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**AFRICAN AGRICULTURE IN 50 YEARS:
SMALLHOLDERS IN A RAPIDLY CHANGING WORLD?**

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ABSTRACT

For economic development to succeed in Africa in the next 50 years, African agriculture will have to change beyond recognition. Production will have to have increased massively, but also labour productivity, requiring a vast reduction in the proportion of the population engaged in agriculture and a large move out of rural areas. The paper questions how this can be squared with a continuing commitment to smallholder agriculture as the main route for growth in African agriculture and for poverty reduction. We question the evidence base for an exclusive focus on smallholders, and argue for a much more open-minded approach to different modes of production. To allow alternative modes and scale of production to emerge, new institutional and policy frameworks are required. A rush to establish 'mega-farms' with government discretionary allocation of vast tracts of land is unlikely to be the answer. Allowing a more dynamic agriculture to develop will require clear institutional frameworks, and not just a narrow focus on smallholders.

INTRODUCTION

It is instructive to think ahead and ask the following question. If, over the next half-century, Africa were to converge on the performance of much of the rest of the developing world both in growth and in poverty reduction, what would be the defining features of the organisation of its agriculture in 2060? The historical experience of most rich economies and the recent experience of fast growing developing Asian economies suggest that five essential characteristics would be concomitant with success: first, a vast reduction in the number of people engaged in agriculture (as this is a feature of economic transformation); second, a massive increase in the urban population and coastal population (as this is where economic activity will increasingly be located); third, in rural areas, a vast reduction of the size of the population living in areas relatively far away from urban areas and from the coast (as incomes in agriculture can only keep up with other incomes where demand is located or where transport is cheap); fourth, a considerable increase in labour productivity in agriculture (as otherwise poverty will have remained high); and fifth, a considerable increase in overall agricultural production, especially in those countries and areas relatively inaccessible from coastal areas (as plentiful and sufficiently cheap food is essential for living standards and growth, and in these non-coastal or less accessible countries and regions, imports will be too expensive to sustain real wages, affecting growth).

The first three are directly linked to migration as part of economic transformation; the fourth is not linked by necessity but nevertheless is typically linked to migration, as throughout the history of development, sustained

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labour productivity increases have been strongly associated with the release of labour from the land. For example, between 1500 and 1800 there was such a transformation in England (Allen, 2009), and in recent years the same has occurred much more rapidly in China, where the rural share of the population has decreased from more than 80% to about 55% in the last 20 years, with rapid increases in labour productivity in agriculture (McErlean and Wu, 2003).

The five characteristics of success are unlikely to be contentious. Nevertheless, they contrast with the current character of much of African agriculture: a vast and only slowly changing number of poor smallholders contributing most of agricultural output, with low yields, limited commercialization, few signs of rapid productivity growth, and population-land ratios that are not declining. In sum, the recent experience is far from being the radical economic transformation which would be appropriate over the next 50 years.

To switch from the slow changing pattern of the past few decades to an agriculture which is rapidly evolving during the next five decades to the entirely different pattern of 2060, a radical improvement in the performance of agriculture is evidently needed. So far, little that we have said should be controversial. The contentious issue is whether the current model favoured by donors and many agricultural economists and scientists is likely to achieve such a transformation. Its approach is to stimulate growth in smallholder agriculture by a variety of interventions, from technology to market development (for discussions see e.g. the World Bank's World Development Report, 2008; Fan, 2011; or Conway, 2012).

The rationale for this conventional donor approach is embedded in the standard development model taught in any basic course in agricultural or development economics. It has three principles: first, both growth and poverty reduction will have to start from agriculture; secondly, smallholder agriculture is efficient in what it does; and thirdly, it needs improvements in technology as well as the functioning of markets (such as for inputs, credit, and output). Once we unlock this potential, growth in agriculture and from this, growth in the rest of the economy will follow. It justifies the current focus of much thinking on supporting African agriculture: an exclusive focus on smallholders as the key to growth and poverty reduction.

In this article, we question this model. More specifically, we argue that the perceived wisdom of the likely success of this strategy is based on weaker evidence than is commonly suggested, while both the changing global economic context suggests that this strategy is unlikely to be successful. In short, without considering more radical strategies, Africa's agricultural growth prospects may be weak. The alternative is not to ditch smallholders and return to the discredited 1950s and 1960s models of mechanized agriculture in the spirit of the Groundnut Scheme. Rather, it is to consider more flexible organisational models in which not all bets are placed on a single unquestioned mode of production. There are striking examples of rapid successful commercialization elsewhere in the world, most notably in the Brazilian Cerrado region or in the Northeast Region in Thailand. Both regions started from 'backward' regions in the 1960s to become successful centres of commercial agriculture, run by private commercial farm and trading enterprises. In Brazil, the farming conditions led to large-scale mechanized production of soybean and rice; in the Northeast region of Thailand, cassava and rice dominate, and farms remain of relatively smaller size but with plot consolidation, vast area expansion and some mechanization, they became commercial farm enterprises different from the typical small peasant and family firms dominating Thai agriculture (World Bank, 2008). No doubt, success will not just come from a naïve replication of these experiences, and it will require appropriate and flexible governance and other institutional arrangements, in terms of access to land and the development of other factor markets. Successful transitions will require a recognition that smallholders are heterogeneous in potential and that there is scope for large scale farmers as commercial enterprises, often in interaction with smaller scale farmers using institutional frameworks that encourage vertical integration and scale economies in processing and marketing.

In the rest of the paper, we first discuss whether the evidence base for an exclusive focus on smallholders is really justified, and argue for a much more open-minded approach to different modes of production. In the second section, we return to the case for smallholders as engines for growth and poverty reduction. Again, the evidence is far more mixed than the exclusive emphasis upon the smallholder approach would lead us to believe. Indeed, too much focus on smallholders may actually hinder large scale poverty reduction. Fast labour productivity growth is what is needed for large scale productivity reduction but smallholders and the institutions

to support and sustain them are weak agents for labour productivity growth in Africa. The current policy ignores one key necessity for labour productivity growth: the kind of growth that will trigger successful migration out of agriculture and rural areas. In the final part of the paper, we discuss some of the challenges of an appropriate institutional setup for the emergence of a more dynamic agriculture with scope for investment in larger scale commercial agriculture. We contrast this with the recent African vogue for ‘mega-farms’. We argue that while commercialization of African agriculture is desirable, the mega-farms are fundamentally geopolitical rather than commercial, and are therefore not an appropriate vehicle for African societies.

1. IS AN EXCLUSIVE COMMITMENT TO SMALLHOLDERS WARRANTED?

In this section, we discuss the case for a focus on smallholders as the preferred mode of production. The lens taken is that of (static) efficiency and the presence or lack of scale economies. In the next section, the focus is more strongly on dynamic arguments: the role of smallholder agriculture in growth and in poverty reduction. There is plenty of evidence that poor smallholders are quite efficient in what they do. This view of ‘poor but efficient’ was powerfully promoted by T.W. Schultz, who famously stated that “(t)here are comparatively few inefficiencies in the allocation of factors of production in traditional agriculture” (Schultz, 1964, pp-37-8). In itself, this is not a justification for focusing on smallholders as the agents for growth in agriculture, as other modes of production may be better at shifting the technology frontier. The empirical argument in favour of smallholders over large scale production tends to rely on the ‘inverse productivity’ relationship, going back to Chayanov (1926), but found to be present across a wide variety of contexts: that yields or output per hectare are higher on smaller farms. To explain this, standard explanations focus on labour supervision costs making hired labour expensive relative to family labour and reducing land productivity on larger farms (Eswaran and Kotwal, 1986). They are usually considered the most plausible explanation for the inverse productivity relationship, although other market failures, such as failing insurance markets, could also deliver this result (Barrett, 1996; Barrett et al., 2010).

Against this, there are good theoretical reasons why market imperfections would actually result in scale economies in agriculture (Eastwood et al., 2008). Reasons include lumpy investment (e.g. machinery, oxen) or working capital needs. For example, Eswaran and Kotwal (1986) use the latter to argue that the smallest farms may be less efficient if collateral requirements affect their ability to raise working capital. In several settings, there is evidence that these factors matter (Eastwood et al. 2008). The result is that any empirical regularity regarding the inverse productivity relationship requires that these sources of economies of scale are outweighed by plausible market imperfections. And even if it exists, the specific market failure driving the result is important to understand. For example, if small farmers seem more efficient because of insurance market failures, trying to fix insurance markets would be the first best solution – not promoting smallholder agriculture.

Descriptive statistics (e.g. showing higher profits per hectare on smaller plots in national farm surveys) are not particularly helpful as agro-climatic and especially soil quality differences should at least be controlled for. There are (only) a handful of reasonably careful studies showing the inverse farm-size/productivity relationship in African settings (including van Zyl et al., 1995 for South Africa; Kimhi, 2003 for Zambia; Barrett, 1996, for Madagascar; Larson et al., 2012 across a number of African countries) but also some showing the reverse (i.e. positive) farm-size/productivity relationship (e.g. Kevane 1996 for Sudan; Zaibet and Dunn, 1998, for Tunisia).

The evidence is definitely not without its problems and is still attracting academic research, even questioning its existence in settings in which it had previously been taken for granted. Factors such as unobservable land quality, selection issues and measurement error could plausibly account for the evidence in data sets such as the ICRISAT village level data for Southern India (Assunção and Braido, 2007). Barrett et al. (2010), while recognising the presence in the data of the inverse-productivity relationship in Madagascar, argue after eliminating other explanations that the most plausible explanation is measurement error.

Furthermore, it is not clear how much of this evidence really tells us about yields of large versus small farms, in any meaningful sense of ‘large’. One key issue is that the nature of the data examined for most investigations of

the inverse productivity relationship in Africa cannot really tell us much about the yields of large farms: most farms in these data sets are really quite small. Overall, the vast majority of farms in Africa are below 2 hectares, with median farm size near 1 hectare in most countries (Eastwood et al. 2008; World Bank, 2007). Few large farms above 5 hectares, are included in these data sets. The inverse productivity relationship is a reflection of efficiency and relative success of the small farm among *smallholder* farms, and this literature is then essentially merely a critique of imperfections in factor markets *within* smallholder agriculture. As a result the evidence that large farming is inefficient is based on extrapolations outside the range of the data.¹

Also, yield is not the relevant variable to assess efficiency of agriculture, or its likely contribution to growth. A more relevant indicator would be value added – returns to land and labour, after intermediate inputs – per unit of land. The evidence base here is much weaker and struggles to find appropriate ways of using reasonable shadow prices for labour, including agency and supervision costs that may well be at the root of the inverse productivity relationship. Rosenzweig and Wolpin (2011) study the value added and profitability of additional land brought into cultivation in India, and finds that smaller farms in their data are less efficient. They argue on the basis of their evidence that with 95 percent of farms in India below 5 acres in terms of owned holdings, efficiency considerations would suggest larger farms are more profitable and a trade from smaller to larger holdings would be profitable.

Overall, there are clearly some real challenges in the evidence base arguing for an efficiency based argument favouring of smallholder agriculture; if anything, the persistent emphasis on the inverse productivity relationship in the debate on large versus small scale production is methodologically flawed. This does not mean that smallholders are *not* reasonably efficient in what they do, given the market failures and other constraints they face. The current policy model focuses then on overcoming these constraints for smallholders by a set of policies including extensive interventions in the relevant markets and support services for smallholders, including inputs, extension and finance. But is this model really sufficient to achieve agricultural transformation and rapid production increases, or should other modes of production be considered?

We identify three key areas of potential economies of scale that would suggest the current model is flawed.² They are: skills and technology, finance and access to capital, and the organization and logistics of trading, marketing and storage. These scale economies are not intrinsic to the size of the farm, but rather to a switch in the form of organization from informal and personalized, to formal and institutionalized. The key benefit of size is that it facilitates (though is by no means synonymous with) *commercialization*. Large, personal ‘grandee’ farms are liable to be inefficient. Larger commercial farms are likely to be close to the frontiers of technology, finance and logistics. The innovations of recent decades have made the rapid adaption of technology, access to finance, and high-speed logistics more important, and in the process given commercial agriculture a substantial advantage over the smallholder mode of production.

Skills and technology

Scale economies linked to technology are probably the most disputed but also most misunderstood area in the discussion on the virtues of smallholder agriculture. There are plenty of examples of dramatically failed projects of large scale production in agriculture, in Africa and elsewhere (Byerlee and Deininger, 2011). Some are likely to have more to do with the nature of the organisation (large scale state farms in Tanzania, Ethiopia and elsewhere in the 1970s and 1980s), rather than with factors intrinsic to the technology of agricultural production. Others, such as the infamous groundnut scheme in Tanganyika, Kenya and Northern Rhodesia, started in 1947, tried to import an entire mechanized production technology to areas unsuitable for the technology used. Many

¹ How should one then research this issue? We could think of pooling data on smallholders with much larger commercial farmers from otherwise similar settings, if we can find these, and fit a production or yield function to them to investigate the farm size productivity trade-off. For good reasons, few researchers would do this, as it would assume that the ‘functional form’, i.e. the production technology of such wildly different farms, would actually be the same, even if we manage to control for differences in market failures faced. In short, most would admit that the production processes cannot easily be compared, but still evidence *assuming* the same technology is used to settle the discussion on large farm enterprises compared to smallholder peasant agriculture.

² Poulton et al. 2005, or Eastwood et al., 2008 offer longer lists of potential economies of scale in agriculture.

had to do with institutional forms with heavy government support that were more driven more by political considerations than solid economic analysis. These dismal experiences are not the same as evidence of diseconomies of scale – but more that the specific large scale farming experiments of the 1970s and 1980s are not appropriate examples for repetition. A more important question is whether alternative institutional arrangements can offer a suitable mode of organisation for productivity and value added growth.

The underlying requirement for productivity growth via new technologies is obviously the existence of these technologies, such as in the form of improved seeds or fertilizer. From a strict production point of view, most of these are scale-neutral: there are no agronomic reasons why an HYV seed would grow better as part of a 1 hectare or 50 hectare farm. However, the step from the availability of new technology to its adoption and efficient use is a large one, and innovations tend to spread slowly (Alston et al. 2008).³ There are at least two reasons why larger scale farms may be better at the process of adoption: handling knowledge diffusion and managing adoption risks. This is very relevant at the moment, as much attention is going towards stimulating a ‘green revolution’ for Africa (Sachs, 2005).

Knowledge is a classic scale economies activity replete with externalities. As is well known, any innovation process in agriculture involves *learning*. For example, what is needed by the farmer is adoption of the new technology, but often in conjunction with some adaptation (making the technology work in the specific local context). Most learning in smallholder agriculture is based on extension (mostly via ‘model’ farmers or village-based field trial plots), on social learning (copying from others) and on combinations of both. Effective learning involves complementary skills such as managerial skills, good numeracy and basic science understanding, and it is commonly observed that there are strong impacts of education on innovation in agriculture (Foster and Rosenzweig, 1996; Haddad et al. 1991; Bandiera and Rasul, 2006). The scarcity of these skills combined with the diverse but specialised skill requirements, make it costly for smallholders to acquire them. Larger organizations are better able to internalize these costs, allowing faster learning. Furthermore, if learning takes place via ‘social’ learning or copying, then ‘noise to signal ratios’ may go high, i.e. information flows with error, and information may be poorly transmitted across a large community of smallholders. Again, in a larger organization, learning may be organized systematically with less ‘noise’ in learning. As a result, in terms of learning, a larger organization may be able to diffuse knowledge much more cheaply, effectively and quickly.

A related issue is that innovation involves risk: entering into something new may involve the need for experimentation and trial and error. As a result, in a community, there are strong incentives to wait until others have tried innovations: one can then pick and choose what really works, with less risk of failure and therefore lower costs. However, the incentive to experiment is then low, and no-one may have an incentive to adopt first, resulting in zero innovation. The underlying problem is effectively a public good problem, resulting in underinvestment in the public good as no-one wants to bear the costs but aims to grab the benefit. Larger farm enterprises can *internalize* these processes: it can afford to use some of its plots for trial and error – and then adopt soon afterwards the successful innovations.

Finance and access to capital

Besides economies of scale in innovation and technology adoption processes, commercial farm enterprises have further distinct sources of increasing returns where scale and organization matters: in terms of finance, but also in the organization of production and marketing, such as related to logistics, marketing and storage. These arguments are not simply related to the size of the landholdings of the farm, but also of the nature and scale of the organization of the farm as a commercial enterprise.

³ Recent work by Alston et al. (2008) on agricultural R&D in US agriculture finds argues that it typically takes around 30 years for a new variety to be widely adopted in the field by smallholders. This average masks a wide variety lag lengths, and there is some suggestion that R&D lags are getting shorter. Even so, one would hardly expect developing countries to shorter R&D lags than US agriculture.

The advantages of finance are not simply related to the standard argument that small farmers do not have enough land (or land without enough security) to offer collateral to acquire necessary working capital, while larger farm enterprises may not face these constraints (Eswaran and Kotwal, 1986). While it is probably correct that larger farms have better access to collateral, it is hardly a good argument to favour larger farms, not least once the supervision costs of labour are taken into account: resolving the market failure in credit markets (for example, by using microfinance style organizations), or, as a second best, redistributing land from larger to smaller farmers, as well as allocating property rights to small farmers may in fact be efficiency enhancing (Eswaran and Kotwal, 1986). The argument in favour of larger farm enterprises with respect to finance, however, is not just about collateral but rather about institutionalization. Like any other commercial organization, a commercial farm builds documented and vetted evidence, such as audited profits and asset valuations that support the accumulation of reputation. It is able to raise capital in a range of complementary forms: equity, bonds and bank borrowing. All this lowers the transactions costs of finance and makes continued access to finance in the face of shocks more likely. Two characteristics of agriculture suggest that these advantages of commercialization are liable to be more pronounced than in industry and services. First, agriculture has unusually long lead times between input purchases and output sales, and hence has recurrent, relatively longer term financing needs. Second, agricultural production is more shock-prone and so reliable access to cover shocks is more important.

Organization of logistics of trading, marketing and storage

Finally, larger scale operations can exploit the presence of economies of scale not just in production, but probably most importantly, in trading, marketing, and storage. Storage, wholesale trading and marketing are characterized by technologies that involve economies of scale, via capital but also via the internalization of information. One of the most striking consequences of the model to promote rapid growth in smallholder production is the inherent weakness of the entire interface between producers and the final product market. The underlying 'markets' model is focused on being complementary to smallholders, with the idealized market a large number of relatively small wholesale traders competing across the country and thereby delivering 'efficiency' to markets, with a myriad of small retailers in cities. For both historical and competitive reasons, large traders are viewed with suspicion. Much has been written about stimulating agricultural trade but agricultural markets remain thin, and many have remarked in need of concerted action (Poulton et al. 2006; World Bank, 2007). Poor infrastructure and capital constraints for investment by traders are just some of the arguments proposed to explain these problems. The result is nevertheless continuing high transactions costs in agricultural markets, combined with large price fluctuations, affecting incentives for smallholder productivity growth. The result is also that in most African countries, despite the liberalization of most internal agricultural markets, a variety of donor-supported government initiatives and interventions take place, including in information sharing, storage and credit, again within the simple model of achieving the 'perfect' market with large numbers of small traders. Many have remarked that before the wave of liberalization in the 1980s and 1990s, marketing boards performed many of these functions and some have even called for their reinstatement as the solution (for a discussion, see Barrett and Mutambatsere, 2009).

The underlying model of using government and donor support to encourage the emergence of an agricultural market with many relatively small traders competing is clearly sensible from a static efficiency point of view, but is in denial of inherent returns to scale in the organization of markets. These returns to scale are typically and effectively repressed in these countries, but clearly are a defining feature of agricultural markets in most of the OECD countries. A relatively small number of operators in markets with scale economies ought not to be at the cost of high efficiency and welfare losses, provided there is appropriate competition policy.⁴ Larger scale private trading and marketing could reduce costs, possibly via vertical integration or at least coordination, even up to the production side.

Smallholder agriculture is not necessarily inconsistent with the exploitation of scale economies higher up in the value chain, but it does not lend itself to the task. Finding ways to get various more dynamic forms of

⁴ Textbook principles of competition policy would suggest that competitive pressures from ensuring potential entry, so that new entrants could potentially enter without being deterred by predatory practices in pricing, investment or other practices, are much more important than the number of firms operating in the market.

organizations to flourish, including large scale production operations, or contract farming structures, as well as promoting the exploitation of economies of scale throughout the value chain, is bound to provide for a more dynamic agricultural sector. When seen as such, this is not just about the size of the landholdings of producers, but looking for dynamic private farm and marketing organizations, responsive to incentives for growth.

Despite the systematic promotion of the smallholder model, combined with small traders and numerous government support services, the pressures for more dynamic organizational structures are clearly already present in Africa. Globalization has created opportunities for a rapid growth in particular niches of exports, such as in flowers or vegetables, which are taking place on commercial farms, sometimes supplemented with smallholder contract farming, with considerable vertical integration, from production to storage and transport. These processes are largely driven by consumer demands for more standardization and certification, leading to innovations in organizational structures and increased use of long-term contracts at various parts in the supply chain (Pingali et al. 2005).

Increased scale economies in retailing are a central part to this, also affecting Africa: the emergence of supermarkets throughout Africa is also bound to start changing the relationship with farmers as they actively seek vertical integration (Reardon et al., 2003). It would be a mistake just to look at these more commercial organizations with suspicion, or treat them as ‘another sector’, needing to be kept away from the smallholder sector, as it would thereby minimize the incentives such organizations could have on productivity growth in the smallholder sector.

Keeping them apart is the real mistake: the incentives from long-term contracts and the need for standardization and certification can have large dynamic efficiency gains not least via innovation and knowledge transfer. Evidence from other areas, including from India or transition economies, has shown that vertical integration and coordination is accelerating but also that it can have high returns (Swinnen, 2005; Pingali et al., 2005), and should not just be treated with suspicion.

Does this mean the end of family-operated farm businesses? Actually, not at all. In the literature there is a tendency to equate smallholders with family farm business. In fact, the vast majority of farm operations in the developed world, including very large ones, are family-run business. In the US, for example, about 98 percent of farms are family business – farms in which a particular family holds the majority shareholdership (Hoppe and Banker, 2010). But these are hardly smallholders: the median farm size among these family-run business is 28 hectares. The use of family labour, including for management of these farms, is not surprising, as the spatial monitoring problem for work in teams is fundamentally different to work on the factory floor in a relatively small space. Family-run businesses offer some solution to the rising agency costs (in terms of monitoring and supervision of labour) that is typical of larger farms. We can envisage African agriculture thriving on family business, but not on a permanent commitment to small farms.

Finally, does this mean that any commercial ventures involving larger scales than currently prevalent in agriculture are necessarily going to deliver on this potential for productivity growth and efficiency gains? Not necessarily, and no doubt more evidence is needed on their potential and to learn lessons for appropriate institutional and regulatory frameworks. But the key conclusion from this section is that the evidence base in favour of smallholder agriculture from the point of efficiency is not at all as strong as suggested, so as to dismiss a potential broader vision of a variety of modes production in Africa.

2. GROWTH AND POVERTY REDUCTION: A FOCUS ON SMALLHOLDERS?

Besides a strong believe in the efficiency and dynamism of smallholder agriculture, a commitment to the exclusive smallholder model is often also inspired by a conviction that as poverty is concentrated in rural areas among smallholders, it is essential that any policy that aims to result in lower poverty must start with smallholders. Furthermore, this is argued not to be inconsistent with an overall growth agenda, as growth starting among smallholders is suggested to have far higher growth ‘linkages’ than growth in any other sector (e.g.

Mellor, 1995). The results in what would seem to be an infallible logic: promoting smallholder agriculture in Africa will lead to growth and reduce poverty better than any other policy. For example, the recent World Development Report 2008 on agriculture stated that stimulating agricultural growth is “vital for stimulating growth in other parts of the economy” and smallholders are at the core of this strategy (World Bank, 2007, p. xiii).

There are more problems with this model than generally tends to be acknowledged. Simply arguing for a sectoral focus because this is where most people or the poor are located is surely a non sequitur: what we ought to aim for is to offer people income earning opportunities while resources in the economy are allocated where the opportunities are highest, and there is little reason to suggest that this must be where labour is allocated at present. Furthermore, taking a longer term view, we know that if we want to have achieved growth and poverty reduction in Africa, we ought to have massive increases in labour productivity and this is bound to include far fewer people in agriculture or in self-employment in general: poverty reduction tends to involve an increasing share of lower skilled people obtaining secure wage-earning jobs.

The evidence for starting growth in smallholder agriculture is usually based on ‘linkages’ research, arguing that production and especially demand linkages are stronger from agriculture than any other sector, so that promoting growth in agriculture has the highest multiplier effects (Mellor 1995). If true, this suggests that agricultural growth will tend to stimulate production in other sectors, via its demand effects. This is indeed argued to be the case for Africa (for a review, see Staatz and Dembele, 2007), a view reflected also in the recent World Development Report on agriculture (World Bank, 2007). The evidence is, however, far weaker than often suggested, and the methods used typically cannot establish any causality, not least in terms of trying to understand where the growth originates (for a detailed review, see Dercon, 2009; Gollin, 2010).⁵

In a careful discussion, Gollin (2010) assesses this view in more detail, calling it the “agro-pessimism” view, in contrast to the “agro-fundamentalism” view of Mellor. “Pessimism” may not be a good term to use here, as the argument is not to ignore agriculture and its contribution to growth. In particular, this is not to argue that growth in agriculture is not important to the economy, but rather that it ignores how this growth can come about – and that growth dynamics in agriculture typically depend on growth in demand, stemming from other parts of the economy.

Wiggins (2000), for example, suggested that any period of rapid productivity growth in agriculture resulted from demand-pressures, in the form of better prices often linked to urbanization and infrastructure. This is (not surprisingly) similar to the evidence on yield growth via innovation in agriculture in England around the time of the industrial revolution: Allen (2009) shows that this largely stemmed from rising food demand pressures resulting from urbanization, with labour-saving technologies developed to offset rising wages.⁶

⁵The existing literature depends on Social Accounting Matrices, often difficult to compile without much guess work, which are then used to assess linkages. Causal relationships are poorly established, and the evidence, while interesting, can hardly be used to assess what are the actual drivers of growth. Consider for example a recent study by Diao et al. (2007), who present a pair of economy-wide models for Ethiopia, based on parameterizations of input-output coefficients, demand elasticities, and other estimated parameters of the economy. These models are used both to explore the growth impacts of a given, *exogenous* rate of technological improvement in one sector or another, and to compare growth rates required in specific subsectors in order to achieve a given target rate of growth for the economy as a whole. However, these models do not set out to consider the *causes* of growth in these agricultural subsectors (where would these exogenous changes come from?); nor (since they generally make simplifying assumptions about the elasticity of supply from industry or other sectors) do they assess the tradeoffs in resource allocation and the complementarities in demand and production implied. Studies that rely more on econometric evidence do not fare necessarily better. The econometric evidence is affected by simultaneity problems, with little scope for convincing use of instrumental variables, hardly resolved by using panel data (World Bank 2007). It is a research programme with serious inherent difficulties that is not offering the evidence required (Gollin, 2010).

⁶Even the often quoted examples from Asia such as Taiwan or Korea are hard to interpret as if there was a clear sequence from first agriculture and then the rest will follow: other economic processes in other sectors were clearly present and without these it is hard to see how success could have been achieved. Despite this, Bezemer and Headey (2008) have offered a reading of the evidence that stresses agricultural growth preceding growth in other sectors. Nevertheless, all these

Most of the reasoning underlying the view that growth must start in the agricultural sector, whether from ‘linkages’ discussions or more formal basic models such as the Lewis Dual Economy model, are also effectively closed economy models. This seriously qualifies the applicability of the results to current policy. The changing context of globalization, but also of gradually improving infrastructure within Africa, makes it unlikely to be a valid assumption, and, as is well-known, most policy results in favour of a strong bias towards agriculture depend on closed-economy assumptions. For certain landlocked economies in Africa with difficult relations with their neighbours, such as Ethiopia, these are reasonable assumptions, but the future comparative advantage for natural resource rich or coastal economies is unlikely to imply that agricultural production will have to lead the growth process, let alone that it should be led by smallholders (Dercon, 2009; Gollin, 2010). Furthermore, it would be hard to claim that the current geographical spread of smallholder agriculture and food production is likely to be the optimal spread of agriculture in a globalizing world facing climate change.

But the argument in favour of promoting smallholders because of its *poverty* impact remains, even if nested within an overall growth strategy that makes agriculture important but not the key sector. For this to matter, we need to focus on labour productivity, as it is directly linked to earnings possibilities. Here, the record for African smallholder agriculture is dismal, with FAOSTAT data suggesting that by 2005, it was still below \$500 US per worker for the vast majority of African countries, and growth in labour productivity has been lagging output growth since the 1960s. Maybe this is not so surprising, as agriculture is typically characterised by low labour productivity. As Gollin (2012) highlights, productivity per worker outside agriculture is twice the productivity per worker in agriculture across all developing countries, after careful attempts to correct for multiple possible causes of measurement error. So as a long-term poverty reduction strategy, it is then not self-evident that agriculture is the sector that most effectively will reduce poverty – it has to do this starting at a very low labour productivity. The large gaps also suggest that the process of labour movement from activities with lower productivity to higher productivity is insufficiently functional. One could argue that the most intuitive process of closing this gap would be to encourage more (labour) resources into the high return activities, taking away from the low return resources, as a means of bringing down this gap.

So we need growth that allows agriculture to start engaging in a process of releasing labour via migration. Growth in the rest of the economy can induce this movement, but it is also important to get labour markets further integrated so that labour productivity gains elsewhere are transmitted across the economy into the rural sector. It is not altogether clear that this is indeed happening in the current agricultural sector dominated by smallholders. Some recent evidence from Tanzania shows migration in action but also how linkages back to the smallholder sector are not delivering much poverty reduction in the rural sector.

Table 1 offers an interesting snapshot of what migration currently means for a poor population. The table (from Beegle et al. 2011) is reporting on a rather unique longitudinal survey. In 1991-94, a survey took place in Kagera, a region in Tanzania near Lake Victoria, in which about 800 households were surveyed. In 2004, a new round took place, but not simply the usual revisit of the same ‘households’ in the villages they were initially resident. This time all the individuals that were members of any of the original households were tracked, wherever they were. This meant that it became an individual panel data survey, and not just confined to the original villages. In fact, 43% of the surviving individuals were found in other locations, some not too far away, but others hundreds of kilometers away in urban centres across Tanzania and in neighbouring countries. Overall, about 87% of the surviving individuals were traced, resulting in a sample of several thousand households in which these individuals were now residing.

The data provide a relevant perspective on the changes in poverty of this population (based on the consumption per capita of the household in which the individual lives). Table 1 reports poverty headcount levels using a

views continue to have in common that growth in agriculture and in the rest of economy tends to be largely a concurrent event (Gollin, 2010).

poverty line not dissimilar to the national poverty line in 1991. We report poverty at baseline (1991) and poverty in 2004, and the difference between these levels and its statistical significance. Overall poverty went down from 35% to 27%. But if the survey had been using ‘standard’ techniques, in which only households and individuals were traced in the original village (e.g. by homestead) then poverty declines would have been far lower, from 36% to 32%. Even more strikingly, the further someone had gone the larger the poverty decline. Those moving out of Kagera experienced the largest declines from 30% to 7%. The data also showed that moving from rural to urban areas had the highest impacts, as well as combining migration with moving from agriculture into non-agriculture as a main activity.

Of course, this is not the same as arguing that migration caused this poverty decline. For example, the standard argument against this is that those who moved were systematically more able to earn higher incomes so the impact of migration is overstated. If they had stayed in the village they would have been better off as well. Against this, for our purposes, it can simply be noted that a remarkably large percentage of the population moved, and that the migrants are doing particularly well: they did migrate, even if we may believe that they could have done well in their original location. Beegle et al. (2011) analyze these data further with these concerns in mind and find that *ceteris paribus* migrants have 36% higher consumption than similar non-migrants.⁷ The improvement in living standards of this previously largely rural based population living off smallholder agriculture was not simply transmitted back into the smallholder economy – earnings seem to remain lagging with limited poverty reduction for those who did not manage to escape.⁸

Table 1: Consumption poverty headcounts and spatial mobility in Kagera, Tanzania

<i>2004 location categories</i>	<i>mean 1991</i>	<i>mean 2004</i>	<i>difference means</i>	<i>N</i>
Remained within village/neighbourhood	0.36	0.32	-0.04***	2611
Migrated to nearby village/area	0.33	0.22	-0.11***	566
Migrated elsewhere in Kagera	0.37	0.24	-0.13***	571
Migrated out of Kagera	0.30	0.07	-0.23***	327
full Sample	0.35	0.27	-0.08***	4075

Source: Beegle et al. 2011 (***=difference significant at 1%)

This discussion also offers another perspective on the problem of focusing too strongly on smallholder agriculture simply because this is where poverty is located. Rural poverty cannot be looked upon in isolation, and migration in search of a better life has to be seen as an essential and necessary part of policy towards these areas. The evidence above suggests that migration on average has strong effects on poverty. An active policy to reduce poverty which focused on rural areas because this is where people live, or tried to keep people there because of imagined adverse effects of migration, would seem poorly conceived and may even remove a key option for poverty reduction. This is not to say that the marginal return to spending on migration opportunities will always be better than spending on agriculture, but it is a choice that should be considered explicitly, not

⁷ Beegle et al. (2011) analyze these data further, and suggest that controlling for individual and household fixed effects and a large number of covariates reflecting earning ability, migration still has a strong impact on consumption and poverty. There are some signs that more people leave from families with higher earning ability, but the return to migration controlling for this is still approximately 36%. The evidence shows that moving from rural to urban had highest impacts, as well as moving from agriculture into non-agriculture as a main activity. However, even controlling for these changes, spatial mobility per se still contributed independently to consumption changes.

⁸ De Brauw and Muller in this volume offer a further discussion of the poverty impacts of migration.

least as the share of the population living off agriculture will have to come down during economic transformation.⁹

But how does this all square with a common argument: growth driven by agriculture is more effective in reducing poverty than growth elsewhere? The most commonly quoted evidence for this is from Ravallion and Chen (2007) for China, suggesting that growth in the primary sector (mainly agriculture) had four times as large a poverty effect as growth from any other sector. External validity of results from the experiences of a vast and relatively closed economy (at least, for agriculture) to African countries would obviously be difficult.¹⁰ Still, using similar methods, Christiaensen et al. (2012) reach conclusions that growth in agriculture has higher poverty effects across sub-Saharan African (SSA) and non-SSA countries, compared to other sectors, although there is considerable heterogeneity linked to the level of income of countries, to existing inequality, to whether they are resource-rich or not, and to whether absolute poverty below \$1.25 per day per person or \$2 poverty levels are considered. Moreover, the average effects estimated in Christiaensen et al. (2012) are (considerably) smaller than the Ravallion and Chen (2007) results for China. Still, this should not come as a surprise, and it is entirely plausible and reasonable that growth in smallholder agriculture *has* considerable poverty effects: this is a sector that is both using low skilled labour intensively and involves a lot of people, so if one manages to boost overall growth in that sector, it is bound to have relatively large poverty effects at the margin¹¹ – in contrast to, say, natural resource extraction, which involves few people for large value added.

But this is not answering whether this means we should actively invest in agriculture, even from the point of view of poverty reduction. To answer this, the cost of investing in agriculture ought to be taken into account: if the costs are several times higher to get a one percent GDP growth in agriculture than in non-agriculture, it may still mean that investing in the latter may be superior for poverty reduction at the margin. Indeed, it is important to highlight where the growth increase will come from, and at what cost – and studies like Christiaensen et al. (2012) don't quantify this.¹² And, as part of the dynamic story of structural transformation, it is still a sector with fundamentally lower labour productivity than other sectors, so this piece of often quoted empirical evidence has limited direct relevance to suggest what ought to be done. A growth process that will allow labour to be released from agriculture is likely to be required for large scale poverty reduction; whether this must start with large investments in smallholder agriculture is hardly proven (see also Gollin (2008), Dercon (2009)).

3. COMMERCIALIZING AFRICAN AGRICULTURE

The main lesson thus far is that a bias in favour of smallholders is unlikely to be optimal for growth. Even for poverty reduction, especially in the long-run, it is not altogether clear that a focus on smallholders is most effective relative to alternative strategies. This does not mean that the route to more commercialization of agriculture is easily achievable, and will require considerable change in the policy, institutional and regulatory frameworks. To what extent should governments be actively involved in supporting the emergence of commercial agriculture? Historical evidence on large scale agriculture in Africa and its political economy will

⁹ It is likely that at low levels of migration, potential migrants are ignorant of the benefit, and they lack the social networks to access migrant jobs. In that sense, the first migrants provide positive spillovers. So if anything, migration should be subsidized. Instead, many governments implicitly tax it, and a few have explicitly taxed it. For an interesting policy experiment offering subsidies to migration in Bangladesh, see Bryan et al. (2012).

¹⁰ External validity of these results for China is likely to be limited, for several reasons, including that China's rapid agricultural growth in the 1980s (probably the fastest in history) was the result of the removal of massive distortions in agricultural production, it started with very low land inequality and retained huge restrictions on moving to cities. Those are three conditions which help to maximize the poverty impact of agricultural growth: a huge number of people trapped in a highly distorted sector, all with similar amounts of land, and all exposed to a massive one-off productivity shock related to market reforms. We are grateful for the editor for pointing this out to us explicitly.

¹¹ Indeed, this is the interpretation offered Christiaensen et al. (2012).

¹² On the cost-effectiveness of agricultural investments, see e.g. Alston et al. (2003),

remain an important warning on what *not* to do, with prestige projects and excessive government involvement typically resulting in dramatic failure (Byerlee and Deininger, 2011).

More recently, some African governments have been surprisingly willing to entertain ‘mega-farm’ deals allowing ‘landgrabbing’. In these deals investors, often encouraged by foreign governments, take a very long lease on a huge area.¹³ Is this the alternative? Not at all. The buyer motivation for these deals arose from the 2008 food crisis, or in a few cases, hedge fund bets on rising land prices in the wake of that crisis, with many of these investors taken speculative ‘options’ on future price rises. In practice, the evidence on these deals remains vague, and reported projects often appear not to have ever been implemented (Byerlee and Deininger (2011), Cotula et al. (2009)). Even so, it is legitimate to ask whether such mega-farms offer a sensible approach for an African government? While we have argued above that there is a good case for commercial agriculture, at a larger scale, this does not extend to mega-farms. First, the notion of a lease for many decennia is inappropriate given the political and institutional context: there is no credible basis for such very long term commitments, and so the deals are highly likely at some future date to be revoked. Rather than enter into deals with such a high probability of being broken, it would be better to adopt a time horizon that is more realistic. Second, whatever the scale economies in commercial agriculture, they are highly unlikely to warrant the creation of a huge entity that would inevitably be a monopsonist in local factor markets. Such monopsony positions may be commercially desirable for the leaseholder, but are neither efficient nor advantageous for the host society. Third, the resulting organizations would be too large to be normal commercial entities. Their rationale is essentially geo-political rather than commercial. Their scale reflects the desire to lock in a sufficiently massive amount of output to achieve food security in the leaseholding country even if this involves a sacrifice of efficiency. Mega-farms are thus more analogous to imperial organizations, such as the groundnuts scheme, than to a globalized commercial agriculture. Fourth, and most crucially, the processes by which leases have been secured are not competitive. Rather, they are firmly in the tradition of geopolitical deals, with an African ruler, sometimes of limited legitimacy beyond physical control of a territory, mortgaging the distant future in a transaction that is opaque and arrived at through a private negotiation. This is not the right approach for the commercial exploitation of a natural asset such as land. It offers too much scope for corruption on the part of the political leaders negotiating the deal, which may indeed account for the willingness of African leaders to entertain the idea. Further, the process is liable to undervalue the asset because the buyer probably has a better idea of potential than the seller. Both problems leave society receiving less than the full social value of the land.

This route to commercialization, with political considerations and patronage dominating the need for competition and transparency are unlikely to offer scope for externalities for the required transformation of agriculture. Different institutional frameworks will be required. Collier and Venables (2012) offer a detailed discussion on how to value these deals and how to ensure that incentives are aligned for larger scale commercial land deals to translate in actual productivity-enhancing investments, focusing on terms at which governments may sell or lease out land: the price, the duration, the conditions imposed on purchases, and the associated commitments made by government. Deininger and Byerlee (2011) focus *inter alia* on the importance of clear property rights for land, on how existing local claims on the land should be compensated, sustainability issues and the consequences for local employment and incomes. The prize for developing appropriate frameworks is likely to be substantial, galvanising much needed new productive investment into agriculture.

¹³ China led the way with a multibillion plan to develop agricultural assets in Africa. Qatar, Abu Dhabi and Saudi Arabia are also actively involved. Headline grabbing private deals such as Daewoo Logistics of South Korea to lease about half the land of Madagascar fuelled the opposition against President Ravalomanana, leading ultimately to a military coup. Another controversial deal is the investment by Heilberg to acquire 400,000 hectares in Mayom district in Southern Sudan by in deal with the warlord and deputy commander of the Southern army. Whether these deals materialise into actual farming is nevertheless not always clear.

4. CONCLUSION

Is it time to revisit the development model for agriculture in Africa that argues for a narrow focus on smallholders? We think so. Revisiting the evidence base, we argue that a focus on smallholder agriculture for growth is not proven. Furthermore, there are good reasons to suspect that larger farmers have an important role to play in experimenting and pushing the technological frontier in agriculture. Similarly, there are likely to be important scale economies higher up the value chain, in logistics, finance and marketing, suggesting that a focus on small trader marketing arrangements are unlikely to be most effective.

Even from the point of view of poverty reduction, the mere fact that a focus on smallholders is required because they are poor is not likely to be dynamically effective in reducing poverty. Economic transformation will be required, and a narrow focus on smallholders may not be a cost-effective route for transforming these poor peasants into a non-poor population. Instead, recognising that poverty reduction will involve creating opportunities to reduce the number of farmers, including via migration, opens up a broader array of policy intervention options.

So what is this alternative? We argue that development strategies need to shift emphasis and resources away from small farmer (and small trader) models and open up new forms of commercialisation. None of this will involve large state-led farms or geopolitically motivated megafarms, but it will require the creation of opportunities for serious, larger scale commercial investment in agriculture, and hybrid models in which smallholders interact with larger farmers and vertically integrated enterprises upwards in the value chain.

How to achieve it is less clear, and the evidence base for specific alternatives is lacking. Research has shied away from taken these alternatives seriously for a surprisingly narrow minded emphasis on the small farm – and re-building this evidence base on alternatives is a key requirement for better policy choices. Policy and institutional arrangements have responded to the failure of state-led models by retreating into either ignoring agriculture altogether or focusing narrowly on smallholders. Opening the space for experimenting with alternative policies and institutions is now essential.

African smallholders have not chosen to be entrepreneurs, they are in this activity by default. Having the single most important sector of Africa's economies almost exclusively run by these reluctant micro-entrepreneurs is a recipe for continued divergence of the sector from global agricultural performance, limiting growth and unlikely to help large scale poverty reduction. A renewed focus on agriculture as a dynamic sector with potential for commercialisation is necessary, if only to contribute to urgently needed economic transformation and a shrinking of the labour force dependent on agriculture.

Not everywhere will this transformation to a more diverse, higher productivity and commercialised agriculture with increased number of larger farms be achieved in the same way. Indeed, we would be the first to acknowledge the need to be agnostic on where and how exactly this will be achieved. Everywhere, politically sensitive choices have to be made on the nature of land rights, on the desirable size and composition of the large farm sector, and on facilitating migration. But ignoring these issues altogether will just slow down the required transformation of the rural economy. African agriculture is bound to look very different in 50 years; whether it will be a stronger and highly productive agriculture will depend on choices made now to make this transformation possible.

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