



Mutations to the Polder Model

Critical Reflections on 'Exceptionalism' and Continuity in the Low Countries

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In this review of the book *Nederland en het poldermodel* [The Netherlands and the Polder Model], the idea that the 'polder model' dates from the medieval principalities in the Low Countries is qualified. It is argued that the coupling of the plea for continuity and the focus on one area causes problems. First, the exact differences between the Netherlands and other regions with a strong civil society and tradition of negotiation and power sharing appear hard to pinpoint. The endeavours to do so are often artificial and ignore broader developments of which Dutch history is a part. Secondly, the continuity thesis is not made credible (especially with respect with the transition to the nineteenth century) and injustice is done to the contingency and unpredictability of historical developments. The result is a teleological narrative and a missed opportunity for a critical reflection on nationalistic ideas.

Veranderingen in het poldermodel. Kritische reflecties op 'exceptionalisme' en continuïteit in de Lage Landen

In deze review van het boek *Nederland en het poldermodel* wordt de idee dat het poldermodel stamt uit de middeleeuwse vorstendommen in de Lage Landen kritisch tegen het licht gehouden. Er wordt geargumenteed dat de koppeling van het pleidooi voor continuïteit aan de focus op één regio voor problemen zorgt. Enerzijds blijkt het heel erg moeilijk om de vinger te leggen op de precieze verschillen tussen (het huidige) Nederland en andere regio's met een sterke civiele maatschappij en onderhandelingsstrategie; dit gaat gepaard met kunstgrepen en het negeren van bredere ontwikkelingen waarvan de Nederlandse geschiedenis deel uitmaakt. Anderzijds wordt de continuïteit (voornamelijk in de overgang naar de negentiende eeuw) helemaal niet hard gemaakt en wordt onrecht gedaan aan de

contingentie en onvoorspelbaarheid van historische ontwikkelingen. Het resultaat is een teleologisch beeld en een gemiste kans voor een kritische reflectie op nationalistische ideeën.

In response to the trend of economic and cultural globalisation a search is underway in the Netherlands, as in many other regions, to pin down collective and historically rooted identities. This search is sometimes conducted at a local level, sometimes at the municipal level, but mainly on the national and regional level. While the process of nation and state formation continues in Flanders, in the Netherlands too there are increasing calls for a return to traditional values and norms, for the cultivation of typical Dutch virtues and even for a kind of Dutch monoculture. Professional historians are also involved in the hunt for historical roots. Since 2007 we have witnessed not only the controversial historical canon¹, but also the years-long attempt to establish (and build) a Dutch Historical Museum.² These initiatives are of course often critical of all-too exclusive interpretations of the Dutch identity, but they are not completely innocent either. In the Amsterdam Museum the presentation of the history of Amsterdam is currently connected to the concept of 'Amsterdam DNA', which in turn consists of the 'core values' of entrepreneurship, creativity, citizenship and free thinking.³ Though not necessarily exclusive, this is nevertheless normative and so politically charged.

In this context the publishing house Bert Bakker found the courage to start a series on the history of the Netherlands. On an even more daring level, within this series there is a socio-economic history of the Netherlands, *Nederland en het poldermodel* [The Netherlands and the Polder Model], written by Utrecht historians Maarten Prak and Jan Luiten van Zanden.⁴ They are also audacious enough to think up a Dutch model whose existence can be traced back to the late Middle Ages and which helps explain Dutch economic developments and the relationship of these to the social and political institutions of the present day. In short, they are referring to what since the 1990s has been called 'the polder model', and what is described in the book as 'the consensus model' or 'the thousand year old traditions of meeting and consulting' (282).⁵ Later in this review it will be argued that the various interpretations given to the model in various places in the book are problematic, but let me begin by saying that Prak and Van Zanden are well aware of the pitfalls inherent in their undertaking. Early on in the

1 <http://entoen.nu/> (1 July 2013).

2 <http://www.innl.nl/page/405/nl> (1 July 2013).

3 <http://www.amsterdammuseum.nl/amsterdam-dna> (1 July 2013).

4 Maarten Prak and Jan Luiten van Zanden, *Nederland en het poldermodel. De economische en sociale geschiedenis van Nederland, 1000-2000* (Amsterdam 2013).

5 Translation of: 'de duizend jaar oude tradities van vergaderen en consulteren'.

introduction (14) they explicitly state that they are not concerned with a sort of ‘national identity’. Moreover, they are of the opinion that one of the most important features of the social form that they see emerging in the Netherlands of the late Middle Ages is its inclusive nature. In doing so they endorse the concepts of the ‘inclusive society’ put forward by Daron Acemoglu and James Robinson, or the ‘open access society’ of Douglass North, John Wallis and Barry Weingast, which basically state that no single group can entirely advance its own individual interests and that market mechanisms, political institutions and negotiation limit the power of elites.

Nonetheless, I shall argue here that the book does not succeed in confirming the idea of that great continuity without descending into the thinking of DNA or national character. Any reader wishing to penetrate to the level of the mechanisms that might explain the continuity and the unique situation of the Netherlands soon comes up against analytical vagueness, conceptual impurity and rhetorical dexterity. Of course, I understand that it cannot be the aim of a book for a wider audience to include explanations of the scientific debates or even just to highlight them, but the problem here is that the existing scientific literature does not allow for the underpinning of this course either.

In what follows I will first argue that it is anything but obvious to distinguish the developments in the present-day Netherlands from the developments in other regions. Then I shall argue on a more conceptual level that there is too little emphasis on transitions and discontinuities. The bottom-line of my review is that the authors would have done better to approach this project by on the one hand, highlighting the cross-sectional ties with other regions and on the other hand to do justice to the historical fault lines. Had they done this, they could have written a book in which nationalist trends are held up to critical scrutiny instead of being confirmed, as they are here, albeit unintentionally.

Explaining the *Sonderweg*

As far as the economic history of the Netherlands is concerned, the authors endorse the renowned book *The First Modern Economy* by Ad van der Woude and Jan de Vries.⁶ However, they explicitly distance themselves from the idea that the origins of the early birth of the so-called modern economy can be attributed to the weak feudal tradition in the Low Countries. In contrast, they argue that the consensus model emerged precisely from this feudal tradition

6 Jan de Vries and Ad van der Woude, *The First Modern Economy: Success, Failure and Perseverance of the Dutch Economy* (Cambridge 1997).

– the origins of which must be sought well before the seventeenth century. Feudalism in the Low Countries is seen as a system of power distribution, whereby lord and vassal were bound by a type of ‘contract’. In no small part because of the lord’s need for taxes, there is room for negotiation and other parties can demand their share of power. In practice parliaments emerged and cities could acquire political power. Water boards, commons and, in the cities, guilds also developed in this context. At least in part all of these were organised from the bottom up, and they arranged and institutionalised consensus, in this way appropriating some of the power and decision-making. These organisations formed the ‘backbone’ of the ‘corporate society’ of the late Middle Ages and the early modern period. In the long term, from the latter decades of the nineteenth century onwards, they were replaced by the ‘neo-corporate model’, in which power is negotiated not only between political parties, but also between organisations representing both employees and employers. The crucial point here is that a tradition of consensus arose in which power is ‘divisible and negotiable’ (58-59, 281).⁷

The book is a good read and will no doubt appeal to a wide audience. However there are two elements in the book that deserve clarification. First is the fact that the institutions referred to emerged in many parts of Europe. The authors rightly point out that they are referring to the European regions which together form the so-called ‘blue banana’, i.e. a corridor of relatively highly urbanised zones stretching from North Italy across Switzerland and the Rhineland to the Low Countries and southern England. Not only was urbanisation strongest in these regions, but also the resistance to the feudal and territorial state. As a result political fragmentation remained greater there between 1300 and 1800 and there were more opportunities for the Republican state form. From this perspective however, it is not a typically Dutch phenomenon, as a result of which the authors are forced to flit between the concept of the polder model and the broader concept of the consensus model (in the conclusion also referred to as the Rhine model), without sufficiently elucidating the specificity of the Dutch case.

What is clear is that within the broader tradition of ‘consensus’ a unique position is attributed to the Netherlands in general and Holland in particular. The polder model, as indicated on page 17, was not a Dutch invention, but was ‘applied more consistently in the Low Countries and particularly in the Northern Netherlands than in many other regions in medieval and early modern Europe’.⁸ One reason for that is to be found in the countryside. On page 69 the authors mention that the major difference between Holland and other regions arose not so much from the growth of the cities, because that

7 Translation of: ‘deelbaar en onderhandelbaar’.

8 Translation of: ‘in de Lage Landen en vooral in Noord Nederland, consequenter toegepast

dan in veel andere regio’s in middeleeuwen en vroegmodern Europa’.

was indeed a process that took place elsewhere in the Low Countries and the rest of Europe, and often even earlier. It rather arose ‘from the comprehensive transformation that the whole of society – including the countryside (perhaps especially the countryside) – underwent in these centuries’.⁹ In this way a distinction is made from the Southern principalities in the Low Countries (in particular the County of Flanders and the Duchy of Brabant) and North Italy, where cities dominated the countryside more forcefully or developed into the centre of a territorial principality. However this story doesn’t entirely hold water, given that in the Southern Netherlands too (for example, coastal Flanders and inland Flanders) intensive market relations developed between rural and urban areas. Moreover, as in Holland, the less labour-intensive cattle farming was important in coastal Flanders, so the argument that people in the countryside became superfluous and consequently moved to the city also fails to justify the distinction.

A second reason is sought in the specific balance of power between feudal or central authority on the one hand and the autonomy of local groups and collectives on the other. On page 39 the authors mention ‘a subtle play of institutional design, in which elements from above and below came together’.¹⁰ In this specific case the reference is to the emergence of the Rhineland water board (‘the first “modern” water board’¹¹) in the thirteenth century, but similar arguments appear later in the narrative. But the book does not exactly make clear in which cases we can talk of a ‘modern’ form of power sharing and negotiation. Moreover, in the end the success of certain institutional constellations is not measured in terms of the extent to which certain institutions are taken over elsewhere or exhibited continuity, but rather in terms of their economic success. Although the success of the institutions involved could also be measured in terms of standard of living, social equality and well-being, what the polder model ultimately is credited with creating is, first and foremost, a more efficient market. On page 135 the polder model merges completely with the typical market mechanisms such as those, amongst others, that evolved into the VOC: ‘the greatest and undoubtedly most successful business in Dutch history, the VOC, was – allowing for some exaggeration – one big meeting circuit’.¹² In the end, what seems to make the difference is the degree to which market mechanisms

9 Translation of: ‘in de grondige transformatie van de gehele samenleving – inclusief het platteland (misschien wel vooral het platteland) – die zich in deze eeuwen voltrok’.

10 Translation of: ‘een subtiel spel van institutionele vormgeving, waarin elementen van bovenaf en van onderop bij elkaar kwamen’.

11 Translation of: ‘het eerste “moderne” waterschap’.

12 Translation of: ‘Het grootste en zonder twijfel meest succesvolle bedrijf uit de Nederlandse geschiedenis, de VOC, was – met enige overdrijving gesteld – één groot vergadercircuit’.

developed: feudal state structures and other institutions were also required for the efficiency of the market.

Here too it remains unclear precisely in which areas Holland or the Northern Netherlands differed from other regions. First, there is the so-called free-rider problem that was difficult to resolve exclusively with autonomous bottom-up organisations. However, that problem is fairly universal and in North Italy in the late Middle Ages it was often as easily resolved via strong city corporations, which in recent literature are regarded as one of the reasons behind the economic success of the Italian cities and city states.¹³ Second, the success both of farmers in the countryside and merchants in the cities is ultimately attributed to the confidence in the market and the definition of property rights. As regards this argument the authors ignore the shadier sides of corporate institutions and feudal structures (surplus extraction, rent-seeking, et cetera), as highlighted by Sheilagh Ogilvie et al.¹⁴ It is also especially difficult for the reader to follow the causal factors that are hereby assumed. In the introduction mention is made of Robert Putnam, who famously argued that corporate organisations were able to improve economic efficiency because they installed mechanisms for consensus and collaboration, encouraged the flow of information and ultimately increased mutual trust. However, as we know, Putnam saw the origins of this not in the Low Countries and certainly not in Holland, but again in North Italy – where growth from the late Middle Ages onwards stagnated and artisanal guilds turned into inefficient bastions of power and wealth.

The key issue is why the countryside in the Northern Netherlands refused to be oppressed by the cities and a ‘subtle play’ of negotiation continued to exist. While on pages 56 and 74 the weaker position of cities and guilds is described as a strength, on page 100 reference is made to ‘economic development driven by discipline of the market, and constitutional formation driven by the discipline of the arms race between nations’.¹⁵ These can hardly be seen as elements that are present only in the Low Countries. Moreover, in such passages market mechanisms threaten to become both *explanans* and *explanandum*, giving rise to circular arguments. As far as the origin of the functioning of the market is concerned, on pages 60-61 the element of the emergence of feudal property rights and the use of the written word in cloisters surface – again developments that go far beyond Holland and the later Netherlands.

13 See for this, amongst others: S. Epstein and Maarten Prak (eds.), *Guilds, Innovation, and the European Economy, 1400-1800* (Cambridge 2008).

14 See amongst others: Sheilagh Ogilvie, “‘Whatever is, is Right’?: Economic Institutions in Pre-

industrial Europe’, *Economic History Review* 60 (2007) 649-684.

15 Translation of: ‘economische ontwikkeling gedreven door de tucht van de markt, en staatsvorming gedreven door de tucht van de wapenwedloop tussen staten’.

Sometimes the trick is to call a trend that also exists elsewhere in Europe (falling interest rates, rising real salaries or incomes after the first waves of the plague) extraordinary because the accompanying causal factors (falling population) were less evident in the Netherlands (page 76). Elsewhere rhetorical dexterity is used to argue that older developments, as it were, waited for the right moment to evolve and make themselves felt. On pages 76-77, for example, it is assumed that the County of Flanders was initially far more developed than the northern regions and that Holland only broke through this pattern relatively late in the day. In order to be able to connect the late blossoming to the early causes and to avoid geopolitical factors arising as the cause, it is then claimed that Holland was already evolving in the right direction, ‘although that was not very clear initially’ (77).¹⁶ Or there is mention of an older pattern, that only further ‘crystallised out’ (*uitkristalliseerde*) from the sixteenth century onwards (77). Elements such as the low interest rates that were already normal around 1300 in Flanders (page 95) are thus dismissed and the emphasis remains firmly on Holland.

In the end a teleological picture emerges, in which the key elements in Holland germinated and reached their peak in the golden century and from there spread further afield. The authors do acknowledge that Antwerp or a broader region including Flanders and Brabant could equally well have developed further in the seventeenth century (page 100), but that is not really my concern. The point is that in this book a somewhat cunning distinction is made between normal periods and atypical periods. The normal periods (the Revolt, the Golden Age and the period in which the polder model ‘returns’ – ‘*terugkeert*’) are the periods in which the Netherlands is doing well and the institutions are working as they should. In contrast, the period 1815-1840 is described as ‘in certain respects an unknown in Dutch history, due to the unusually large degree to which the central authority, in the guise of King William I, was able to stamp his mark on society’ (193), while of course lines could also be drawn from that period to the current day.¹⁷

Explaining continuity

If I or one of my colleagues in Belgium were to write such a book about Belgian socio-economic history, the origin of the (efficient) consensus model could equally well be situated in the County of Flanders. It could be attributed to the strong guilds in the strong cities, which ensured economic and social

16 Translation of: ‘al was dat aanvankelijk niet zo duidelijk’.

17 Translation of: ‘in bepaalde opzichten een buitenbeentje in de Nederlandse geschiedenis,

door de ongewoon grote mate waarin het centraal gezag, in de persoon van Koning Willem I, een stempel op de samenleving kon drukken’.

successes in the late Middle Ages and up to the sixteenth century, and after a period of difficulty (the period of exception 1650-1750) resulted in the first industrial revolution.¹⁸ From then on, the story would run along pretty much the same lines as that of the Dutch. Because, despite the stereotypes about the meeting culture in the Netherlands, the structures, institutions and consensus practices in Belgium, Germany and other parts of Europe are often very similar. The question therefore, is why the authors of this book chose a story of centuries-long specific continuity – including the period from the end of the eighteenth to the end of the nineteenth century – rather than looking at broader European developments that were specific to certain periods (the late Middle Ages and the period after 1870).

Here we arrive at the second element that requires clarification. The authors themselves point out that the corporative model completely disappeared in the Netherlands around 1800 (in contrast for example, to large regions of present-day Germany) and that the civil society of the *ancien régime* was dismantled. As a historian, I therefore wonder how it could return without being forced to attribute it to a sort of DNA or part of the Dutch ‘national character’. The most concise explanation that I can find is in the introduction on page 15:

The pattern is reproduced over time because on the macro level some rules of the game are laid down in laws, and organisations arose that controlled and forced adherence to those rules. On a micro level people incorporate the rules of social traffic in a certain attitude to life that they deem as more or less natural: ‘that’s how we do things here’.¹⁹

There is a reference here in a footnote to Anthony Giddens’ structuration theory, without any clear explanation as to why.²⁰ Furthermore, nowhere in the book is there an indication of how the authors see this in concrete terms. Had the laws and organisations of the *ancien régime* not completely disappeared by then? How did the people in the mid-nineteenth century incorporate these rules in order to develop from that point new but similar organisations, institutions and models? Is the issue here real continuity at the level of practice

18 This story should obviously refer to Catharina Lis and Hugo Soly, ‘Different Paths of Development: Capitalism in the Northern and Southern Netherlands during the Middle Ages and the Early Modern Period’, *Review* 20 (1997) 211-242.

19 Translation of: ‘Het patroon wordt in de tijd gereproduceerd doordat op macroniveau een aantal spelregels is vastgelegd in wetten, en organisaties zijn ontstaan die de naleving

van die regels controleren en afdwingen. Op microniveau incorporeren mensen de regels van het maatschappelijk verkeer in een bepaalde levenshouding die zij inderdaad als min of meer vanzelfsprekend ervaren: “zo doen wij dat hier nu eenmaal”’.

20 A. Giddens, *The Constitution of Society: Outline of the Theory of Structuration* (Cambridge 1984).



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Pieter Beuckels, *Het bestuur van het Brugse
kleermakersambacht*, 1754.
Stedelijke Musea, Brugge.

and use or rather reinvented traditions? The latter was suggested by William Sewell among others, who argued in his book *Work and Revolution* that in France a strongly corporative discourse persisted in the first half of the nineteenth century, despite the disappearance of the corporate institutions during the French Revolution.²¹ Sewell is talking chiefly about discourses on the noble character of labour and manual skills, but similar arguments emerge in the area of political thought, in which republican ideas are observed to have had a strong continuity.²² However, anyone looking for arguments about the precise nature of the continuity in this book will search in vain. The readers must take the authors at their word and trust that there were indeed mechanisms that ensured continuity.

This is obviously a problem that far exceeds the ambitions of this book, but nevertheless I think it is useful to draw attention to the broader discussions on synchrony versus diachrony. After all, the author that Prak and Van Zanden refer to in the footnote was also accused recently of providing too little explanation and specificity for the terms he used. It is well known that Giddens wanted to transcend the eternal antithesis between on the one hand, determining structures (socio-economic and/or ideological) and on the other hand rational or non-rational actors who can intervene in these structures and therefore ensure change. He did so by speaking of 'structuration' (instead of structure) and of the duality of structures (instead of structure versus agency). For Giddens a structure is both the agent and the result of practices. Actors reproduce (and change) structures but are also activated by structures; they have agency within and through these structures. However, in his recent epic *Logics of History* William Sewell pointed out that Giddens did not sufficiently define the precise nature of structures. Giddens speaks of the 'rules' and 'resources' involved in the reproduction of social systems, but otherwise remains vague. The 'rules' and 'resources' seem to have a predominantly virtual existence, like 'memory traces' in the human brain, which become concrete in actions and which give structure to practices. According to Sewell a more specific vocabulary is needed, or at the very least a better understanding of the content of the terms used. To begin with, he points out that 'rules' can be more or less formal or institutionalised and can encompass both unintentional conventions and laws. As far as 'resources' or power media are concerned, there is not only the question of whether this refers to power over things and power over people, but also whether resources do indeed only exist virtually, or also

21 W.H. Sewell Jr., *Work and Revolution in France: The Language of Labor from the Old Regime to 1848* (Cambridge 1980).

22 Peter Blickle, 'Kommunalismus, Parlamentarismus, Republikanismus', *Historische Zeitschrift* 242 (1986) 529-556; Martin van Gelderen and Quentin Skinner (eds.), *Republicanism: A Shared European Heritage* (Cambridge 2002).

include material elements – from daily items to weapons and factories – and capital.²³

Without this being the place to go into a detailed discussion on this matter, the way in which Sewell redefines this could be instructive for historians who wrestle with this issue of continuity and discontinuity. Sewell chooses to speak about schemes instead of rules (if it does indeed concern the virtual component) and regards ‘resources’ not as virtual but as ‘actual’. He then looks for the duality in the interaction between (virtual) schemes and (actual) resources: resources need schemes and in turn influence the schemes. This might sound abstract, but what Sewell does with this intervention is not only give power a place again (in the form of material resources of power), but also shed a more concrete light on the causes of and obstacles to change. The detailed account of Giddens appears in a book that wants to restore history’s place in social sciences as a whole. To this end, Sewell speaks of an ‘eventful sociology’ and indicates how minor and major events are forever changing the course of history via the complex interaction between virtual schemes and real resources. While the virtual schemes in the heads of actors determine the meaning and agency of material resources, material circumstances (such as demographic factors, but also failed harvests, the accumulation of technology, et cetera) also repeatedly change the reproduction of those virtual schemes. The result is a vision of history in which necessity and inevitability are replaced by path dependency (the largely unpredictable accumulation of history) and teleology by radical contingency (the complexity of the historical context).²⁴

In short, even if some continuity can be established on a rhetorical or institutional level, we must guard against eliminating the historical context by placing an excessive emphasis on continuity. In the case of the polder model moreover, the French and Batavian revolutions must have been significant ‘events’, which together with the industrial revolution and technological developments, fundamentally changed the course of history. Given that most of the formal rules and institutions involved in consensus making and power sharing disappeared around 1800, it seems plausible that in the first half of the nineteenth century the polder model lived on mainly in the form of ‘memory traces’. The extent to which these ‘memory traces’ were translated into practices of consensus and negotiation however, is still very much a subject of debate. The nineteenth century after all could equally well be described as a century of conflict and confrontation rather than of consensus and negotiation. At the point at which consensus and negotiation were re-institutionalised (and appear in the sources), technological and material conditions had changed so much that a mismatch between schemes and resources must have been inevitable.

23 W. Sewell, *Logics of History: Social Theory and Social Transformation* (Chicago 2005) chapter 4.

24 *Ibid.*

The most plausible scenario therefore seems to be that the consensus model survived in discourse, which immediately throws a different light on the book by Prak and Van Zanden. Given the lack of scientific ambition to really confirm the continuity hypothesis, this book will itself become part of the tradition in which the corporate model from the nineteenth century onwards is ‘invented’ or (but to what extent?) reinvented as the ideal model of society. There is nothing wrong with that per se – the illusion that science can be neutral and apolitical has long since been shattered – but as a critical reader I did expect to read more about alternative explanations and opposing voices. They are thin on the ground in this book.

Conclusion

It is of course a little unfair to review a book intended for the general public as if it were a book with purely scientific ambitions. I certainly applaud the ambition to reach a wider audience – the more so since this is an attempt to inform readers about the far less popular socio-economic dimension of the history of the Netherlands. However, we should still ask ourselves what message this book ultimately really wants to broadcast, and from that perspective there are not insignificant comments to be made. The decision to write a national history and then link it to firm ideas about continuity is unfortunate in my view. Firstly, a certain degree of artificiality is required to be able to situate the origins of the consensus model – the lens through which the past is viewed in this book – in the core region of the modern-day Netherlands, and not for instance in Northwestern Europe (including present day Flanders and the Rhine Valley) or North Italy. A second, related, problem is that a teleological vision emerges in this book, whereby a continuity is described without any explanation, let alone examination, of the underlying mechanisms. It might sound a little paradoxical, but a real historical analysis presupposes first and foremost a search for change.

Surprisingly enough these are two historians who have a very broad view and an intense awareness of the importance of comparative research. The problem however, is not the lack of comparison, but that in this book they were forced as it were to cut through the synchronistic cross-connections between the developments in Holland and the Netherlands and the developments elsewhere in Europe because of the national perspective – and that precisely at a time when many historians are renouncing the focus on national history in exchange for what some call *histoire croisée* or ‘entangled history’.²⁵ Many political historians rightly argue that the emergence of, say, parliamentary

25 See, amongst others: Joachim Werner and Bénédicte Zimmermann, ‘Beyond Comparison:

Histoire croisée and the Challenge of Reflexivity, *History and Theory* 45 (2006) 30–50.

institutions and democratic practices cannot be understood from a purely national perspective, but on the contrary must be explained from a perspective in which developments at the national level are connected to networks, discourses and practices at a supralocal and supranational level. I think this is also an inspiring perspective for socio-economic historiography. Following on from this, there is a need for more reflection about the question on what scale exactly which sort of phenomenon should be investigated, and on what scale precisely which causal links are situated. In my view, if this approach had been taken, the book would have been more challenging. ◀

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