

Vicente Pinilla and Henry Willebald (Eds.)

**Agricultural Development in the World Periphery: A Global Economic History Approach**

Cham, Springer International Publishing/Palgrave Macmillan, 2018, 507 pp.

**M**y grandfather was a South African ostrich farmer. He managed to eke out a living in the hostile semi-desert of the Klein Karoo, enabling all of his seven children to complete school and attain some form of tertiary qualification. None of his children chose to remain on the farm, all (bar one) moving to towns and cities where most took up professions such as teacher or hairdresser. All of his grandchildren now live in cities or big towns.

My grandfather's brother was also an ostrich farmer on the neighbouring farm. His children and grandchildren chose to stay on the family farm, continuing the battle against the barren soil and sporadic rains that has come to characterise the area. Comparing the fortunes of the family who remained on the farm – now struggling for survival – to those who left for the bright lights of the cities – now doing fairly well,

even if one of them is a professor of economic history – is a harsh lesson in the promise of agriculture as a source of economic development.

It is against this background that I read *Agricultural Development in the World Periphery*, a book of 507 pages, 18 chapters and 29 authors. The book is an ambitious attempt to cover the structural transformation of agriculture over the nineteenth and twentieth centuries in regions that are typically regarded as developing (except for the inclusion of Australia and New Zealand): three in Africa, thirteen from Asia and one chapter on the entire Latin America. The length and breadth makes a comprehensive review impossible. Instead, I've opted for a very personal route.

My family's story would support the agro-sceptics, as defined by Martin Andersson and Emelie Rohne Till in chapter 2. In their review chapter of the main

theoretical contributions that explain agriculture's role in development, they argue that the agro-sceptics are those development experts that *question the idea that agricultural growth is generally efficient in reducing poverty* (p. 30). Think of the Arthur Lewis or Paul Rosenstein-Rodan set of models, in which labour was a valuable yet underutilised resource that could be re-allocated to the modern sector for the capital formation necessary for industrialisation. That theory fits the story of my father's side of the family.

It is not only a historical phenomenon. Urbanisation is now the most successful poverty-alleviating strategy in South Africa, as a recent Human Sciences Research Council study shows. Black South Africans who move from impoverished peasant farms to the cities, even if they end up living in tiny shacks in sprawling townships on the periphery of Johannesburg or Cape Town, are more likely to be socially mobile and escape poverty than their counterparts who remain in the rural areas.

Yet it is not that simple. One of my grandfather's sons (my uncle) decided that even though he did not want to be an ostrich farmer, he wanted to remain in agriculture. After leaving school, he went to work on a fruit farm closer to Cape Town. In contrast to what has happened to the ostrich industry, the fruit farm my uncle now manages has done exceptionally well. They have invested heavily in state-of-the-art technology, automating much of the harvesting process. They have expanded, buying up neighbouring farms and investing in warehousing and transport. The farm is

now an industrial enterprise, producing tomatoes and apricots and berries for both the national and international market, and its owners are multi-millionaires. Clearly, this is a case that would support the agro-proponents, as Andersson and Rohne Till would call those who argue that *agriculture plays a crucial role for both aggregate and pro-poor growth* (p. 30).

Which one of these stories holds true is, ultimately, the purpose of this book. The African case studies, perhaps predictably, are largely pessimistic. One example is provided by Christer Gunnarsson (chapter 5) who, in writing about Ghanaian cocoa production, draws on Hla Myint's version of the vent-for-surplus theory. He argues that while the arrival of cash crops in Ghana were initially complementary to subsistence farming, its success hinged on specialisation.

*One could assume that only the more successful and prosperous farmers, encouraged by their previous success, would decide to devote more of their time and efforts to specialised cash-crop export production. They acquire more land, by purchase or lease, they hire labour from outside the family, and they start buying food at the market. Their farms are likely to be bigger while at the other end of the spectrum the number of dependent small farmers would be increasing. Inequality would thereby surge* (p. 137).

Yet this process never did allow Ghana to experience the structural transformation that Asian economies, for example.

*Given the level of prosperity attained in the boom periods, one would not expect Ghana to have had worse preconditions for*

*structural change than a comparable economy like Malaysia. The economy was initially clearly unimodal in character, that is, land was fairly evenly distributed and possession of land appears not to have been a major source of political authority* (p. 147).

Today, more than fifty years after independence, its level of industrialisation remains low and the country is *as dependent as ever on primary commodity exports* (p. 147).

This result is, as Gunnarsson suggests, the disappearance of small farms. In some ways this is already happening; *the average cocoa farmer is ageing and the younger and more educated generation prefers living in urban and globalised environments to tilling the land of their ancestors*. The alternative is to *go for large-scale mechanisation of mega-sized farms*, but this is *a risky enterprise and it is not evident that it would be the best strategy for achieving macro-economic structural change*. In fact, large-scale agriculture received strong state support earlier in the century, with limited success. Gunnarsson thus recommends *a strategy supporting the medium-sized specialised farms*, although such a strategy, he concludes pessimistically, *remains to be seen. This is regrettable since, as suggested by historical evidence, it is often the safest way to incentivise macro-economic structural change by means of agricultural growth* (p. 146).

Ellen Hillbom and Samuel Jenkin (chapter 6) are equally pessimistic about agriculture in Zambia, the second African country examined in the book, as a catalyst for growth. They develop an analytical framework that rests on three pillars: 1) the

natural resources available and its location within the country; 2) the political and economic institutions within which policy-making is embedded; and 3) the agency of government officials, and commercial and subsistence farmers. The unique combination of minerals, notably copper, and its location relative to the most fertile regions have resulted in an alliance between commercial maize farmers producing almost exclusively for the mines and the urban population. Maize, they conclude, has become a *social contract crop*, with the result that *agricultural policies have distorted opportunities for widespread agricultural diversification, creating instead a dual agricultural sector* (p. 173).

As in Ghana, the state played an important role in shaping these outcomes, supporting commercial farmers in the interests of mining and against the interests of the broader economy:

*While we have seen a century of political and financial efforts being invested in encouraging agricultural growth, the fundamental role of the agricultural sector has throughout our period of investigation been to service the mining areas and growing urban population. With the government's consistent dependency on copper export revenues, Zambia remains caught in a reliance on two interdependent primary sectors, neither of which is dynamic enough to drive a structural transform process* (p. 173).

Jan C. Greyling, Nick Vink and Emily van der Merwe, writing on South Africa (chapter 7), also expose the interplay between the mining and agricultural sectors, and the political economy that ultimately

helped transform the agricultural sector into a successful export sector. With the discovery of gold in the South African interior in the late nineteenth century, much like for copper in Zambia, an alliance of maize and gold was established, lasting for several decades before it disintegrated. Using an analytical framework that includes both large-scale and subsistence white farmers, black farmers, black and white mine labourers, mine owners and the state, they describe the evolution of political tensions that stemmed from *the converging and diverging interests* (p. 179) of the mining and agricultural sectors. This dynamism is important, as it signals a shift away from what seems to be a static dichotomy between commercial and subsistence farmers, or between inward-looking versus export-led growth. The mineral revolution in South Africa, and the sudden large external market, *kick-started commercial farming through increased productivity and eventually an expansion to food exports* (p. 198). This gave farmers more political power, which disrupted the alliance between maize and gold, shifting state support to the agricultural sector. What may appear to be an agro-positive story is, in fact, more a case of artificial support for a politically-connected elite that came at the cost of both black labour on the mines (who would have benefited from lower prices of food, and thus higher real wages) and black farmers (who were displaced or dispossessed of markets).

Of course, *Agricultural Transformation in the World Periphery* is not only a book about African agriculture. Most developing

countries continue to regulate the sector or provide substantial state support. In chapter 9, Anne Booth writes that *economists continue to debate the extent and impact of interventions in markets for food and other agricultural commodities across Southeast Asia including Vietnam, Laos and Cambodia, where market reforms were introduced more recently and have had a considerable impact on output and yields of a range of agricultural commodities, including rice, coffee, rubber, sugar and pig meat. That government policies affecting the prices of both agricultural output and inputs have been important in encouraging the adoption of new production technologies is not disputed. But the longer term impact of these policies on both economic growth and equity within and between sectors is often controversial* (p. 247).

What are the government policies that may help? Kym Anderson (chapter 14), using the success of Australia to sustain a thriving agricultural sector, refers to four things: 1) availability of land; 2) significant declines in transport costs for agricultural goods; 3) innovations *both by farmers and via a strong public agricultural R&D system*; and 4) sound macroeconomic policies that avoided the resource curse. Future governments, Anderson concludes,

*may still occasionally provide some direct assistance to struggling firms in marginal electorates, but much more efficient social safety nets—and, even better, trampolines—are now available to assist the losers from economic growth to adjust to future sectoral trends and shocks* (p. 382).

Which brings me back to my grandfather's ostrich farm. Apart from a boom in

ostriches at the end of the nineteenth century, the industry would likely have struggled without the support that the Nationalist Party government of the twentieth century provided. Alternatives without state support were and remain limited, as the poor performance of this regions since the 2000s shows. Had there not been political interference, it is likely that my grandfather would have had to move elsewhere, either to towns or to other districts. Urbanisation may have happened one generation earlier, and the structural transformation of the economy too. Read through this lens, agriculture and the support of the state retarded rather than spurred South Africa's economic development.

But that does not mean South Africa should not have an agricultural sector. Investments in better transport (including air transport) and other infrastructure (including electricity, telecommunications and

water), research and development that develop improved crops for a changing climate, and generous but general support for those farmers (and workers in other industries) that fail to adapt to the global market, are key to a productive agricultural sector. On my uncle's fruit farm, access to local and international markets are key to their success, which means they must have access to efficient and reliable transport and communication infrastructure. Their farm is indeed an example of agricultural transformation in the global periphery. The question for many developing countries in a low-productivity spiral, like Ghana and Zambia, remains: how do we break the political alliances that prevent this kind of transformation from happening?

**Johan Fourie**

**orcid.org/0000-0002-7341-017X**

Stellenbosch University

Tariq Omar Ali

### **A Local History of Global Capital: Jute and Peasant Life in The Bengal Delta**

Princeton, Princeton University Press, 2018, 272 pp.

**J**ute sacks contained the commodities of world trade from the mid-nineteenth to the mid-twentieth centuries. As the world trade expanded, the demand for this fibre increased manifold. Within a few decades the Bengal delta had become the world's biggest jute producer, as the acreage increased from *ca.* 50.000 acres in the middle of the century to *ca.* four million acres in 1906 (p. 21).

In *A Local History of Global Capital* Tariq Omar Ali shows how this fibre had not only shaped the economic lives of thousands of peasant households, but also had been a central feature of political discourses in the region for over a century. The book is much more than just the history of jute in the Bengal delta. Through seven chapters (plus introduction and conclusion) Ali explores the economic, social and

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political history of the delta from *ca.* 1850 to 1950 by applying a commodity-centred approach. The first half of the book documents the rise of the jute industry from the Crimean War to World War I, and analyses its impact on peasant production and consumption, nationalist politics and the spatial relation between hinterland and metropolis (chapters 1-3).

Ali shows how closely jute acreage and price corresponded over time, which allows him to argue that the expansion of jute production was primarily the peasants' reaction to favourable market conditions. The cultivation of jute, Ali writes, *took place without colonial coercion, but entirely through peasant choice, through the autonomous and independent decisions of tens of thousands of peasant households in the Bengal delta* (p. 22). The Bengal delta's peasants simply took the opportunity the booming jute market offered them. This is what distinguishes the peasant production of jute from most other cash crops produced in the *global countryside* during the nineteenth and twentieth centuries. The production of most other raw materials cultivated in the hinterlands of the Global South was accompanied by some form of violence and coercion. Indigo or opium are just two examples of cash crops that were based on the coercion of peasant labour, executed by capitalists and the colonial state via the means of debt bondage or blunt violence. Jute cultivation, on the contrary, was entirely the peasants' choice and involved no colonial coercion, Ali argues (p. 6).

This argument is not entirely new and has been made in a similar way by Ali's

doctoral adviser Sugata Bose (1993). However, Ali's analysis goes beyond production and includes consumption and can thus explain why the peasants chose market-entanglements in favour of subsistence security, despite the risks involved. Their decisions to grow more jute instead of rice, Ali argues, *were driven by their desire for the various goods and services that they could purchase with proceeds from sales of fiber* (p. 38). The new income was spent on a wide range of consumer goods, from British umbrellas to fashionable haircuts, from clothes to metallic kitchen utensils, from more durable building materials to golden ornaments. A notable aspect of this new wave of consumption was the increase in lawsuits that tenants filed against their landlords. Backed up by the Rent Acts of 1859 and 1885, the peasants now also had the financial means to legally claim property on the land they occupied (pp. 41-46). In this part of the book Ali proposes an interesting thesis about the jute peasants' resistance to the *Swadeshi* movement. A central theme of the movement was the boycott of imported consumer goods – those goods the delta's peasants had come to enjoy buying at the weekly markets or seasonal fairs. The sometimes violent clashes between the (mainly Hindu) *Swadeshi* activists and the (mainly Muslim) jute peasants were, Ali argues, of neither communal nor economical nature, but *vigorous attempts to protect rural markets and fairs as spaces of pleasurable and enjoyable consumption* (p. 40).

Although the book is clearly influenced by the work of Sven Beckert (2014) in the

sense that it follows a commodity-centred approach to a wider history of capitalism, Ali distinguishes himself explicitly from his former fellow at Harvard. Through the concept of *war capitalism* – a term that stresses the violent nature of mercantilism – Beckert emphasizes the important role of empires and states in the formation of the global countryside (Beckert, 2014: xv). The opium monopoly in nineteenth-century North India, a topic the author of this review has written about (Bauer, 2019), neatly fits Beckert's state-centred approach to agricultural commercialization. The jute industry, on the other hand, where peasants responded to world-market prices, allows Ali to tell a story in which the cultivators were the main agents of change. Sometimes they prospered, sometimes they impoverished, but it was always their decision to participate in those newly developing market-entanglements. This is a unique narrative in the many contexts of the transformation of the global countryside.

World War I brought a sudden end to the jute boom and the consumerist lifestyle of the delta's jute peasants (chapter 4). Not only did the demand for jute decrease rapidly, but the region was also hit by ecological disasters that caused disease and death in the form of malaria, smallpox and cholera. Additionally, population pressure further fragmented the peasants' holdings down to miniature sizes of one acre or less, which made it impossible to produce sufficient subsistence. The combination of those unfavourable developments hit the delta's peasantry hard. It led to destitution and hunger and eventually culminated in the

Bengal Famine of 1943, when millions perished.

In the second part of the book Ali masterfully connects the peasants' *immiseration* and their self-fashioning as ascetic Muslim peasants. The change from well-off and market-friendly to destitute and market-sceptical peasants opened up the space for new discourses of agrarian Islam based on the ideals of hard work, austerity in consumption and patriarchal authority. *In this era of immiseration*, Ali writes, *the ideal Muslim peasant was envisioned as a savvy market operator who worked hard and reduced consumption so as to be financially self-sufficient* (p. 134). Based on these observations, Ali can explain the rise of the peasant populist Krishak Praja Party (KPP) in the decades after World War I (chapter 6). As the KPP failed to fulfil one of their biggest campaign promises – influencing jute prices to the advantage of the cultivators – the party disappeared as a political force in the delta. In the 1946 elections the population of the jute tracts in the delta helped the Muslim League to victory. They voted for the League's campaign of a Pakistan as peasant utopia. Ali shows that once more jute became central in the region's politics, as the newly independent state of Pakistan used this highly charged commodity in its *project of fashioning a national economy* (p. 170). Jute, it was hoped, should become a national resource *producing revenue, foreign exchange, and strategic advantage for the state* (p. 192). However, the jute manufacturing mills still stood in metropolitan Calcutta, on the other side of the border. The East Pakistani state's at-



tempt to undermine the jute exports to India resulted in extremely violent acts against its own citizens. At the border the army shot and killed suspected smugglers on sight.

Ali concludes the book with a clever analysis of the question why jute, despite the fact that the fibre is no longer significant in the delta's economy, continues to play an important part in the national discourse of Bangladesh. Jute, Ali argues, has since the 1850s been *entwined with the nationalist discourse of Bangladesh* (p. 195). It has never been just another commodity, but continuously shaped and reshaped the self-fashioning of the Muslim peasantry and the political discourse of the delta.

*A Local History of Global Capital* is a well-written and comprehensive book that

will be of great interest for students and scholars of British India, independent Bangladesh, the commercialization of agriculture in the *global countryside* and Muslim peasant politics.

**Rolf Bauer**

**orcid.org/ 0000-0002-4284-7758**

Universität Wien

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Niels Grüne, Jonas Hübner and Gerhard Siegl (Eds.)

### **Ländliche Gemeingüter: Kollektive Ressourcennutzung in der Europäischen Agrarwirtschaft / Rural Commons: Collective Use of Resources in the European Agrarian Economy**

Innsbruck/Wien/Bozen, StudienVerlag, 2016, 310 pp.

**T**his volume on the rural common-pool resources, and its institutional and conflictive management and use, is an important historical contribution to the study of the commons. It compiles 19 studies covering 500 years of commons management and change between the 16<sup>th</sup> and 21<sup>st</sup> century within Europe, with a special emphasis on England, France, Italy, The Netherlands, the German-speaking countries, and also Eastern and Northern European countries. This is

in itself a challenge, given the wide range of heterogeneity. Still, the volume overcomes this and fills the existing gap in comparative studies on European commons. The book includes mainly historical research, but the historical processes narrated provide lessons and theoretical challenges of broader scope, especially for scholars specialized in commons.

The studies are structured in three sections: research trajectories and new orientations; local and regional case studies; and

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The studies are structured in three sections: research trajectories and new orientations; local and regional case studies; and

problem-specific analyses and comparisons. The first section provides an overview of research topics in the European history of the commons, such as change, generation of more formalized rules, different enclosure tendencies and conflicts. The second section introduces different studies since the 18<sup>th</sup> century, and their contributions. In the third section, data and reflections on multiple settings are presented. Finally, the editors of the volume summarises the main findings and thoughts in the concluding chapter.

All contributions include regional and empirical evidence in a compelling way, that makes it an easy-read for scholars from other disciplines. Some of them provide outstanding insights, such as the studies on Spain (José-Miguel Lana), England (Jonathan Healey), France (Sylvain Olivier) and Germany (Stefan Brakensiek). Healey, for instance, presents interesting tendencies on the English commons management and institutional change, which, in spite of the challenges resulting from the reliability of historical data, give an insight into different constellations of enclosure processes, conflicts and contestations. His contribution highlights that commons are related to the collective use of resources, but also to daily contestations and exclusion processes. He thus introduces the issue of power, as he speaks of the *politics of the commons*, showing the different power relations within and outside the common-pool resource management contexts, thereby pointing to local agency.

The other three case studies in this section uncover how the institutions for the

management of common-pool resources operated and changed in regions that do not often attract scholarly attention, as Poland (Piotr Guzowski), Bohemia (Eduard Maur) and Hungary (Antal Szántay). In the latter one, the author draws attention to the changing value of common pool resources over time, and how different actors perceive this change. This is an important economic variable in the analysis of institutional change of the commons.

The second section presents six local and regional studies on northwest of Spain (José A. Serrano), the French Alps (Anne Marie Granet-Abisset), South Tirol (Evi Pechlaner), the Lombard Alps (Luca Mocarrelli), Osnabrück in northwest Germany (Jonas Hübner) and Dinkelsbühl in southern Germany (Teresa Massinger). These studies deal with other related aspects, such as the importance of the commons as poor relief in pre-industrial societies, their new uses (such as how tourism shapes the commons), how inequalities and legal pluralism in bi-national contexts impact local livelihoods, and the impact of the growing state intervention on common property regimes.

In the third section some comparative studies are presented. Tine de Moor and Annelies Tukker, for instance, stress that the stability of common property institutions is related to the issue of self-regulations and resilience, which they regard as the ability to find consensus rather than to monitor and sanction. Excess monitoring and sanctioning increase transaction costs and lower resilience, as it is shown in the comparison of eight Dutch commons systems. Likewise, the impact of externally induced cri-

sis on the commons systems and its increased use is shown in the Scandinavian comparative study by Jesper Larsson.

The other four contributions, by Christoph Pöll, Anne-Lise Head-König, Niels Grüne and Gerhard Siegl, and Hein van Giels, Rohan Mark Bennett and Martin Hipondoka, point out the role that market incentives, conflicts (in and between communities and with the State) and State interventions play in phases of political and economic change.

Although the volume is a valuable contribution to the literature, there are several issues to be addressed, especially from a non-historian standpoint. On the one hand, all authors recognize that they are not just dealing with a narrow concept of the common-pool resources, such as pastures and forests, but with many other such as water, fisheries, fruits, veld products of large sorts (including medicinal plants, materials for construction, etc.), as well as collective infrastructure, namely roads or irrigation infrastructure. In addition, the studies consider that it is not just about legal institutions, but also overlapping forms of commons and private property, and seasonal use of private lands as commons and vice versa. This is why the German word *Ländliche Gemeingüter* (rural commons) and not *Allmend-Ressource* is used.

All contributions show that the authors have a clear understanding of legal pluralistic settings and practices, and attempt to explain changes in these pluralities, rather than using a simplistic notion of duality between traditional commons and modern State or private property. Similarly, they

highlight that external changes lead to different constellations of power-relations, action arenas and discourses which, again, shape institutions where common-pool resources are managed. This indicates that common property regimes in which common-pool resources were regulated did not keep people away from turning to the modern world, but instead enabled early industrialization by reproducing labour at a low cost.

I also concur with the issue of membership inclusion and exclusion, and internal rule compliance (monitoring and sanctioning), as being key elements of the commons management—either in the sense of reducing these costs or as vector of change if these costs are getting too high.

There are, on the other hand, critical issues too. As much as I appreciate the discussion about the definition of the commons in German (*Allemende* vs. *Gemeingüter*), common-pool resources should be defined by the criteria of defendability and subtractibility (see Ostrom, 1990) taking into account local language and property context where they are institutionalized. I believe that there is no other way of paying attention to this range of resources, languages and local perceptions of meaning, regarding what we are dealing with. The term *Gemeingut* is also problematic as it already includes the legal notion, which should be separated from the quality of the resource (see Ostrom, 1990; Haller, 2013).

Another critique is that the volume could provide further discussions on which types of external forces trigger change in lo-

cal power constellations, and the selection of institutions regarding the management of the commons, as well as the legitimisation processes that the actors use. As international research on the commons influenced by a neo-institutional and political ecology approach demonstrates, inclusion, exclusion and enclosure processes relate to change in value of the common-pool resources and to the increased bargaining power of actors to shape the rules of the game (see North, 1990; Ensminger, 1992; Landolt & Haller, 2015; Haller, 2007, 2010, 2016).

In the conclusion the editors choose not to reflect on commonalities of such processes, which I believe is a lost chance to draw attention to the data presented. Access to the common-pool resources and established institutions depends largely on perceived value and interests by heterogeneous actors, not only at the local level, but also at the level where we find feudal, State and market forces. The volume contains one example of Non-European commons, but there are no further thoughts on other fields of contemporary common studies in Europe and elsewhere (see Netting, 1981; Liechti & Biber, 2016; Baur, Liechti & Binder; Mackinnon, 2018; Landolt & Haller, 2015, and also several publications in the *International Journal of the Commons*) and how these could contribute to the comparative analysis. Furthermore, there is no reflection as regards to the literature on legal and institutional pluralism, although studies mention it and refer to the issue of so-called forum or institution shopping (see Haller, 2016). Again, a neo-insti-

tutional approach, combined with a political ecology orientation, would strengthen the analysis of litigations that are alluded to in the studies, and the changes in the perception of the value of the common-pool resources related to power relations.

The selection and discursive legitimization of institutions could be much better explained. Characterizing actors and their power could also lead us beyond broad categories of local poor people, middle class, elites and feudal and/or state actors. The examples contain data that show why some actors selected institutions based on a broader ideological framing (*e.g.* modernity), hiding market level interests and gains.

Another issue refers to the robustness of common-pool resources. The Swiss case, amongst others, shows that the level of local participation in rule-making might be the crux of the matter. While in England and France, as well as some parts in Germany, commons are nowadays close to not existing at all (because commons were linked to feudal systems), the Swiss case shows that early regional independence was helping to set up robust institutions when governance could be decided at the local level (for a Dutch case, see also De Keyzer, 2018). Market and neo-liberal forces broke this local autonomy in the other areas and used the State as a chief driver to gain power in the 19<sup>th</sup> and 20<sup>th</sup> centuries.

I would thus argue that the contributions presented in this volume, as well as the concluding chapter, could have been improved analytically by focusing on external changes in relative prices and common-pool resources, and by looking at who

has bargaining power and who is able to select institutions according to their interests. In the conclusion, after arguing that there is no real comparable driving process, the editors recognize in the final paragraph that market forces and bargaining power are important. Along this line the contributions and the conclusion (which should also be made available in English) could have been improved, paying more attention to the way powerful actors legitimate their claims for changing the common property regime (for a comparative research project on Swiss Commons addressing these issues, see [http://www.anthro.unibe.ch/forschung/scales/index\\_ger.html](http://www.anthro.unibe.ch/forschung/scales/index_ger.html)).

Despite these limitations, this volume is of great interest to scholars working on commons. It offers interesting case studies on specific issues, such as conflict and change in the management of the commons, for several European territories. Perhaps the conclusion could be reworked as a paper for the *International Journal of the Commons*.

**Tobias Haller**

Institut für Sozialanthropologie  
Universität Bern

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Peter M. Jones

### **Agricultural Enlightenment: Knowledge, Technology, and Nature, 1750-1840**

Oxford, Oxford University Press, 2016, 288 pp.

**A**gricultural Enlightenment, de Peter M. Jones, es un libro sobre el desarrollo y la difusión de nuevas ideas y tecnologías en la agricultura europea entre mediados del siglo XVIII y 1840. Es la continuación de otro trabajo anterior del mismo autor, titulado *Industrial Enlightenment* (Manchester University Press, 2008) en el cual realizaba un ejercicio similar para el caso de la revolución industrial. Ambos libros están claramente influenciados por la obra de Joel Mokyr, (un reciente ejemplo: *The Enlightened Economy*, Yale University Press, 2010), si bien es cierto que Jones es crítico con algunas ideas de este autor.

La teoría fundamental que subyace en los tres trabajos es que la revolución intelectual que hoy conocemos como Ilustración fue determinante para explicar el cambio económico que tuvo lugar en Europa desde el siglo XVIII. Enfatizan, en consecuencia, la importancia de los factores intelectuales e institucionales –frente a los cambios en el mercado o las variables geo-

gráficas– como impulsores de la *gran divergencia*, esto es, del aumento en la brecha entre la renta de los países europeos y del resto del mundo. El libro que presentamos en esta reseña se centra, en particular, en el sector agrario, esto es, vincula el auge de la Ilustración y los cambios intelectuales e institucionales que llevó aparejados a las transformaciones agrarias que tuvieron lugar en el Viejo Continente entre mediados del siglo XVIII y mediados del XIX. Es esta una asociación interesante, creativa, poco habitual pero también, según sostiene el propio autor, incluso *perversa*. La tesis fundamental es que la Ilustración creó un marco eficaz para la difusión de ideas y conocimiento en la agricultura europea, y que las instituciones de la época facilitaron ese proceso a la vez que sublimaron el desarrollo de mejoras productivas y económicas.

El libro supone una contribución decisiva en un momento en el que la historiografía agraria está –o, mejor dicho, lleva estando durante al menos dos décadas–

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dominada por estudios cuantitativos o bien por estudios que ciñen la historia agraria a lo que ocurre a pie de finca. La historia intelectual del mundo rural, la historia de la difusión de sus ideas y la influencia que estos impactos han tenido en el cambio agrario, aunque en absoluto han estado descuidadas, han perdido presencia relativa. Este libro, por tanto, supone un avance muy importante a la hora de superar esta carencia. Nadie puede obviar la importancia de la *economía del conocimiento* y, por tanto, de su difusión, como motor explicativo de los cambios agrarios y económicos a lo largo de la historia. Por ello, resulta muy importante seguir profundizando en su estudio, especialmente con propuestas metodológicas renovadas: en este caso, desde una perspectiva de historia de las ideas que no solo analiza los cambios tecnológicos, sino el clima intelectual y los medios de difusión que propiciaron su propagación por todo un continente.

El libro se estructura en ocho capítulos, además de la introducción y las conclusiones. En rigor, es posible identificar tres grandes partes: en los dos primeros capítulos se ofrece una revisión de los principales cambios en el pensamiento y las instituciones políticas y económicas europeas desde el siglo XVIII. Los capítulos centrales, del tres al siete, ofrecen los principales resultados. En ellos el autor documenta la difusión de las ideas y las tecnologías agrarias, así como los principales vectores y agentes de esa difusión. Esta parte detalla el cambio institucional, la aparición de nuevas organizaciones y asociaciones agrarias, el surgimiento de revistas, imprentas, colecciones

y otros medios de canalización del conocimiento agrario. Y, principalmente, cómo este proceso tuvo lugar a escala paneuropea. En los dos últimos capítulos el autor analiza el impacto de ese proceso en el cambio agrario europeo de finales del siglo XVIII y, también, el declive de lo que él llama *Ilustración Agraria* debido al surgimiento de la química agrícola y otros desarrollos agronómicos que propiciaron una nueva oleada de cambios en la agricultura.

El trabajo evita un error habitual entre muchos teóricos de la *gran divergencia*: extrapolar la evidencia inglesa al resto del continente. Jones, siguiendo a otros autores como Jan Luiten van Zanden, insiste acertadamente en señalar que detrás de la *gran divergencia* también hubo divergencias dentro del continente europeo. Así, esta obra, aun con carácter continental en su cobertura geográfica, es cuidadosa a la hora de ofrecer contrastes y matices nacionales e incluso regionales. La mayor parte de sus evidencias se centran en los casos –no tan usuales, lo cual se agradece– de Dinamarca y Escocia, pero, además, a lo largo del libro encontramos múltiples referencias a evidencias regionales, incluyendo Cataluña, Andalucía y Extremadura (sic).

¿Cuáles son las principales aportaciones de este trabajo? El libro documenta los principales cambios en la denominada *economía del conocimiento* dentro del sector agrario, esto es, las nuevas ideas, tecnologías o conocimientos asociados a las prácticas agrarias entre 1750 y 1840. Por otro lado, analiza con gran erudición los mecanismos de difusión de esos cambios, para lo cual demuestra el papel central de las nue-

vas instituciones ilustradas. Hasta aquí la argumentación y las evidencias que la sostienen son sumamente convincentes: el contexto de la Ilustración *allanó el terreno y creó unas condiciones óptimas* para el desarrollo agrario del período estudiado.

Dicho esto, el libro aborda dos preguntas adicionales que se responden de manera menos convincente. Primero, ¿hasta qué punto el contexto de la Ilustración fue el factor más determinante a la hora de difundir el conocimiento agrario? El autor muestra que, sin duda, fue importante. Sin embargo, el libro no es concluyente a la hora de discernir hasta qué punto lo fue. Muchos avances tecnológicos, como por ejemplo el desarrollo de rotaciones más productivas, ya eran conocidos en el siglo XVIII e incluso mucho antes. Entonces, ¿por qué no se pusieron en práctica hasta bien entrado el siglo XIX? No podemos obviar las respuestas más convencionales: que el surgimiento de nuevas oportunidades de mercado o la presión poblacional guiaron la generalización de nuevas tecnologías. Esto no invalida, empero, el argumento de Jones, sino que nos obliga a recordar que los factores explicativos que arguye concurren con otros a la hora de explicar la difusión tecnológica.

En segundo lugar, ¿hasta qué punto esos cambios incidieron en la revolución agrícola? El autor es honesto en este sentido: explica que no puede establecer una relación causal unívoca entre Ilustración y revolución agrícola, aunque sostiene que sin duda la Ilustración facilitó el desarrollo de las mejoras agrarias que tuvieron lugar en Europa en el período estudiado. Se agra-

dece el hecho de que evite dar a toda costa una respuesta simplista y una explicación monocausal a un problema complejo, cosa que, dicho sea de paso, es muy común en la literatura académica actual, en la que los libros más exitosos son, trágicamente, los que ofrecen visiones más simplificadas de cambios complejos. Ser didáctico y ofrecer respuestas inteligibles no debería ir asociado a la simplificación argumental.

Sin embargo, el autor no evita la lógica tentación de incidir en favor de su hipótesis, esto es, de la estrecha relación entre Ilustración y revolución agrícola, aun cuando la evidencia no alcanza para ello. En esta defensa, en mi opinión, encontramos la parte menos convincente del libro. Se sustenta en dos argumentos poco trabajados. El primero, que el período que va de 1750 a 1840 fue un período exitoso desde el punto de vista del crecimiento agrario, llegando a citar —con datos del caso francés— que la productividad del trabajo se quintuplicó. En este sentido, el autor discute varias veces el argumento de que la agricultura europea podría estar alcanzando su límite productivo e incluso afrontando crisis ecológicas en algunos territorios. Reitera esa idea en diferentes pasajes del libro (en la introducción, las conclusiones y los párrafos finales del capítulo ocho) con el claro propósito de enfatizar que durante el auge de la Ilustración, la agricultura siempre protagonizó una senda de crecimiento y progreso y, por tanto, que la difusión tecnológica estuvo relacionada con tal tendencia. ¿Cómo muestra que la agricultura europea no sufrió estancamiento productivo, crisis ecológicas o que incluso

afrontó límites maltusianos? De ninguna manera. Nos invita a dar por buenos sus argumentos, obviando una ingente literatura de historia económica y ambiental reciente que ha puesto de manifiesto en los últimos años los límites productivos de las agriculturas orgánicas, especialmente en Europa.

El segundo argumento que me parece poco convincente tiene que ver con la cronología propuesta en el libro. Pone punto final al trabajo en 1840, porque asume que desde esa fecha las agriculturas europeas pudieron esquivar los límites productivos debido al auge de la química agrícola y de otros avances desde las ciencias naturales. En mi opinión, esta visión es sumamente optimista si nos referimos al continente europeo en su conjunto: la ruptura evidente de los límites productivos en la agricultura europea no tuvo lugar hasta varias décadas después, cuando la química agrícola y la mecanización se difundieron de manera generalizada.

Una última interpelación al libro tiene que ver con el inevitable desbalance geográfico en el que incurre. Como apuntábamos más arriba, el libro es muy cuidadoso a la hora de ofrecer evidencias de las diferentes estados y regiones europeas. Lo hace en base a una extensa revisión de literatura. Sin embargo, resulta chocante que esa cobertura de amplio espectro desaparezca en la introducción (cuando plantea las grandes cuestiones que abordará el libro) y en las conclusiones (cuando aborda las principales contribuciones del mismo). En esos apartados no hay rastro de países que no sean los del norte de Europa. El resto del continente queda literal-

mente fuera de los análisis más importantes de la obra, que se centran en Inglaterra, Escocia, Dinamarca, los países escandinavos y Francia.

Más allá de los lógicos desbalances en los estudios de caso, así como en la manera con la que se resuelven algunos capítulos, el libro que reseñamos es, sin duda, sumamente recomendable. De una manera innovadora y original aborda un tema poco trabajado y de gran importancia: la difusión del conocimiento en los cambios agrarios del siglo XVIII. Los resultados suponen una contribución decisiva para conocer mejor las causas de la revolución agrícola y, con ello, de la excepcionalidad europea en el desarrollo económico mundial en los últimos dos siglos, aportando información muy importante que nos permite dejar de lado planteamientos simplistas y unidireccionales de tales procesos.

**Juan Infante Amate**

**orcid.org/0000-0003-1446-7181**

Universidad Pablo de Olavide (Sevilla)