

Jan Lucassen and Leo Lucassen (eds) *Globalising Migration History: The Eurasian Experience (16th - 21st Centuries)*, (Leiden and Boston: Brill, 2014) 500 p. ISBN 978 90 04271357.

The book attempts to give migration a more prominent place in in global history and big debates on social and economic change. According to the Lucassen brothers, the lack of consensus on what defines migration is responsible for the relative absence of the topic in these fields. The book therefore starts out by giving clear definitions of migration and mobility. Inspired by Patrick Manning the editors put forward a model that determines whether the migration trajectory took place within a similar cultural space or across cultural boundaries, and considering only the latter as migration. The Cross Cultural Migration Rate (CCMR) method aims at measuring and comparing migratory moves within a given space through time to assess both their extent and impact. These moves are divided into six categories to refine the forms of migration (urbanization, colonization, seasonal and temporal multi-annual labor) and the quantification (immigration, emigration). The model that the editors developed to map out European migrations is now tested and adapted by applying it to the Asian experience. To do this they invited a broad range of scholars who, some more consequently than others, tested the model on Russia (Sunderland, Kessler), South India (Ramaswamy, Amrith), Indonesia (Bosma, van Lottum), Malay Archipelago (Ota) Southeast Asia (Mazard) China (McKeown, Umeno, Shen) and Japan (Lucassen, Saito and Shimada).

Each article has merits on its own and jointly they produce some interesting findings. At the same time they highlight some of the limitations of the model. The biggest contribution is in quantifying Asian migrations and further questioning the 'mobility transition' as coined by Wilbur Zelinsky who linked a revolution in mobility and migration patterns with the Industrial Revolution. The results for Asia corroborate the findings for Europe, showing that overall rates of cross-cultural migrations were much higher than assumed, especially before the mid-nineteenth century. For Europe, the Industrial Revolution and its technological innovations intensified (mostly pre-existing) migration patterns to cities and overseas, but did not generate a transition. Asian migration corroborates this and the results show much more similarities than acknowledged so far. For instance, the levels of pre-industrial urbanization in some parts of Asia are much higher than assumed and challenge the notion that these were inferior to the European experience. Only after the takeoff of the Industrial Revolution in Europe did the urbanization gap grow until Asian regions caught up their backlog.

The articles show that a rigid application of the categories are not viable. Depending on the cases there can be a lot of overlap between categories. There are very blurred lines whether some migratory movements should be counted in

one category or the another. Another problem is that some categories are much more visible and traceable than others. For instance, the model has a clear merit to give soldiers and sailors a prominent place in the multi-annual labor category. Both have been neglected by migration historians and the argumentation to include them is convincing. They highlight the need for more qualitative studies to uncover their mobility patterns and quantitative studies to capture their volume. This is especially true for merchant sailors, which were difficult to include in most articles. Yet by identifying the multi-annual labor migration nearly exclusively with sailors and soldiers, the category fails to include the mobility of domestics, tradesmen, tramping laborers and artisans, vagabonds, etc. Although this problem is acknowledged, it still needs to be solved. Stating that these groups remain limited and that they are captured in other categories remains a weakness of the model. The numbers available only allow vague guestimates of their true proportion. Moreover, downplaying the flaw by pointing that they are captured elsewhere is problematic. The model uses the categorization and the volume of the different forms of migration as one of the cornerstones to analyse the impact of each form of migration on social, cultural and economic change. Hence whether some groups belong to certain categories or others can majorly influence the interpretation. Finally as McKeown remarks, the categorization is designed on settled agricultural societies, and loses its utility for comparisons with nomadic and semi-nomadic peoples through space and time.

The book has merits to pierce through the stigma of one directional migration pattern. The model includes all forms of migration patterns from permanent to seasonal, internal to cross-border, and one directional to return and circular migration patterns. However, it falls short to its claim to assess the impact of migration on both sending and receiving societies. The focus remains nearly exclusively on exchanges that occur in the receiving society, while the impact on sending societies is barely addressed and transit regions are not considered at all. Furthermore, although the model relies heavily on the quantitative approach, the authors rightly warn not to overstress the importance of the sheer volume of migrations, but that the intensity of the exchanges can just as much depend upon the qualitative type of migration. Yet how to measure this qualitative importance with the CCMR model is less clear. Finally, the book also rightly calls for a more explicit integration of trade and transport networks in the CCMR model as it highlights the impact of trade routes and technological innovation on migration flows. Some of the articles stress the importance of businessmen (traders, entrepreneurs, bankers...) and go-between recruiters in steering migrations. They uncover the close connections between trade routes and human mobility although these were less prominent than in Europe. The more coercive and less commercial nature of Asian migrations in general, distinguishes them from the European experience. The role

of markets and states in shaping migration are obviously acknowledged, but more importantly the model is intended to be used to assess how migration shapes markets and state formation. Economic, social, political and cultural changes have an impact on the migration patterns, but the opposite is also true. However, the incorporation of migration in these bigger debates remains absent. Can the CCMR model convince scholars to do so?

The book proved to a skeptical of quantitative approaches like myself, that it has a lot of potential. Given the scale of the ambitions, the CCMR model opens itself for a lot of critique. Yet the authors are well aware of the shortcomings and did not fall in the trap to draw too far-fetched conclusions. Critiques are used to adjust the model which can only improve by extending the comparison through space and time and making more explicit how migration influences social, political and cultural change. A good starting point would be to link the CCMR model to other macro-scale approaches to world history, such as world system analysis. Applying this to African migrations seems like a nice challenge! The book is a very interesting read for both scholars in migration history and world historians alike and will hopefully reach beyond.

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Saartje Vanden Borre, *Toga's voor 't Hoge. Geschiedenis van de Leuvense universiteit in Kortrijk* (Leuven: Universitaire Pers Leuven, 2015), 183 p. ISBN 9 789462 700406.

Bezien met een Nederlandse blik is het een wonder dat de Katholieke Universiteit Leuven Afdeling Kortrijk (in de wandeling Kulak geheten) vijftig jaar na haar oprichting in 1965 nog altijd bestaat en aanleiding is geworden voor de uitgave van de hier besproken geschiedenis. Want een succes kan deze universitaire instelling toch moeilijk worden genoemd. Zij was het resultaat van een succesvolle lobby van de bisschop van Brugge en de burgemeester van Kortrijk die de handen ineen hadden geslagen. De een wilde een katholieke West-Vlaamse elite vormen en met een campus in Brugge of Kortrijk voorkomen dat de jongeren in zijn diocesis aangewezen waren op de vrijzinnige rijksuniversiteit in Gent, de ander wilde de economie van de provincie een impuls bezorgen. Of de economie erdoor werd gestimuleerd, blijkt uit deze studie niet echt. Duidelijk is wel dat van de vorming van een katholieke West-Vlaamse elite niet veel is gekomen.

De Campus Kortrijk werd ontworpen voor 5000 studenten. Dit aantal was destijds voor de Belgische regering de norm voor een kleine universiteit. Maar Kortrijk trok veel minder studenten dan was gehoopt en verwacht. In 1982 telde