

The Netherlands and the Polder Model: A Response

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In response to the contributions by Davids-'t Hart, De Vries, and De Munck we emphasise that our book *Nederland en het poldermodel* [The Netherlands and the Polder Model] has been written for a general audience and therefore does not provide a detailed theoretical framework, nor a large number of graphs and tables. We have focused on the territory of the Netherlands, fully aware that this was not a (politically or economically) coherent territory before the sixteenth century, but any other choice would have been equally arbitrary. In the Middle Ages the region developed much like other, neighbouring parts of Western Europe, but whereas elsewhere the rise of centralised states and absolutist monarchs ended the development path based on bottom-up institutions, the successful Dutch Revolt and the formation of the decentralised Dutch Republic ensured much more continuity. We share the assessment of our critics that the transformation from this institutional structure via the mid-nineteenth century phase of 'liberalisation' into the new corporatism of the late nineteenth and twentieth centuries is still incompletely understood.

Nederland en het poldermodel. Een replek

In reactie op de bijdragen van Davids-'t Hart, De Vries en De Munck benadrukken we dat het boek *Nederland en het poldermodel* geschreven is voor een breder publiek en daardoor niet het uitgewerkte theoretische kader noch een overdaad aan tabellen en grafieken bevat. We hebben ons daarbij gericht op het grondgebied van het huidige Nederland, hoewel dat tot in de zestiende eeuw geen duidelijke eenheid bezat – in economische noch in politieke zin. Tot ver in de middeleeuwen kende dit gebied een ontwikkelingsgang die sterke parallellen vertoonde met de rest van West-Europa. Maar daar waar elders de middeleeuwse erfenis van institutievorming van onderop met de opkomst van gecentraliseerde staten en absolutistische

ambities afgesloten wordt, zet deze ontwikkelingslijn zich in de Republiek met haar gecentraliseerde staatsvorm voort. We delen het oordeel van onze critici dat de complexe overgang van deze vroegmoderne institutionele structuur naar het twintigste-eeuwse poldermodel, een overgang die gekenmerkt wordt door een fase van liberalisering en institutionele hervormingen (vanaf 1798 tot ongeveer 1870) gevolgd door een hernieuwde beweging van ‘bottom-up’ collectieve actie, nog meer duiding verdient.

In March 2013 we published the book *Nederland en het poldermodel* [The Netherlands and the Polder Model] on Dutch social and economic history that has been generously reviewed in the previous pages of this journal by four distinguished colleagues.¹ Despite the fact that all four reviewers are early-modernists, our book also covers the Middle Ages and modern era – and so do some of the comments. In fact, the book covers approximately one thousand years of history. It is an attempt to sum up a lot of work that we, as well as dozens of colleagues inside and outside the Netherlands, have published about the development of the Low Countries, but also about Europe as a whole. The book focuses on what we see as the outstanding feature of Dutch history, i.e. the interactions between often small-scale institutions and large-scale commercial networks. The former created ‘agency’ for a lot of inhabitants of the Low Countries during much of this one-thousand years history, the latter helped engender a level of prosperity at a relatively early stage in history that made the Netherlands perhaps not unique, but placed it in a quite exclusive group of unusually prosperous countries. The interactions between localised institutions and global commercial networks moreover, allowed Dutch merchants to dominate the world-economy of the seventeenth century and to produce a remarkable number of large multi-nationals in the twentieth century. In our book we claim that all of this took place within a societal context that throughout this long period displayed three key features: high levels of participation in civic institutions and organisations, a high degree of civic influence, and low levels of social inequality. These features define what we call the ‘polder model’ society.

Even though this is acknowledged by all four reviewers, for the readers of this journal it could still be helpful to clarify the position of the book, and some of the practical decisions we were forced to take while writing it. *Nederland en het poldermodel* was commissioned by the publisher for a series on Dutch history that otherwise employs a chronological framework. Separate volumes have been published on the Dutch colonial empire, which we have

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

therefore more or less ignored. The other volumes cover a limited period, although some still treat several centuries. Ours however, was supposed to cover all ages, even though we have chosen to leave out the pre-historical period, Antiquity and much of the Middle Ages. The long period covered by the book required choices in terms of selection and geography.

Geographical coverage will always be a problem for a series like this. For more than half of the period covered by our book, the Netherlands as a country did not exist. Inevitably, any history starting before 1566, or even 1609, will have to imply a serious amount of anachronism. We have solved this by highlighting regional variations for those earlier centuries. For some of that earlier period what is now the Netherlands was joined with what is today Belgium, although it is easy to overstate that connection as for most of the Burgundian-Habsburg years (c. 1430-1566) only Holland and Zeeland were ruled by the same sovereign as Flanders and Brabant. Most of the regions that would ultimately form the Dutch Republic were only added during the reign of Charles V (1515-1555), Guelders as late as 1543. Even if we take into account the joint period under William I, from 1815 to 1830, it would have been equally anachronistic to have treated Belgium and the Netherlands equally throughout the book, as if they had always been a single country. We have solved this problem by paying extra attention to the southern regions in those chapters when the Netherlands and Belgium were joined in political union (82-102, and 192-202).

Historical information about the one thousand years covered by *Nederland en het poldermodel* is so abundant that it would not have been a problem to write a book double or triple the current length: but who would have wanted to read such a book? Given the fact that this book was primarily destined for a non-specialist audience, we decided that we wanted to achieve two things. First of all, we wanted to give readers a clear idea of the main developments, without overloading them with detail. Second, we wanted to connect our story of the past to some issues that our readers might worry about in the present. We wanted, in other words, to write a history that is not just interesting in its own right, but also reflects on current issues. This was a hazardous strategy for two reasons. First, it goes against the grain of academic professionalism as it has developed in the last half century or so. In the 1950s and 1960s it was still customary for historians to provide historical background to current issues in newspaper columns and radio broadcasts. Nowadays, many professional historians prefer the position that historical events are unique and unrepeatable, and therefore provide no lessons whatsoever. We, on the other hand, think that today so much historical research funded by the tax payer is undertaken that historians cannot afford to stay aloof inside their ivory tower. Using historical evidence to uncover patterns that will shed light on current issues however, implies a second hazard, which is that one takes a stand in a political debate. We were forcefully reminded of these two risks by reviews in the *Volkskrant* and *Historisch*

Nieuwsblad, incidentally written by a political commentator and a bookseller respectively, both trained as Ancient historians and with no expert knowledge of Dutch history, who condemned our book for precisely these reasons.² It is reassuring to see that none of the three reviews in this journal finds our position in itself objectionable.

Dauids-'t Hart however, are critical about the theoretical framework that we employed to make the radical selection of the available evidence that we needed to create a text that would be both succinct and relevant. In a book like this there is only limited room for theoretical discussion and we have to acknowledge that we had to cut some corners to create the summaries of sometimes complex positions. On the other hand, we are not persuaded by all of their arguments. Their main concern is that we find the roots of the 'open access' society (as analysed by North, Wallis and Weingast) already in the Middle Ages. We supply two arguments for this: large parts of the Low Countries saw the emergence of a market economy in which decisions about the allocation of labour, land and capital were coordinated by market forces. Second, people organised themselves in relatively large numbers in 'bottom-up' institutions, such as brotherhoods, communes, guilds, commons. This 'silent revolution' was not managed or blocked by the state, and resulted in a disciplining of feudal elites through the formation of effective countervailing powers (by communes, via parliaments). The organisations that resulted were impersonal (another important feature of the North, Wallis and Weingast approach) and were rooted in a quasi-egalitarian ideology characteristic of the medieval communal movement.³ Dauids-'t Hart correctly note that these guilds and other forms of cooperative collective action organised a minority of the population, but it was a substantial minority – much more substantial anyway than the membership of modern political parties. We all know that modern democracies suffer from commitment problems, and so, no doubt, did the pre-modern Low Countries. The point we tried to make however, is that remarkable numbers of people were members of organisations that were in one way or another involved in politics. The opposition between the era

2 Martin Sommer in *de Volkskrant* (9 March 2013); Bastiaan Bommeljé in *Historisch Nieuwsblad* (May 2013).

3 Tine De Moor, 'The Silent Revolution: A New Perspective on the Emergence of Commons, Guilds, and Other Forms of Corporate Collective Action in Western Europe', *International Review of Social History* 53 (2008) 179-212; Antony Black, *Guilds and Civil Society in European Political Thought from the 12th Century to the Present* (London 1984) 68-75.

of democracy and the centuries preceding it, we argue, therefore has been significantly overstated as far as the Netherlands is concerned. It is the same point that Putnam has made about Italian city-states of the Renaissance. These civic organisations, we have tried to demonstrate, forced local and regional elites to engage with popular concerns. They did so willingly, because they felt that the alternative, i.e. princely rule, was even more unattractive. In the book we quote (104) the Amsterdam mayor C.P. Hooft, who in 1584 insisted that not only the middle class men serving in the civic militias and ‘all other citizens’, but also the sailors (*het zeevarende volck*) would be asked to express themselves about the conferment of the title of Count of Holland on the then stadtholder William of Orange, because ‘the condescension and love of the common people has favoured our cause [...] and we would not want to turn it into an enemy’.⁴ This sort of attitude indeed created the features that North, Wallis and Weingast define as ‘open access societies’, and Acemoglu and Robinson as ‘inclusive societies’. Even in today’s society opportunities are not equally available to all. Progress has been made in the past five hundred years, but we think that we were able to demonstrate that already in late-medieval Holland elites were no longer in a position to take the economy for a ride. As a result of political fragmentation and competition, local and regional elites had subjected themselves to the rule of law and the laws of the market, and by implication given non-elites the same position.

A second issue that worries the reviewers is continuity and discontinuity. Is the medieval and early modern polder model really comparable to the post-1870 polder model, Jan de Vries asks. In the book we distinguish three institutional varieties of the polder model: an agrarian (eleventh to sixteenth centuries), a commercial (seventeenth and eighteenth centuries), and an industrial (nineteenth to twenty-first centuries). Remember that we have defined the polder model as a society with high levels of organisation and political participation, and low levels of inequality. We think that those features were well in evidence during all three stages. Institutionally there were clear continuities from the first to the second phase, even though some institutions played a radically different role after the Dutch Revolt, compared to previously. Not only had the sovereign (‘landsheer’) disappeared, but as a result the provincial States and the States General were in a completely different position, and local councils were all of a sudden creating foreign policy.

While in some areas there were also continuities across the second great divide, the Batavian Revolution and its aftermath, for example with the polder boards (*waterschappen*), in most there was not. Both De Munck and

4 *Memoriën en adviezen van Cornelis Pietersz. Hooft*, part 2, H.A. Enno van Gelder (ed.) (Utrecht 1925) 8-9.

De Vries point out that we supply no convincing explanation for the revival of the consensus-oriented institutions in the second half of the nineteenth century and therefore, as De Munck insists, still have little else to offer than ‘national character’, a type of causality that we expressly reject in the Introduction of the book (14-15). We have to agree with our critics that this is probably the least persuasive part of the book. To some extent the revival of corporatism in the nineteenth century was inspired by the memories of earlier organisations, especially the guilds. Both Groen van Prinsterer and Kuyper, two of the founding fathers of modern politics in the Netherlands, in their criticisms of the liberal state explicitly referred to them as an alternative mode of organisation.⁵ As De Vries points out, these ideas were especially popular in the various churches and their political off-shoots. Religious organisations, more than any other type of organisation, claimed the inheritance of the corporatist tradition, even though they themselves had not been a major aspect of that tradition before 1800. Why this had to be so, is still poorly understood.

The reviewers are also worried by the balance between Dutch uniqueness and general European patterns. That concern has also bothered us. Given the fact that the book was commissioned for a series on Dutch history, the suggestion by both De Munck and Davids-’t Hart that we should have written a comparative European study is attractive but unrealistic. It is quite obvious that many of the developments that we discuss in the book were European, rather than Dutch developments. We have tried to indicate this in many places, for example when discussing the development of specific European institutions in the Middle Ages (chapters 1 and 2), or the world economy in the twentieth century (chapter 5). In a global perspective, these often relatively small differences between European countries might count for little. When writing a national history, on the other hand, the point is precisely to highlight those smaller variations to identify the different paths taken by the different countries. The Dutch development path in particular was ‘unique’ during the early modern period because the republican political institutions rooted in well-developed civil society inherited from the Middle Ages persisted, thanks to the successful Revolt against Spanish Absolutism. This ‘exceptionalism’ also had important consequences for social and political institutions during much of the eighteenth and nineteenth century, and for the specific development path of the economy, including the pattern of industrialisation during the nineteenth century. As we point out in the Conclusions of the book (278-279), those ‘unique’ features were shared by a small number of other European countries, most notably Italy, Switzerland,

5 For further references Maarten Prak, ‘Corporatism and Social Models in the Low Countries’, to be published in *Tijdschrift voor Sociale en Economische Geschiedenis* 11 (2014).

Germany and Belgium, all situated in what is called the ‘blue banana zone’. De Munck and De Vries both wonder why Belgium did not take the same path. Part of the answer is that it did travel along much of the same road as the Netherlands. At the same time, the Dutch Revolt did create a fork in the road. Precisely because they were economically so important, the cities of Flanders and Brabant were conquered by the Spanish troops. The result, as we all know, was that much of their financial and human capital moved to the North, at one and the same time promoting the Dutch economy and weakening that of the South.

Finally, an issue that transcends the historical framework, but is indeed crucial to the message of the book: was the polder model beneficial to the economy, or was its success really due to the benevolent ‘regenten’-regime, as Jan de Vries insists? Apart from the perhaps too obvious observation that the ‘regenten’ and their attitudes were actually part and parcel of the polder model, it is correct to say that we have not been able to fully deal in our book with questions of causality. There is a substantial literature claiming that at crucial points in the post-Second World War period, both immediately after 1945 during the Reconstruction era, and again after 1982 (Wassenaar Accord), the agreements between labour, capital and the government were indeed beneficial to the economy. However even for this recent period it has proved difficult to provide compelling evidence for a causal relationship. In the book we also accept that many more factors besides *polderen* [meeting and consulting] contributed to these positive outcomes. We also stated in various places (15-16, 161-165, 261-265) that the compromises hammered out between various societal stakeholders do not automatically generate economic growth, and could indeed also create stalemate and prevent necessary reforms. We therefore suggest (198-201) that the liberal reforms of the mid-nineteenth century, achieved outside the ‘polder’-context, were a pre-condition for renewed growth. The neo-corporatist structures were only created as growth was gathering pace, partly in response to precisely these developments.

For the late Middle Ages and the early modern period, given the lack of solid quantitative data, it is even more difficult to establish such a causal relationship between institutional structures and Holland’s relatively high levels of prosperity. Nonetheless, in a number of specialist papers we have attempted to make this plausible. A strong connection was found for example, between growth and political representation in the pre-industrial period.⁶

6 Jan Luiten van Zanden, Eltjo Buringh and Maarten Bosker, ‘The Rise and Decline of European Parliaments, 1188-1789’, *Economic History Review* 65 (2012) 835-861.



The 'Wassenaar Accord' of 1982 is a recent example of the Dutch style of policy making in which agreements between labour, capital and the government play a central part. In the picture, Wim Kok (left) of the FNV [Netherlands Trade Union Confederation] and Chris van Veen of the VNO [Confederation of Netherlands Industry] have just signed the Accord, 4 November 1982.

ANP Photo.

More specifically, we have looked at institutional elements that coincided in time with the ‘Little Divergence’, i.e. the growth spurt of the North Sea area in this period. We think we were able to demonstrate a positive impact of human capital, also in relation to the rise of Protestantism, and of citizenship.⁷ These studies substantiate the New Institutional Economics’ claim that institutions were a crucial ingredient for economic growth – or indeed its absence.⁸ Our studies tend to see the causal order as going from institutions to economic performance, even though they sometimes also take a reverse causality into account. Assuming that institutions were merely redistributive or outright obstacles to growth would make it even more difficult to explain the extraordinary developments on both sides of the North Sea during this period. We do not shy away from the fact that corporatist institutions and their successors had negative consequences for some people or during some stages of the thousand years covered by our book. We do think however, that in the long run in terms of well-being, material or otherwise, societies that promote citizen organisation allow their members political agency and protect social equality tend to be more successful. Our book argues, in other words, that the historical record of the polder model has been a positive one over all. ◀

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7 Jan Luiten van Zanden and Maarten Prak, ‘Towards an Economic Interpretation of Citizenship: The Dutch Republic between Medieval Communes and Modern Nation States’, *European Review of Economic History* 10 (2006) 111-147, and in a slightly different version: Maarten Prak and Jan Luiten van Zanden, ‘Tax Morale and Citizenship in the Dutch Republic’, in: Oscar Gelderblom (ed.), *The Political Economy of the Dutch Republic* (Aldershot 2009) 143-165; Alexandra M. de Pleijt and Jan Luiten van Zanden, ‘Accounting for the “Little Divergence”: What Drove Economic Growth in Pre-Industrial Europe, 1300-1800?’ (working paper, Centre for Global Economic History 2013).

8 See also Bas van Bavel, *Manors and Markets: Economy and Society in the Low Countries, 500-1600* (Oxford 2010), who prefers to talk in terms of ‘socio-institutional structures’; and Oscar Gelderblom, *Cities of Commerce: The Institutional Foundations of International Trade in the Low Countries, 1250-1650* (Princeton 2013).