

## Thoughts on *How the West Came to Rule*

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*How the West Came to Rule: The Geopolitical Origins of Capitalism* by Alexander Anievas and Kerem Nişancioğlu is a remarkable book that offers much needed correctives to mainstream accounts of the emergence of capitalism. Working generally within the “uneven and combined development” framework as first articulated by Leon Trotsky<sup>1</sup>, Anievas and Nişancioğlu argue that “capitalism is best understood as a set of configurations, assemblages, or bundles of social relations and processes oriented around the systematic reproduction of the capital relation, but not reducible — either historically or logically — to that relation alone” (2015, 9). Seen as such, its emergence cannot be explained by studying national histories alone. This insight gives rise to the two main contributions of the book. On the one hand, the authors advance very compelling criticisms of other influential Marxist inspired accounts of the emergence of capitalism, such as that of Wallerstein and Brenner, as being Eurocentric.<sup>2</sup> On the other hand, the authors offer their own substantive account of the emergence of capitalism, pointing to factors heretofore either ignored or under-scrutinised in the literature: “a demographic crisis brought about by the Black Death; the Ottoman-Habsburg rivalry; the discovery of the New World and its division along linearly demarcated spaces of sovereignty; the festering atmosphere of revolt and rebellion; the economic significance of colonisation” (4). Especially significant in their account is the ‘contributions’ of the Mongolians and the Ottoman Empire to the development of capitalism.

Given the relative insularity of both the “uneven and combined development” school in particular and Marxist approaches in general in IR, one worry is that this book will be read primarily by scholars who are already working within those traditions. I hope this is not the case, for two reasons: first, because this might limit the debate about the book to issues of whether Anievas and Nişancioğlu’s criticisms of other Marxist influenced approaches are warranted, and second, because I believe the book has much to offer to those of us who are not working within Marxist traditions but are interested in the questions of how and also *why* “the West came to rule.” In this brief essay order I make this case from a non-Marxist perspective by first discussing Anievas and Nişancioğlu’s criticisms of the existing literature and then some of the substantive contributions. My main goal is to

demonstrate what sorts of productive conversations can be had across literatures, even when we disagree.

### **Criticisms of existing explanations for the emergence of capitalism**

The explanations of Immanuel Wallerstein<sup>3</sup> and Robert Brenner<sup>4</sup> receive the brunt of the criticism by Anievas and Nişancioğlu. Wallerstein is criticised for “the unwitting reproduction of Eurocentrism that erases non-European agency; and the inability to provide a sufficient historicised conception of capitalism” (2015, 13) and Brenner is criticised for his “commitment to a methodologically internalist and concomitant Eurocentric (or Anglocentric) analysis of the origins of capitalism; second the resulting deficiencies in [his] examination of the relationship between the making of capitalism and geopolitics; and third, [his] highly abstract and minimalist conception of capitalism” (14). Other Political Marxist approaches that build on Brenner, such as that of Teschke (2003), Lacher (2006) or Wood (2002), are not spared either, and are criticised for ignoring “the many ‘war-assisted’ processes of capital accumulation, geopolitical coercion, competition, rivalry and the like littering the history of capitalism’s development” (28) and for conceptualising capitalism as contingent in its relation to the multistate system. In fact, it could be argued that on the whole, the authors prefer Wallerstein’s World Systems Theory to the explanations favoured by Political Marxists, because the former pays more attention to the *longue duree* (15) and “has necessitated the study of societies outside of Europe” (16) despite its “typically Eurocentric view that the transition from feudalism to capitalism took place uniquely and autonomously within the clearly demarcated spatial confines of Europe” (17). Political Marxists, on the other hand, have ignored “intersocietal interaction and the concomitant geopolitical relations of political-military competition and war-making” (27). In other words, while both approaches suffer from Eurocentrism, the latter has the additional sin of pronounced “methodological internalism”.

On the whole, these criticisms are rather convincing, especially in light of the more substantive arguments advanced later in the book. For example, in Chapter 4, the authors demonstrate that many developments taken to be entirely endogenous to Europe by the literature would not have happened at all had it not been for the presence of the Ottoman Empire on the “periphery” of Europe. In particular, “it was the Ottoman threat that so persistently redirected both Habsburg and Papal resources away from the internal divisions that were stretching the Empire in the northwest, contributing in turn to the perpetuation of ‘multiple polities within the cultural unity of Christian Europe’ that ‘time and again frustrated universal imperial ambitions’” (114). It was the Ottoman presence in the Mediterranean that rendered the Atlantic more attractive as a site of commercial activity (115). Finally, “The Habsburg-Ottoman rivalry...formed a geopolitical centre of gravity that often

redirected imperial concerns away from England and the Low Countries" (117). Both also benefited from capitulatory commercial privileges given to them by the Ottomans as a result of inter-polity rivalries, privileges that gave them access to raw materials and staple commodities, "'freeing' agricultural land from extensive production" (117). When we put all of these factors together, it does indeed seem to be a glaring omission in especially Brenner's account that "the world outside Europe does not figure at all" (25). It is hard to disagree with the conclusion that a more complete picture of the emergence of capitalism in North-western Europe would have to emphasise the presence of external dynamics. The clear message is that one cannot explain the emergence of capitalism in England only by studying the English countryside.

The one thing that gives me pause here, however, is a body of literature that the authors do not engage with at all. What I have in mind is the literature on Asian capitalism, especially the argument that Japan (and to a lesser extent, China) had independently developed capitalism *before* their interactions with European powers. For example, working with a neo-Weberian model (with a Schumpeterian twist), in "An Asian Route to Capitalism: Religious Economy and the Origins of Self-Transforming Growth in Japan" Randall Collins (1997) strongly criticises the Weberian assumption that the breakthrough to capitalism only occurred in Europe. The Weberian model assumes the following as necessary conditions for the transition: "there must exist markets for all the factors of production: land, labor, and capital" (845); "control of all the factors of production must be combined in the hands of entrepreneurs" (845); there must be present "an economic ethic of disciplined work and calculation of productive gains" (846). Historically, these institutions could not develop in most places because of social obstacles, especially societal hierarchies (846-7). Weber argued that only in Europe these obstacles were overcome by the development of systemic law in connection with the rise of the bureaucratic state, as well as the presence a "universalistic religion of salvation, above all Christianity, that broke through the barriers of ethnicity and kin groups", especially after the emergence of "the Calvinist doctrine of predestination" which emphasised "the economic ethic of ascetic self-restraint and calculative rationality directed toward economic productivity" (847). Collins disagrees with Weber that these elements were only present in Europe, and argues that where they were present, i.e. at least in two other contexts, medieval Buddhist China<sup>5</sup> and pre-Tokugawa Japan, capitalist arrangements had indeed developed to a considerable extent. Collins notes that according to the Weberian logic, agrarian-coercive structures are rather resilient to the emergence of merchants, monetization and long-distance trade, *unless* these developments are also accompanied by "property relations freeing up all the factors of production and giving legal protection to their market transactions, the dissolution of social barriers against full participation by individuals in the market, and the circumvention of status hierarchies whose

incentives worked against long-term calculation, ascetic restraint, and investment" (862). In "medieval Christian Europe, medieval Buddhist China, and pre-Tokugawa Buddhist Japan", however, social obstacles were initially overcome by monastic "entrepreneurs", and in each case, "the initial breakthrough of the religious leading sector was followed by a church reformation, in which the distance between religious specialists and laity was narrowed and religious property was confiscated. Each reformation resulted in a second wave of self-transforming capitalist growth-in the secular economy of agricultural capitalism" (862). The main difference, of course, which Collins also notes, is that in only one did the developments continue to an industrial revolution, and why this was the case needs further interrogation.

I have taken this long detour into neo-Weberian lands for several reasons, all with interesting implications for Anievas and Nişancıoğlu's criticisms of the Marxist influenced literatures for being methodologically internalist and Eurocentric, criticisms to which I am very sympathetic, as noted above. However, if Collins (and others) are right about capitalism having developed independently in Japan (leaving aside the issue of the correctness of their causal explanation), this may actually give internalist arguments such as Brenner's a surprise boost, while leaving the other charges of Eurocentrism intact. For if there is one other place in the world that resembles England in its suitability for methodologically internalist analyses, that must be Japan. Setting aside the even for Collins less developed case of medieval Buddhist China for the moment, if it could indeed be shown that capitalism developed *independently* in island societies facing similar resource and demographic challenges, societies which, for varying reasons, were similarly *insulated* from the developments in their neighbouring continents, would this not make the emergence of capitalism (if not the expansion of it) one of the rare social phenomena that is more suited to endogenous explanations? I do not think this is the case, but we do not really know how the model would deal with this particular objection because Anievas and Nişancıoğlu have not considered the arguments that capitalism emerged in locations outside of Europe.

This brings me to the issue of Eurocentrism. Anievas and Nişancıoğlu offer a very useful discussion of Eurocentrism in their introduction to the book, and point out that Eurocentrism rests on three distinct assumptions: *methodological internalism* as referred to above, "whereby European development is conceptualised as endogenous and self-propelling"; *historical priority*, "whereby Europe is conceived as the permanent 'core' and 'prime mover' of history"; and, *linear developmentalism*, "in which endogenous processes of social change - from tradition to modernity, feudalism to capitalism and so on - are conceived as universal stages which encompass all societies of the world, at different times and different places" (2015, 5). Identifying Eurocentrism is easier than overcoming it, however. Especially via their chapters on the Mongolians, the Ottoman-Habsburg rivalry and European

colonialism, Anievas and Nişancioğlu do an excellent job of combatting *methodological internalism*, by demonstrating that European development was not endogenous or self-propelling. However, there is a tension between the possible remedies to the assumptions of *historical priority* and *linear developmentalism*. If processes of social change are not universal, should that necessarily imply that the transition to capitalism happened only in Europe? And does that conclusion not bolster the idea that Europe is the core of history? Does it matter to tell a much more layered story with much more room for non-European agency if we are still putting Europe and developments in Europe at the front and centre as the subjects of inquiry and analysis?

It could be said that Anievas and Nişancioğlu's remedy for Eurocentrism is to take a few steps back to see a fuller picture of European developments, with multitude of sources both within but especially outside of Europe. This is similar to the approach favoured by John M. Hobson, for instance in *The Eastern Origins of Western Civilisation* (2004). Collins' approach as discussed above offers another way of combatting Eurocentrism; instead of assuming Europe is the only picture worth looking at, he holds the European picture side-by-side by that of Japan and attempts to discover what is generalisable to both cases (and therefore not uniquely European). A similar approach is favoured by Jack Goldstone in "East and West in the Seventeenth Century: Political Crises in Stuart England, Ottoman Turkey and Ming China" (1988) where he argues that the English Revolution of 1640, the *Jalali* Rebellions in the Ottoman Empire in the first half of the seventeenth century and the fall of the Ming Dynasty in China around the same were rooted in similar causal dynamics, and compares the responses. He concludes that "the divergence of Eastern and Western civilizations after the mid-seventeenth century cannot be attributed to a structural difference between Western 'revolutions' and Eastern 'peasant rebellions'" (132) and have more to do with "ideological differences governing state reconstruction" (133). I am very sympathetic to the former strategy of combating Eurocentrism as deployed by Anievas and Nişancioğlu or Hobson. However, given the interesting comparative work being done as exemplified by Collins and Goldstone, among others, I do have some reservations about conceptualising examples of Eastern agency only as "tributary rivers" that fed into European "lake", which still remains the proper object of inquiry, especially if the ultimate goal of this research is not just narrating *how* the West came to rule, but also answering the *why* question implied therein.

This is not to say I am necessarily championing Collins' or Goldstone's causal explanations over Anievas and Nişancioğlu's. I have reservations about both, although I do think that Collins' neo-Weberian model is indeed an improvement over Weber's in explaining why capitalism did not emerge independently in Muslim societies — Islam, after all, has a merchant prophet — especially in the Ottoman

Empire, which, after the fifteenth century had monetization (and a common currency throughout the empire), distance trade, vibrant merchant communities, as well as an increasingly bureaucratized state.<sup>6</sup> What the Ottoman Empire did not have are the monastic type of religious organisations which Collins considers foundational in the initial transition to capitalism, in both Europe and in Japan. It is an argument that needs further scrutiny. Of course, it is possible to argue that comparative approaches such as Collins' and Goldstone's still suffer from Eurocentrism in that they inevitably operate with concepts and explanatory frameworks derived from the European experience. It could also be argued that at least some approaches of this type suffer from "methodological internalism" as well. Perhaps a way forward, then, is a combination of the two strategies of combatting Eurocentrism: taking a set back to develop a better understanding of the bigger picture *while* maintaining a comparative sensibility and an interest in the dogs that did not bark as much as those that did.

### **Substantive Contributions of *How the West Came to Rule***

Despite the criticisms above, it has to be said that the entirety of *How the West Came to Rule* is a testament to what is to be gained when research attempts to break out of its Eurocentric shackles. The book makes many substantive contributions, but the chapters on the long thirteenth and the long sixteenth centuries are particular highlights in terms of their discussion of the Mongolian and Ottoman contribution to European politics.

In Chapter 3, which focuses on the "long thirteenth century", Anievas and Nişancioğlu argue that had it not been for the Mongolian invasions, Europe would have remained peripheral to the world system, and that "the establishment of the *Pax Mongolica* was...a major boon for overland trade connecting East and West, which notably benefited Northwestern Europe" (2015, 74). The argument here is that it was the Mongols who connected Europe and Asia for the first time "under a single authority" and thereby facilitated "the emergence of a nascent 'world economy'" (75). Anievas and Nişancioğlu argue that it was during this period Europeans became aware of the possibilities of trade with the Far East, and maintained that interest even after the Mongol Empire collapsed and trade declined (76). Furthermore, the existence of this previous network made possible what came later: "The Europeans 'did not need to *invent* the system, since the basic groundwork was in place'" (76, citing Abu-Lughod 1988). Though some may object to this argument as taking the causal chain of events too far backward — after all, everything in human history has a precursor in something else that came before — I think the argument is a necessary corrective to a literature that has ignored the Mongolian influence in history to its own detriment. Due to the Eurocentric bent of studies of world history, the Mongolians, like the Huns before, and to some extent

the Ottomans after them, until relatively recently were barely considered proper objects of study or were studied only through the lens of Others who brought destruction to Europe. Revisionist history in recent decades has seriously challenged what we thought we knew about all of these groups, and IR is only beginning to catch up.

In complicating our thinking about the Mongolians, Anievas and Nişancioğlu join a small group of IR scholars who have made similar arguments for studying them, such as Iver B. Neumann and Einar Wigen in their article “The Importance of the Eurasian Steppe to the Study of International Relations” (2013) and Martin Hall with his work on “Steppe State Making”. I think this is a very productive line of inquiry, and as the work of these three authors also suggest, the influence of the Mongolians on the international system may not be limited to what is discussed by Anievas and Nişancioğlu. For example, Guy Burak has recently done some very interesting work on the influence of the Mongolian notions of *yasa* (law) on Muslim polities of the early modern era: “The Chinggisid universalist notion of sovereignty rested on the view that the divine dispensation to rule the world was given to Chinggis Khan and his descendants. Fittingly, Chinggis Khan was considered a divine legislator. Two concepts capture this notion of sovereignty: Chinggisid *yasa* and the Turkic *töre* (or *törä*)” (2013, 595). Burak argues that the legacy of the Mongolian invasions was to significantly change the understanding of lawmaking in several Muslim polities, giving rise to experiments with non-religious lawmaking and changes in the notions of sovereignty. Neumann and Wigen point to similar influences in the case of Russia. Given their preoccupation with more material dynamics, Anievas and Nişancioğlu are not particularly interested in the ideational legacies of the Mongol invasions (and in fact their characterisation of the Mongolian polity is slightly at odds with the arguments from the revisionist literature on the Mongols), but their account of the Mongolian linkages does raise the possibility that there could indeed be ideational traces even in the case of Europe. Clearly this is an interesting line of inquiry and more research is needed; I hope others will pick up this thread from where they left off.

A similarly welcome corrective offered by Anievas and Nişancioğlu is the chapter on the Ottoman-Habsburg rivalry. In addition to the arguments discussed above pointing to the role played by the Ottoman Empire in European dynamics, this chapter also contains an interesting comparison of state-building trajectories in the Ottoman Empire vs. Europe. Anievas and Nişancioğlu argue that the Ottoman Empire “can be conceptualised as a tributary mode of production, distinct from — rather than a subvariant of feudalism” (99). The Ottoman model differed from Europe in that the main mechanism for surplus appropriation was taxation; peasants “were legally considered free” and “had inalienable rights to land” though “all land was formally owned by the Sultan, while military fiefs — *timars* — were

predominantly nonhereditary” which made them dependent on the state (99 - 100). As a result, “the primary contradiction of the tributary mode therefore lay in the structure of the ruling class itself, which could come into conflict over the distribution of surplus between its central and provincial sections” (101). As long as the Empire kept expanding, however, these conflicts could be kept at bay. In such a system, merchants were not particularly important – “the Ottoman ‘military-agricultural complex’” did not require external financing to raise vast and loyal armies (102-3). There is some debate in the literature on whether the foundational period of the Ottoman Empire (especially the *beylik* period) could be characterised as feudal, but on the whole, these characterisations of the Ottoman Empire, especially if limited to the sixteenth century, are on the whole compelling. Though I wish that the authors had engaged more directly with the question of why capitalism did not emerge in the Ottoman Empire as much as they did with the question of why it did in Europe (see also my comments in the previous section), their account in this chapter does give us promising clues about the later divergence of the fates of this region and Western Europe.

If the authors could be faulted for something it is in overemphasising the degree to which “the devices of ruling class reproduction under the tributary mode proved remarkably efficient and stable” (101) based on material factors alone, which leads them to paint a slightly static picture of Ottoman politics and economics. As recent revisionist Ottoman history has shown<sup>7</sup>, the sixteenth century was arguably a period where the Ottoman state centralised its authority to an extent not witnessed before in Islamic polities, and this was underwritten by various ideational justifications, such as the Mongolian concept of *yasa* referred to above as well as the Millenarian currents underwriting the rivalries the Ottomans had with the Habsburg and Safavid empires. Such centralisation efforts were greatly resented and resisted by alternative loci of power such as the *ulama* and (later) the janissaries, a fact which started become apparent towards the end of the sixteenth century, long before conquests had come to a halt. Therefore, while the authors may be correct in claiming “rebellion in the countryside was a less marked feature of the Ottoman tributary mode” if they are only speaking of the sixteenth century, by the seventeenth century this was no longer the case (see e.g. the *Jalali* rebellions referred to in the previous section), and I am not convinced that such developments can be explained without references to competing notions of sovereignty and authority at play during that period.

## Conclusion

None of these objections take away from the fact that *How the West Came to Rule* is an excellent read for even — or perhaps especially for — those of us who are not working within “the uneven and combined development” tradition. Of all the



IR books and articles dealing with this particular subject, this book probably goes the furthest distance in countering the Eurocentric bent of the literature and points to many new avenues of research and productive conversation while doing so. The failure to go all the way is not in anyway a shortcoming of the book but a reflection of the history in and against which we are all working.

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## Notes

- <sup>1</sup> For other applications in IR, see e.g. Rosenberg 2006, 2013; Matin 2007.
- <sup>2</sup> Postcolonial accounts are similarly (but more gently) taken to task for taking the existence of capitalism for granted prior to critical interactions of the West with the non-West.
- <sup>3</sup> See e.g. Wallerstein 1974.
- <sup>4</sup> See e.g. Aston and Philpin 1987.
- <sup>5</sup> See Collins 1986.
- <sup>6</sup> These facts would not have been known to Weber.
- <sup>7</sup> See e.g. Burak 2013; Tezcan 2010; see Zarakol 2016 for an overview.

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