argues that the redistributive land reform implemented in China between 1947 and 1952, looked at with favour by GKI, did not lead to the predicted, pronounced rise in agricultural output. Nor was it egalitarian, inasmuch as it deliberately preserved the rich peasant economy. Indeed, but for that, there might have been no growth at all. The second land reform, of 1981–3, also taken as exemplary by GKI, similarly did not give rise to an acceleration of agricultural growth. Rural income inequality has been held in check since 1984, but this has been the result not of redistributive land reform but of local government intervention. It has not been because family farming is intrinsically egalitarian. Bramall concludes: ‘GKI claim far too much for the impact of China’s land reforms of 1947–52 and 1981–1983’. He further concludes that while ‘it may be that the system of farming put in place in China in the early 1950s and restored after 1981 is preferable to collective farming’ and while ‘GKI are surely right in their criticism of landlordism as practised in China before 1949’, nevertheless ‘the superiority of family farming is far less marked than GKI claim’. He goes on: ‘If there are lessons from the Chinese experience, they are that peasant farming works only when implemented and overseen by pro-active local government, and that technological modernization – based around HYVs, chemical fertilizers and improvements in irrigation – is at least as important as systemic reform in generating sustained increases in agricultural production’.

Bramall further argues – far more briefly – that China’s experience of land reform is mirrored by the experience of other East Asian countries. A century of land reform has not resolved Japan’s deep-seated agricultural problems, nor those of Taiwan and South Korea: ‘Nor did East Asian land reform [in Japan, Taiwan and South Korea] provide the foundation for sustained long term growth’.

With respect to the other East Asian exemplars, Bernstein points out that, paradoxically, there is an absence, prior to reform, in the East Asian ‘success stories’, of large-scale production deploying coerced labour (the Latin American model) that is central to the GKI case for redistributive land reform, and which constitutes the essence of their theorizing of monopsonistic landowners and rural labour. Byres points out that, after reform, in Taiwan, the most favoured of all the GKI exemplars, it has been an agriculture thoroughly embraced by the state: a state that has been described as a ‘garrison state’, with administered prices. In Taiwan, it has been suggested according to one writer, that a ‘tightly controlled large-scale context leaves only a residual area of manoeuvre for free small-scale decision making’. That is hardly consonant with the GKI vision.

The GKI ‘urban bias’ argument is scrutinized and rejected by Karshenas, via consideration of the *historical experience* of Japan and Taiwan. Karshenas argues that agrarian relations play a critical role in the pattern of intersectoral resource flows and the way in which the agricultural sector shapes the macro-economy in developing countries. The notion of ‘urban bias’ used by GKI is defective in its abstracting from the pre-existing agrarian system and from the prevailing institutions and in focusing on one simple set of influences outside the agricultural sector itself, i.e. government policy bias. This is illustrated with reference to the experience of Japan and Taiwan: the former between 1888 and 1937, which is far more relevant to poor countries than the post-1945 era and when Japanese agriculture made an important financial contribution to the rest of the economy; and the latter from 1911 to 1969, which corresponds to distinct phases of colonial and post-colonial development, which laid the foundation for the post-1960s phase of export-led development. Their experience shows that high rates of taxation and surplus extraction from agriculture are not incompatible with maintaining profitability and production incentives in agriculture, as long as agrarian relations and other enabling conditions can ensure a fast enough rate of technological progress and productivity growth in the sector. The macro-economic implications of different agrarian relations are much more complex than the urban bias story told by GKI would suggest.

Some Other Experiences: The Former Soviet Bloc Countries and Sub-Saharan Africa

Although Latin America is mentioned in the contributions to this special issue, we have no substantive treatment. This is unfortunate because of the general significance of Latin America, and of its particular importance for the GKI model, as we have seen. We would welcome future contributions on Latin America.⁹

⁹ One notes the outstanding contribution of Cristóbal Kay to our understanding of land reform and, more generally, of the agrarian question, in Latin America. This clearly bears closely upon the GKI case. GKI, indeed, cite, with favour, two of his works (Kay 1981, 1998). This is not the place to pursue the nature, or the details, of Kay’s contribution, which, indeed, has continued since his first seminal piece in 1974 (Kay 1974) through to the present, including a recent article in this journal (2002). They have included penetrating studies of individual Latin American countries – Chile (Kay 1975, 1977, 1978, 1981, 1992, 2002; and Kay and Silva 1992) and Peru (1983) – and general analyses of Latin America (1974, 1980, 1983, 1998, 2000) that are always powerfully illuminating.

In a brief but illuminating note, Kitching focuses on the former Soviet bloc countries. He argues that, while it is difficult to be critical of GKI because the picture painted of the countries in question is so broad, the treatment is not sufficiently detailed to be analytically illuminating. Four important weaknesses are identified. The first is the problematic analogy of private plot agriculture on the former state and collective farms with *minifundia* in Latin America, and the consequent analogy of collective farms with *latifundia*. This is a false analogy because of the very different institutional context within which labour relations were set in the Soviet Union, such that the real power relations were very different. The second is the argument that private plot farming had been involuting in a Geertzian sense. This is, quite simply, irrelevant, inasmuch as, in the given circumstances, in which the private plot was essential to keeping body and soul alive, price or value measures of agricultural output are beside the point in assessing the real value of agricultural output or the real income and welfare situation. Thirdly, the GKI redistributive land reform agenda for Russia is questioned and a case is made for privately owned and managed large farms. It is hard to be optimistic, he argues, about the real bargaining position of the small farm sector in the fight for resources. Finally, GKI are criticized for failing to take account of the limited rural labour supply in Russia.

Sender and Johnston consider the available evidence for sub-Saharan agriculture and argue strongly that it simply does not support the GKI proposition that small farms are more efficient than large farms: small farms do not outperform large farms in terms of yields. They further argue, on a basis of that evidence, against GKI, that some forms of capitalist agriculture may create net additional employment in farming, and thereby have at least some impact in reducing rural poverty, while supposed redistributive land reform simply does not have the postulated effect. Thus, the beneficial employment effects are suggested of plantations (producing, for example, sugar cane, tea and bananas), agribusiness (specialized in high-value export commodities, such as horticultural products) and dynamic medium-scale farm enterprises (producing a variety of crops), with crops produced in ways that are both capital- and labour-intensive, as well as internationally competitive. They suggest, moreover, that recent land reforms in Southern Africa have had negative consequences for employment and poverty, and especially for the weakest groups in rural labour markets (and particularly women). Their position, then, is especially interesting. It represents an attempt, unique among the contributions to this special issue, to marry the classic insistence on the superior productivity of large-scale (capitalist) farming with a forceful argument that it delivers greater employment and income benefits, especially to the poorest of the rural poor, i.e. the landless and the semi-landless who depend on wage labour (particularly casual labour), than does small-scale farming. In short, Sender and Johnston take on GKI on the terrain that they claim as their own, that of development that reduces rural poverty.

Bernstein contests the generality of at least the employment and income benefits of capitalist farms claimed by Sender and Johnston. He argues that they base their argument on the highly unusual experience of the Western Cape

province of South Africa. Elsewhere in South Africa, and more generally in sub-Saharan Africa, it may not apply.

The 'Real Politics of Land': and Particular Treatment of Bangladesh and Zimbabwe

Finally, GKI are taken to task for their ignoring of what Bernstein terms the 'real politics of land'. Both Khan and Bernstein, in their different ways, address this critical issue. Byres points to the massive and dramatic action by the state that is called for if the GKI vision of redistributive land reform, on the recommended scale, without compensation, is to be implemented. GKI simply abstract from this. This is one sense in which the 'real politics of land' is, indeed, simply ignored. For what purports to be a political economy treatment, this might be seen as a serious omission.

Khan points out that, while the conventional neo-classical approach ignores politics altogether, GKI do try to identify a political obstacle to change. But they do so on the basis of a generalized ahistorical model. Khan seeks to remedy this. He argues that the configuration of power in society is historically specific and can only be analysed in the context of specific cases. Yet this configuration of power is a critical variable determining the pace and direction of the agrarian transition that can in turn explain long-term productivity growth. Such a framework might well be deployed to address the likelihood of implementation of massive redistributive land reform. Such implementation is not at all likely. While, in a country like Bangladesh, the role of the state in constructing agricultural infrastructure is widely recognized, the determinants of why the state does not do this more vigorously are inadequately theorized. Indeed, they are not theorized at all by GKI. Similarly, one might say, implementation of redistributive land reform, in the real political conditions of poor countries like Bangladesh, is difficult to conceive. Khan provides a treatment, for Bangladesh, in which the organization of landed peasants in *factions* is central in explaining such political realities, within which the balance of power between competing factions can in turn be explained by the disposition of class forces in this society. So, again, the explanatory power of a class-theoretic approach is urged.

For Bernstein, it is on the ideological and political plane, and especially in the 'real worlds of politics', that GKI present materialist analysis with far greater challenges than they do on the plane of intellectual contestation. Populist ideologies, of various stripes and in various ways, claim to articulate the injuries of exploitation, oppression and injustice generated by 'structural inequality of resource access' in the countryside, and to address them through redistributive land reforms. It may be that GKI abstract from political realities in the manner suggested by Byres and by Khan. But, for Bernstein, however weak the economic logic and evidence they deploy concerning productivity, what gives such claims their ideological resonance is the links they make between redistribution and rural employment/poverty. And that resonance is the more potent to the extent that, Sender and Johnston notwithstanding, materialist analysis is unable to provide plausible alternatives to the problems of employment, poverty

and insecurity that define the daily existence of the rural (and urban) majorities of the 'South'. He argues that the tendency of 'globalization' to 'fragment' labour leads to the question whether there might be a (new) agrarian question of labour now detached from that of capital, rooted in crises of employment, and manifested in struggles over, and for, land to secure some part of its reproduction needs.

Nowhere is the 'fragmentation', insecurity, crisis of employment and oppressive condition of labour – all emphasized by Bernstein – more obvious than in Zimbabwe. He seeks to illustrate the profound dilemma confronted by those in search of a viable strategy for resolving these resolutely intractable problems, via a treatment of land redistribution in Zimbabwe with all its manifold contradictions. He describes it as 'a unique case of comprehensive, regime-sanctioned, confiscatory land redistribution in the world today'. My reading of his account is that it suggests that it is a land redistribution that, in practice, quite obviously has done little or nothing to address the condition of the poor in rural Zimbabwe. On the contrary, it has worsened it: especially the condition of farm workers (particularly women). The major beneficiaries have been better-off Zimbabweans, and the effect on agricultural output has been disastrous. Zimbabwe, of course, has its own specificities: not least the concentration of land in the hands of large white farmers, while the rural poor are a black 'lumpen semi-peasantry' and farm workers. Here is no 'development' policy 'intervention' of the GKI kind. It was the chaotic outcome, rather, of a cynical and opportunistic, not to say desperate, politics. The Zimbabwe land redistribution has proceeded in a manner, and with results, far from the orderly redistribution envisaged by GKI. Yet, I would suggest, who is to say that an attempted redistributive land reform elsewhere might not, in the political conditions that exist, have similar results?

Bernstein insists, quite rightly, that such struggles over land cannot be ignored. He holds, further, that issues of redistributive land reform in contemporary capitalism should not be surrendered to the concerns of neo-classical populism. That, surely, has some plausibility. Nor, he argues, should they otherwise be assigned to the dustbin of history (of which, he says, other contributors to the special issue are guilty). That, too, has some force. Yet his admonitions are not accompanied by any clear statement of a way forward.

Conclusion: Comparative Analysis and Scholarly Debate

Such is the 'interrogation' of GKI by the contributors to this special issue, and my gloss on it (with which some of the contributors might not agree). To savour the nuances of the individual contributions, they need to be read, of course, in their own complexity, which is here abstracted from.

It is an interrogation that confronts GKI with an alternative, materialist approach to the issues of agrarian change, and the dynamics of production, property and power in contemporary agrarian formations in poor countries. That approach, as will already be obvious, is not without its own disagreements.

The wide-ranging critique offered of GKI, as well as the differences among the critics, along with the range of countries considered and the comparison made between them, serve to further the journal's goal of providing a 'forum for serious comparative analysis and scholarly debate'. This particular debate is one that is likely to continue. We hope, in particular, that it will attract detailed treatment of Latin America and of the former Soviet bloc countries.

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