

Bas van Bavel

The medieval Low Countries as a testing ground for new ideas on economic and social development.

A reply from the author of *Manors and markets*

*The reviews of the book *Manors and markets* bear witness to the flourishing state of economic and social history of the pre-industrial period in the Low Countries, the lively debates, and the new and promising roads taken in this field. They also indicate a number of important lacunae in our present knowledge, most clearly with respect to ecological changes, human capital formation, social inequality and indicators of welfare. Further progress in these fields will produce a fuller picture of the long-term changes in the economy and society, and of the geographical differences they display, and a better understanding of their causes.*

The preceding reviews of my book *Manors and markets* are written by some of the finest members of a new generation of economic and social historians of the pre-industrial Low Countries. Some of them, like Jean-Pierre Devroey and Erik Thoen, are already well-established experts, while others are talents storming the ladder of academia. In their research, they all build on the great work done by their predecessors, including Adriaan Verhulst, Walter Prevenier, Raymond van Uytven and Wim Blockmans, who have pushed our knowledge of the pre-industrial economy and society – and especially of the late Middle Ages – much further. At the same time, these reviewers tread new roads. They are the exponents of the new directions the economic and social history of the pre-industrial period is taking. They each represent a sub-field where in recent years exceptional progress has been made in Belgian and Dutch historiography: early medieval history, agrarian history, late medieval urban history, ecological history and quantitative economic history. All of these reviewers, and even the younger ones among them, are used to linking up their research to the bigger questions posed in international historiography, and sometimes actively contribute to the theory formation or the rise of new ideas in their discipline, bearing witness to the flourishing condition of the field in the Low Countries. I feel honoured that they were willing to review my book.

Looking at their reviews is sobering in a way. They all take a common line: ‘*Manors and markets* is wrong in my own sub-field of expertise, but apart from that it is a great book’. This is a very worrying compliment for its author. On the other hand, I console myself with the fact that the overall framework of the book seems to be endorsed by most reviewers. There is consensus on the value of investigating very long-run developments and the focus on the region as the main unit of economic and social development. These two aspects were

also the ones where the book aimed to make the most progress, since no comparable studies existed. In writing this book, I set out to break down the historiographical barriers of the nineteenth/twentieth-century nation-states, and their national histories, and those of the traditional periodisations, and the ensuing cleavage between early medieval and late medieval, and between medieval and modern history, and employ the research potential offered by crossing these traditional lines.

More debatable, but still accepted by most reviewers, is the idea of path-dependent development at the regional level, as proposed in the book.¹ The starting point of the book is the striking regional diversity within the Low Countries, and the observation that these differences are not flattened out over the centuries as a result of increasing exchange, growing urban markets and political unification. Instead, the boundaries between these regions remain and the differences between them become even more pronounced in the course of the Middle Ages. By way of the book I want to capture the opportunity this regional diversity offers as a new angle to investigate economic and social development, an opportunity hardly used as yet, and to contribute to understanding its causes better. In doing so, the book on the one hand stresses continuity and shows how long-term development is to a large extent shaped by existing social and institutional arrangements at the regional level, which originated in the early Middle Ages. On the other hand, it analyses how – within this path-dependent process – there is a constant alternation in pace, nature and success of regional development, with ever new core regions taking the economic lead.

Some reviewers ask themselves what the prime mover in this process is, or reproach me for leaving this open.² Maybe this is the result of a common misconception. I think the concept of ‘the prime mover in history’ is a false one. It should be disentangled and separated into, on the one hand, the forces which create dynamism and development (the movers) and, on the other, the elements which determine the direction of developments (the prime determinants). I see it as the motor of a car which does not determine what road the car will follow. The movers or motors of development are the classical ones: technological innovation, transport revolutions, growing towns and expanding urban demand, and population growth or decline. All of them are found in the medieval history of the Low Countries, and all of them exercised a dynamic force, and as such they are extensively treated in the book. But none of them determined the direction of dynamism and the path a region’s economy and society would take as a result of this dynamism, as evident from the divergent effects of similar forces. Population decline could go hand in

1. Cf. also the discussion of the conceptual framework of the book in the review by Tim Soens and Erik Thoen.

2. See the contribution of Peter Stabel in this volume.

hand with restrictions on labour mobility, coercion or the freezing of wages, or with increasing bargaining power and freedom for labour. New technology could be used to enable extensification by substituting capital for labour, or be employed to develop more labour-intensive production methods. Even growing urban demand, long favoured by scholars as a determining force, in part because of its striking presence in the medieval Low Countries, turns out to have had divergent effects. The growing urban markets in the late Middle Ages, for instance, stimulated scale-enlargement and a fairly labour-extensive agriculture in the Guelders river area and coastal Flanders, while the same market demand exercised by the same towns stimulated agricultural and proto-industrial intensification and extreme fragmentation of holdings in neighbouring Holland and inland Flanders, respectively.³

These explanations for divergences are not rejected off-hand in the book, as Jord Hanus seems to suggest. Instead, by way of its regional analysis, the whole book is a demonstration of their inadequacy to offer explanations. They can help to describe and explain certain developments, of course, but do not offer explanations of the differences between regions. These differences, or even divergences, are determined by the socio-institutional constellation embedded in the region itself. This functions as a prism, diverging the mentioned dynamic forces. It operates at all kinds of geographical levels, including the local one, but most clearly at a regional level. The focus of the book is concentrated at this level, where the characteristics are sharpest.

Before looking at these regions and their delineation, I first would like to offer some more detail about these socio-institutional arrangements. I did not exhaust myself in long definitions of institutions, as observed by Jord Hanus. The thirty-page explanation of institutions by Avner Greif, which Hanus refers to, has produced in me a kind of academic shock and awe, instead of a deeper understanding. I have rather kept it simple and focused on the formal and informal rules of exchange and allocation of land, labour, capital and goods, and the associated organisations. These institutions are not formed, or at least not solely, in response to economic or ecological needs, but have developed within the bargaining around conflicts of interest between people and social groups. I have always placed them within their social context. It is precisely the interaction between institutions and social context that produces their different characteristics. Regions sometimes have similar institutions, of course, but there are two provisos. Firstly, even small institutional differences can produce different effects. Secondly, even if institutions are the same, their effect differs widely because of the social context: who uses them (very different people/groups), what is the user's goal (very different per person/group) and who is affected (what kind of people/society). This creates

3. Van Bavel, *Manors and markets*, 294-298.

a triple divergent effect. A striking illustration is the role of short-term leasing in late medieval Flanders. Although the institutions of the lease system were broadly similar, in inland Flanders, dominated by small-scale peasant landholding, the leased land was fragmented into tiny parcels, used by peasants in a highly labour-intensive way, while in coastal Flanders the leased land was used by big agricultural entrepreneurs to push the process of scale enlargement.

A lot still needs to be done, but the book has systematically assembled and analysed the most relevant institutional arrangements. It offers an analysis of the regional differences in the way in which overlapping property rights to land slowly gave way to exclusive, absolute rights in private hands in the later Middle Ages, and how this paved the way in some regions for competitive short-term leasing (pp. 162-178). It also reconstructs the changes in the institutional organisation of markets for commodities, labour and capital, in part building on empirically well-grounded, recent PhD theses.⁴ This institutional approach is applied throughout the text, but consistently linked to its social context. Subsequently, I have tried to show how the way groups of people shape and apply institutions has divergent effects on the development of economy and society, most clearly at the regional level: leading to economic growth or stagnation, to the application of new technology or not, to increasing specialisation or not, or to demographic growth or not. The book does not offer an empirical investigation of how this worked out at the micro-level, since this would exceed its scope, but offers an analysis at the macro-level of the regions.

When one accepts the determining influence of socio-institutional arrangements on the development of economy and society, the question becomes why they were sometimes favourable and sometimes not. The book's contribution, I think, is mainly in demonstrating how and why this influence develops from a positive to a negative one. It shows how the economic growth resulting from a favourable institutional arrangement of exchange results in growing polarisation, an ever more skewed distribution of property and power, allowing groups to shape institutions to their interests and not be adapted to more general needs in the face of changing contexts, resulting in stagnation or even decline of the region in question. In this respect, the book offers a model of the interaction between economy, society and institutions to be tested in further research. But how, as rightly asked by Peter Stabel, do these regions start out with a positive constellation? The book is less clear about this, but it offers some clues.

4. C.J. Zuijderduijn, *Medieval capital markets. Markets for renten, state formation and private investment in Holland, 1300-1550* (Leiden, 2009), and J. Dijkman, *Shaping medieval markets. The organisation of commodity markets in Holland, c. 1200-c. 1450* (Leiden 2011).

The crucial element is a balance of power and property: a balance between social groups, allowing institutions of exchange to be shaped according to general needs, instead of private interests. Where does this social balance come from? One possibility investigated in the book is that successful revolts or revolutions by non-elite groups created it. I have discarded this possibility, however, on account of the fact that these medieval revolts all appear to be defensive in nature and mainly sought to consolidate the position of non-elite groups against the advances of threats by the elites.⁵ In this sense, the agency of people seems limited. Still, they are not impotent pawns on the chessboard of history. I believe the way people organise their lives, their interaction and exchange – albeit within the strict limits determined by existing arrangements and power relations – is more determining for the development of society than abstract, non-human forces like the climate, the soil or the neo-classical market. And even if the margins are small, non-elite groups can make advances, and these are found in broad and slow developments, most specifically the gradual advances in self-organisation and self-determination, allowing them to shape interaction and exchange themselves. The biggest example is the formation of countless numbers of associations in the high Middle Ages, including the guilds, merchant associations, commons, town communities and villages.⁶ Again, these voluntary associations of ordinary people do not come out of the blue, but probably build on early medieval foundations (p. 93), a continuity hardly investigated as yet, in part because of the scarcity of sources. In those regions where they developed in massive numbers, became formalised and gradually acquired more power, they contributed to a social balance.

The resulting constellation made the region in question well-positioned to benefit from new opportunities created by, for instance, advances in technology or growing market demand, and as a result propel itself forward. In a way this may look like the Sleeping Beauty who is suddenly kissed and awakened by the prince of history, but such a comparison would show a lack of appreciation of the deeper, long developments underlying this moment, although the causes of the slow genesis of this favourable situation, and its absence in other cases, indeed deserve more research in the future.

The negative feedback cycle, leading to the stagnation and relative or even absolute decline of regions, is more extensively elaborated in the book. An important element in this stagnation is the erosion or decay within the existing institutional arrangement of exchange, because of the concentration of property and power in the hands of an ever smaller group of people, and also

5. Van Bavel, *Manors and markets*, 271-278.

6. Aptly called a silent revolution by T. 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 (suppl. 16) (2008) 175-208.

the impossibility to develop this arrangement further in order to make it better suited to new circumstances, leading to these regions being overtaken by other ones offering a more favourable arrangement or more flexibility. Institutional organisations which had favourable effects under certain circumstances therefore lost them over time, as these circumstances were altered by economic, ecological or technological developments. A recently investigated example of decay from within is offered by the water management boards in coastal Flanders, where the land in the course of the late Middle Ages had accumulated in the hands of urban investors who were only mildly interested in maintaining an expensive water management infrastructure, while at the same time the land users and local inhabitants became excluded from the decision-making processes, elements which negatively affected the institutional arrangements and their effects.⁷ Another traditional example, which despite recent nuances has not lost its relevance, is the way the Flemish and Brabantine guilds in the fifteenth and sixteenth centuries blocked the introduction of new labour-saving techniques and the associated scale enlargement of production, in part by way of explicit restrictions but even more so indirectly, by way of the organisation of production that the guilds supported and protected. In an earlier period, the same guilds had helped producers in the Flemish and Brabantine towns to reap the possible gains of growing markets and better transport opportunities, and promoted and enabled further development.⁸

These examples show that the different organisations, or coordination systems, did not per se have a positive or negative effect on the economy or society, but had an effect that depended on their exact organisation and the context in which they functioned. The emergence of manorial organisation in the eighth and ninth centuries, for instance, contributed to specialisation, growing surpluses and urbanisation, and perhaps in some of its less rigid varieties even offered personal advantages to peasants, as suggested by Devroey and Wilkin. With the development of transport, technology and markets, however, other coordination systems became more conducive to economic growth, especially since they offered a greater degree of balance between the social actors and better opportunities to reap the benefits of these changes. So, rather than hesitating about the effects of manorial organisation and their link to urbanisation – as Devroey and Wilkin think I do – the book treats these effects as depending on their exact arrangement and context.

The same applies to the role of the guilds, most notably in Flanders, as discussed above. Peter Stabel reproaches me for my negative judgement of

7. T. Soens, 'Polders zonder poldermodel? Een onderzoek naar de rol van inspraak en overleg in de waterstaat van de laatmiddeleeuwse Vlaamse kustvlakte (1250-1600)', *Tijdschrift voor sociale en economische geschiedenis* 3 (2006) 3-36.

8. B. van Bavel, *Manors and markets*, 346-349 and 361-364.

their effect, but for the earliest period – eleventh to thirteenth centuries – this judgement is clearly positive. This changes, however, as power within the guilds becomes more skewed, and they come closer to being interest groups aimed at defending vested interests, for instance *vis-à-vis* rural producers. Moreover, at the same time, new opportunities are offered by improved transport, technology and growing market demand, and these opportunities are used more efficiently by the new coordination system offered by open, free and competitive markets for land, labour, capital and goods, as most clearly found in Holland. This positive effect of markets, however, in its turn disappears around 1600, as the social polarisation resulting from sharp market competition and the growing economic and political power of merchant elites, and their increasing use of coercion, erodes the positive effects.⁹ This book, as rightly noted by Peter Stabel, does not tell a story of the inexorable rise to the final destination, the paradisiacal state of open markets, as epitomised by Golden Age Holland, but instead it shows the necessity of constantly developing new systems of exchange and allocation.

How should these regions be delineated? Petra van Dam is critical of the supposed absence of definition of a region in the book. Partly she is right, since the definition is further developed in the course of the book. The initial definition is: these regions have clear specific characteristics in soil, occupation history, social structures and institutional organisation of the rural economy, separating them from surrounding regions.¹⁰ And the identity of these separate regions was strengthened by the fact that many of them were also designated as counties in the Frankish period, adding a political dimension to their separateness.¹¹ The discovery of further constituent elements coming to the fore in the later Middle Ages, especially in the field of regional economic specialisation, should have been reflected in a more extensive definition in the conclusion, however.

Compared with political boundaries, which are traditionally used in historical research and often unquestioned, this definition is less easy to apply, and the boundaries less easy to indicate exactly. The traditional political boundaries, however, are to a large extent irrelevant to economic and social developments, and blur an analysis of these developments and their underlying causes. This applies not only to the political boundaries of modern nation-states, but also to those of the late medieval principalities within the Low Countries. In the search for a geographical unit smaller than the nation-state, these principalities or, later, provinces do not offer an alternative. Witness the social and economic differences, or even divergences, between coastal

9. Cf. also Bas van Bavel, 'The medieval origins of capitalism in the Netherlands', *Bijdragen en Mededelingen voor de Geschiedenis der Nederlanden* 125 (2010) 45-80.

10. Van Bavel, *Manors and markets*, 25-26

11. Van Bavel, *Manors and markets*, 70.

Flanders and inland Flanders within the county of Flanders, or those between the Veluwe and the Guelders river area within the Duchy of Guelders, or, conversely, the similarity between parts of different principalities, like the Holland peatlands and the adjacent ones in the Nedersticht Utrecht.

To some extent, the regions employed throughout the book are like the social agro-systems recently developed and used as a concept in rural history,¹² but they are extended here, since the book links them with soil conditions and the early medieval occupation, and also with the urban sphere, which the book tries to link with the regions and the rural economy and society they spring from. The focus on these regions, however, does not deny any differences between areas or towns within that region, and the interaction between them. Sometimes this interaction is even characteristic of that region, as in the example of the light clay soils of the stream ridges of the Guelders river area and the heavy clay of the backswamps, which do not constitute two separate regions, but together form one agricultural and socio-economic system.¹³ The same applies to Holland, with the small strips of river clay and *geestland* interspersing the extensive peatlands and forming one region. Petra van Dam rightly notes that the boundaries of this region are not sharply delineated in the book. In some cases, as with the northern boundary of this 'Holland', this is the result of a lack of relevant studies. In other cases, however, the boundary is much sharper and easy to indicate, as between the Guelders river area and the Holland peat area: a sharp line, not even a transition zone. This is indeed the case I know best, but other examples of similarly sharply delineated regions crop up in recent research, and in the future new research, perhaps also within historical geography,¹⁴ will amplify and further sharpen our picture.

However difficult it may sometimes be to define their boundaries exactly, these regions are the most relevant unit of pre-industrial economic and social development. This is the geographical level where differences are most pronounced, up to the nineteenth century. It is, therefore, also the best unit to test explanations of economic and social development, and to falsify them, and to identify the cause of the differences in development. All relevant processes (commercialisation, specialisation, intensification, proto-industrialisation, factory industrialisation) were regional phenomena, not features

12. As by Erik Thoen, Tim Soens and myself, but most systematically elaborated by E. Thoen, "Social agro-systems" as an economic concept to explain regional differences. An essay taking the former county of Flanders as an example (Middle Ages-19th century), in: Bas van Bavel and Peter Hoppenbrouwers (eds.), *Landholding and land transfer in the North Sea Area (late Middle Ages - 19th century)* (Turnhout 2004) 47-66.

13. Van Bavel, *Manors and markets*, 26.

14. Through studies like the magnificent book on Drenthe by Theo Spek, *Het Drentse esdorpenlandschap. Een historisch-geografische studie*, 2 vols. (Utrecht 2004).

of individual towns or localities. Neither was the nation-state, favoured by nineteenth- and twentieth-century historiography, relevant for economic and social development in the pre-industrial era – perhaps not even for the period of the Industrial Revolution.¹⁵ Instead, the distinctiveness of the regions increased over the centuries as a result of growing interaction, because of market integration and increasing inter-regional trade and political unification within bigger empires or states. Growing interaction made the regions more complementary to each other and more distinct.¹⁶ It was only from the fifteenth century, as central states began to exercise a greater influence through central legislation and administration of justice, and urban industries increasingly developed apart from regional social and agricultural structures, that convergent dynamics grew, albeit very slowly and initially matched by divergence. Only in the nineteenth and twentieth centuries did regional convergence gain the upper hand.

Each of the reviewers specialises in a specific region within the Low Countries: Petra van Dam in Holland, Tim Soens in coastal Flanders, Erik Thoen in inland Flanders, Alexis Wilkin in the Meuse Valley, and Peter Stabel and Jord Hanus in the towns of Flanders and Brabant. From this regional perspective each of them may feel that ‘their’ region is misrepresented or underrepresented in the book. Most clearly, the feeling of underrepresentation is justified for the southeastern parts of the Low Countries, as noted by Jean-Pierre Devroey and Alexis Wilkin. This is very unfortunate, in view of the historical significance of some of these regions. Mostly, however, it stems from the absence of any substantial historiography, as acknowledged by the same reviewers. For Alexis Wilkin as a young scholar and for his Walloon colleagues, there is still a lot of scope for progress, especially since some of the most dynamic and interesting regions in the Low Countries are situated here. This applies to the Meuse Valley in the early Middle Ages and the Haspengouw/Hesbaye in the high Middle Ages. The latter region awaits more thorough investigation; the review by Jean-Pierre Devroey and Alexis Wilkin already indicates some avenues. Most inviting, however, is the history of the Meuse Valley in the sixteenth century. John Cockerill did not come out of the blue; it is no coincidence that this was the first region on the European Continent to experience an industrial revolution. This revolution was firmly rooted in the growth spurt in metallurgy, chemical industries and mining in the sixteenth century, accompanied by scale enlargement, technological innovation, big capital investments, accumulation and energy intensification.

15. Pat Hudson, ‘The regional perspective’, in: Id. (ed.), *Regions and industries: A perspective on the industrial revolution in Britain* (Cambridge 1989) 5-38. A similar argument is also made by Pollard, Kiesewetter and many others, although – again – definitions of these regions are not always clear.

16. Van Bavel, *Manors and markets*, 394-396.

In their turn, these developments built on the late medieval emergence of these sectors. As a result of the lack of relevant studies, however, it is unclear to what extent and how exactly the social structure and institutional organisation of exchange in this region, as mainly formed in the Middle Ages, shaped the path of these long-term developments. Hardly any substantial work has been done on this topic in the last decades. If there is one marvellous research opportunity for the new generation of economic and social historians, it is offered by this case. Fresh research into this case would also allow us to further open up the discussion on the causes and origins of the Industrial Revolution and rescue it from its perhaps too exclusive focus on the English case.

If, in the coming years, other regions are as well investigated as inland Flanders, coastal Flanders, the Guelders river area, Holland, and to a somewhat lesser extent Drenthe and the Campine, this will allow for a fuller comparison and will identify the dozen cases that can function as a control group, as requested by Jord Hanus. Since he is the youngest of all the reviewers, and his demands are the biggest, I assume he will bear the heaviest burden in making progress in this field. The new cases will surely not be identical to the ones used; I called them exemplary, or representative of possible developments, because they are typical of the developmental paths we know. Perhaps one of the regions in the southeast of the Low Countries, characterised by fierce competition between feudal powers, will on closer inspection reveal a completely different path, as suggested by Devroey and Wilkin. Other new cases, however, will probably show more nuanced or mixed characteristics, since the ones used throughout the book are not only the best investigated, but perhaps also the most extreme or most pronounced ones.

Each of these developmental paths has its own characteristics; there is no ideal type. Neither is one regional path necessarily more successful than another. Rather, I have tried to show that in the long run, some 'reversal of fortunes' can occur, and this is not directly linked to resources (which often have always been there and do not automatically lead to flourishing), favourable location (*idem*) or fertile soil (*idem*). Neither is the availability of technology or capital a necessary precondition. The latter factor would rather promote the continuation of a region's lead, since the leading region would have ample capital at its disposal. Apparently there is more, and this is something made by people: the way societies are organised in the long run can lead to stagnation and downfall or to growth and blossoming, aside from natural endowments and capital.

The opportunity a society and its institutional organisation give to education and human capital formation is undoubtedly an important element, but in the review by Jord Hanus this is made into a kind of *Deus ex machina*, followed by the development of favourable institutions. In fact, rather the reverse is the case, since investment of time and money in education and skill formation is only feasible within the right institutional framework, that is, the

scope or stimulus offered, for instance, by inheritance laws, marriage rules, guild and other professional regulations or urban by-laws. Exactly how these institutions resulted in better education and human capital formation needs further investigation.¹⁷ Assembled in *Manors and markets* are all little pieces of mainly old studies on education, but in recent years – despite the interest of economic historians in human capital formation, the development of the skill premium or the level of numeracy – the empirical research into how exactly education and skill-formation in the Middle Ages were organised and financed has been virtually absent in recent decades. This is a clear research desideratum.

Then there is the positive or negative picture of the achievements of the Middle Ages. Jord Hanus reproaches me for sketching too gloomy a picture of them. On closer inspection of the book, this gloomy picture would apply only to one aspect: levels of welfare for the ordinary people. I do not deny at all the advances made in the Middle Ages in other fields. The book amply discusses the progress made in technology, population growth, physical output and land productivity and in the qualitative development of the economy and its ever more complex organisation. But, indeed, I did not celebrate these advances. First, I focus more on the differences in these developments between regions and on finding the causes of these differences, instead of describing these developments themselves, since I think this is where the most progress can be made. Second, not because I am less convinced of the progress the Middle Ages made in welfare or living standards (or the rise of markets from the high Middle Ages onwards for that matter), arguably the most crucial criterion for a society's success.

Even if we start the discussion by looking at GDP/capita – where I think the picture is most favourable compared to other indicators of welfare – the progress made in the Middle Ages is much more modest than assumed some years ago. The very low levels of GDP/capita assumed by Angus Maddison for the high medieval period (some \$400 around 1000) and the associated rise in the following centuries have recently been abandoned for much higher estimates (\$800-1000), and therefore a more modest rise in the following centuries.¹⁸ Further progress in this quantitative field is vital, especially if we are to be more successful in placing these abstract figures in their wider social and economic context. Turning to real wages, an indicator closer to the real experience of real people than GDP/capita, the picture is even more mixed.

17. A big step forward is Jan Luiten van Zanden, *The long road to the industrial revolution. The European economy in a global perspective, 1000-1800* (Leiden 2009).

18. A. Maddison, *The world economy, 1-2001*, 249-262 (international dollars of 1990), and for the recent estimates: S. Broadberry, Bas van Leeuwen c.s., 'British economic growth, 1270-1870': <http://www2.warwick.ac.uk/fac/soc/economics/staff/academic/broadberry/wp/britishgdplongrun8a.pdf> (29 April 2011).

Apart from the windfall gains made in the late fourteenth century, many studies show a substantial decline. Jord Hanus advances the newest insights for 's-Hertogenbosch in the sixteenth century, where a fall of more than 50 per cent in real wages for masons and assistant masons in the period 1500-1570 miraculously went without a decline in living standards for broad layers of the urban population.¹⁹ I do not want to deny the value of this recent case study – and certainly do not want to deny the valuable attempt to separate the conditions of the working labourers from the middle-class entrepreneurs and professionals, and to include the latter's income from capital, but would like to place it in the perspective of the incredible evidence amassed in previous studies on the sharp decline in living standards of fully employed labourers in the southern Low Countries, let alone of those who were not fully employed. If we could better reconstruct the social stratification and the various sources of revenue available to various groups, we would be better able to evaluate this issue. This would require us to put long-neglected topics such as social structure and inequality back on the research agenda; something I hope will happen.

At this moment, the evidence at least shows that wage labourers and those only partly employed, probably comprising a third or more of the population in the town and countryside of the Low Countries,²⁰ saw their real wages decline sharply in the sixteenth century. Many of them fell below the poverty line, even when fully employed, and this in regions undergoing rapid economic development, with high GDP/capita and dynamic markets. And these data do not even capture the effects of other, less easily quantifiable aspects of welfare, such as pollution, living conditions and leisure time. Indeed, in the book I have placed considerable weight on the indications offered by human stature, since this is the only quantitative indicator reflecting the quality of diet, living conditions, health, pollution and harshness of labour, that is, the main elements of the quality of life. The paucity of data is a problem, I readily admit, but there is no more available at the moment. What little information there is, however, points to decline over the centuries, reaching its lowest point in the nineteenth century.

Investigation of bones is apparently not a priority for archaeologists, and unfortunately so, because their research would better allow us to identify trends and regional differences, and to place the findings more accurately within their social context. This would allow for a major step forward in

19. B. Blondé and J. Hanus 'Beyond building craftsmen. Economic growth and living standards in the sixteenth-century Low Countries: The case of 's-Hertogenbosch (1500-1560)', *European Review of Economic History* 14:2 (2010) 179-207.

20. Bas van Bavel, 'The transition in the Low Countries. Wage labour as an indicator of the rise of capitalism in the countryside, 14th-17th centuries', *Past & Present Supplement* 2 (2007) 286-303.

reconstructing and interpreting changes in economy and society. Here, again, is a clear research desideratum. What the preceding reviews perhaps most clearly do is to show where the scope for academic progress in our field is and where the main lacunae are found; lacunae the reviewers will hopefully help to fill in themselves in their further careers. This surely also applies to Petra van Dam's call to integrate ecological change better as a factor. This is indeed more urgent than ever before, since it is clear that without this element, no sustainable economic growth or high living standards are possible. Understanding what type of societies and institutional arrangements can prevent ecological degradation and cope best with ecological changes and hazards is therefore of prime importance. Integrating this aspect together with the other desiderata identified, most clearly human skill formation, social inequality and indicators of welfare, will result in a more holistic approach to economic and social development, and a much better understanding of its causes.

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