

## **'I proudly call myself a card-carrying economic historian'**

*Herman Roozenbeek en Peer Vries in gesprek met Jan de Vries\**

**I would like to start with a general question about your position as a historian. Do you regard yourself as an economic historian or do you prefer to be described as a general historian? Or do you not attach any importance to the difference?**

I think the importance attached to that term 'general historian' is different to different people. I proudly call myself a card-carrying economic historian and I do not see any reason to think that to be specialized as an economic historian or a cultural historian or a political historian suggests some grave limitation to the point of being unable to address issues that go beyond the narrow competence of economic history. I think specialization is a good thing and for the same reason that classical economists thought it was a good thing. But they also believed in free trade: there should be discussions and interaction among specialists and to that extent I think being a specialist, in this case an economic historian, is a strength rather than a weakness. Besides, I sometimes have to ask myself what it means to be a 'general historian'.

**When one looks at most recent historiographical surveys, one gets the impression that economic history, and especially what one used to call 'the new economic history', is in decline. Many people prefer not to be called economic historians and to not have their books described as 'economic history'.<sup>1</sup> Do you share this impression, and if so, do you have an explanation for this decline?**

Well, I think there are two issues here. One is the issue of specialization in general and the second is the current interest in or importance of economic history as one of those specializations. So, if we deal with the first more general issue, I myself do not see any tendency among historians to avoid specialization; almost all of my colleagues describe themselves as a political historian, a cultural historian and so on. They do not usually call themselves general historians.

**But what is the reason so few of them call themselves economic historians?**

Well, that is the next issue, which is the position of economic history. I think that it is not the same in every country and that the reluctance to be described an economic

historian in the United States is not as great as you have just described it to be here. That is in part because most economic historians in the United States are economists by training rather than historians and they are addressing a different public to some extent, working with different methods. Perhaps they do not feel this *malaise* as acutely as economic historians working within history. In the United States, there is a definite tendency for historians to not describe themselves as economic historians, but that has, I think, quite specific reasons. One is that these historians do not possess the technical training that the economist-economic historians have and so they have tended to move away from the field as a matter of competition. A second reason is that many of the most interesting - and that is where we are finally getting to the drift of your question - historical works of recent years have a strongly cultural and subjectivist character. Economic history certainly in its quantitative and more or less positivist form seems unable to address the types of questions that most interest historians and the readers of history today.

**You are known as a historian who holds the view that an economic historian needs a training as an economist and who is in favour of an approach in which theories and model-building are important. More important than they are for instance in the work of Jonathan Israel, which is rather descriptive or interpretative. Do you still hold that view?**

I still would hold that view. I think I would describe the appropriate training and background of an economic historian today a little differently than I would have fifteen or twenty years ago, but I continue to believe that, not only in economic history but in all kinds of history, we have to bring to the historical material a tool kit of methods and of theoretical understandings to try and make sense of the past, and that in the absence of that we work with half-formed, half-understood ideas, and unspoken biases, that only make the work of the historian more difficult and less satisfying.

**When discussing economic history of the early modern period, many people regard the serialist approach as the best, while especially Anglo-Saxon economic historians, for example Forster in his 'Achievements of the Annales-school', claim that from an economic point of view the serialist approach is not really very satisfactory.<sup>2</sup> What is your opinion?**

I have some difficulty with the term 'serialist'. I am not quite sure what people mean when they use it, except to say that one is very much interested in the '*longue durée*' and in long time series in history. I often work with time series, but that is not the same thing. I work with quantitative material and quantitative material need not be serial or

have that as its chief characteristic. There are many theoretical or more technical forms of economic history that I am very interested in, that do not have as their sole interest the construction or interpretation of time series. To the extent that quantitative economic history is reduced to a history of time series I think I could agree with Forster and anyone else who would regard that as unsatisfactory, limiting and theoretically a bit questionable. If serial history is part of a larger, quantitative and theoretical history I do not have problems with it.

**Could one deduce from this that you are not very impressed by the work of Fernand Braudel, not for the reasons Jonathan Israel gave, but for an opposite reason: you think it is not 'theoretical' enough?**<sup>3</sup>

Yes. Many years ago I wrote a book review of what were then modern works in economic history in which Braudel, a section of Braudel's *Méditerranée*, which had then not yet been translated into English, appeared along with the work of other Annales-school historians and I criticized it at the time because it seemed to me that, while addressing some of the things that have always fascinated me, it did not bring to the subject matter any well developed theoretical apparatus.<sup>4</sup> As a consequence it tended, despite its pretensions, to be very descriptive, very *ad hoc* in its procedures. Ultimately it is impressionistic. So at the time I defined myself as someone who was trying to wed both the best of the Annales-school and the new economic history, an impossible task perhaps, but that is where I thought I fitted in.

**I suppose your criticism that the work of Braudel and other Annalistes is impressionistic and not really based on economic theory also applies to Wallerstein's books?**

With Wallerstein the situation is a bit different because he approaches the material in a much more structured way. There is a much clearer agenda involved in his work than in the case of Braudel. Impressionism is not the first word that comes to mind in describing Wallerstein, but my criticism of him, which I also made public in a rather lengthy book review shortly after the first volume of his *Modern world-system* appeared, was based on the fact that his model did not seem to hold together.<sup>5</sup> It did not seem to have the content of economic reasoning that could explain the phenomenon that he was purporting to explain. So it seemed to me that, apart from all the historical problems - mainly questions of fact and interpretation of a traditional historical type - the theoretical apparatus he was bringing to bear was simply inadequate to the task and only confused matters.

**Many people who criticize a more theoretical approach in economic history do so because they say modern economic analysis is not suited for analyzing early modern history. Would you agree and would that imply that you think that one needs models that are specifically adapted to the early modern period?<sup>6</sup>**

The unsuitability of modern economic theory to historical work is the reason I am not an economist but an economic historian. I tend to agree with the criticism that the most influential stream in economics, neo-classical thought, is quite explicitly a-historical. However, there are, in the large, rather rich literature of economic theory other theories, some now not as much favoured as neo-classical theory, that do have a specific historical content. I think the social sciences in general, not only economics but sociology and political science as well, need a greater historical grounding in their theoretical development. So, to me, being a social-scientific historian, is not simply to say: 'I am such an admirer of social science theory that I want to apply these wonderful theories to history'. It is rather that I am interested in the enterprise of social theory. I see inadequacies in those theories, in part because of an absence of a satisfactory historical grounding, and I want to see what can be done about that. Now aspiring to be a social theorist on one's own, to develop new theories that are historically grounded, is obviously a great ambition. I am not sure that I am capable of contributing much in that direction, but one goes step by step. What one can do in a more limited way and what I have tried to do, is to use those theories that seem to have a historical applicability and that do not seem to hopelessly distort our understanding of the past but can in some way enrich it. I want to show the possibilities of those theories, but also their limitations.

Now, neo-classical theory, despite its a-historical character, can have limited applications in understanding particular problems that people in the past faced. They faced problems of scarcity and choice just as in a text-book example of modern micro-economics the owner of a firm or a consumer faces choices and constraints. Clarifying them, seeing what the trade-offs are, and examining human behaviour in this rigid framework can, I think, have rich rewards. In my book on the trekschuiten, *Barges and capitalism*, I used some rather elementary theory of the firm, that I had learned when I was a student, to try and understand the pricing policy, the tariffs that were established for passenger transportation.<sup>7</sup> These were established by the cities and the barge-operators had to live by the tariffs that were officially set. They sometimes complained that they were too high or too low and that they needed to be changed and so they would write letters petitioning for change, usually an increase, to the city. If I had not studied as an economist I suppose I could have simply interpreted these letters as some kind of a power-struggle or a political issue between these two parties. But then it would have been hard to judge who was right or wrong and to understand what were the consequences of the policy set and what would have been the consequences of the

alternatives. That is, full understanding of the issue, in my view, could not have been achieved.

The objection that these barge-men in the seventeenth century themselves did not know about cost curves, marginal pricing, theories of monopolistic pricing and all the other issues that could have been raised, is, it seems to me, not relevant. The fact that they did not express themselves in these terms does not mean that they did not face the objective issues that are described by these theories. And so in this sense it seemed to me perfectly reasonable and not anachronistic to apply this kind of model, even though it is not itself a historical model and even though the people at the time perhaps did not have an understanding of the issues as fully developed as they are in modern economic literature.

**You already mentioned the word 'politics', which introduces another theme in the discussion with Jonathan Israel, the theme of the relationship between economics and politics. It is perhaps possible to separate politics from economics on an analytical level. But do you think the historian could also sharply distinguish between these fields in practice?**

No one could deny that this is a problem. A common complaint about economic theory, particularly neo-classical economic theory, is that it cannot account for power as an independent factor. In economic models the various interactions seem to be governed by scarcities, by supply and demand factors. The power of the economic actor is, as it were, to the extent that it exists at all, simply subsumed in the context of the economic dimensions of the issue. To the extent that these criticisms are correct, it is a great and constant danger for historians to lose sight of the obvious: the relations among individuals in society have other dimensions besides the strictly economic. Economic issues can be overridden by the exercise of political power. To some extent the role of the political historian is to admonish the economic historian with: 'You do not fully understand the issue. You have not taken the political factors into account'. This kind of challenge is sometimes correct and calls to our attention issues that the economic historian stands in danger of ignoring.

So far, I accept that kind of criticism. I see it as a danger for my specialization and believe that we occasionally stand in need of correction. The other side of the coin, however, is this: Political power and political events - as treated by the what I will call now not the general, but the political historian - are often treated without any reference to the economy while they are often explainable, or partially explainable, by underlying economic factors. The options open, in terms of the exercise of political power, are generally severely constrained. That is, just because you want to do something, it does not mean that you are able to do it. And the inability to do something often has to do

with one's economic position. To say that France closes her markets, and that this is a political fact that the economic historian concerned with numbers somehow just loses sight of, is by itself not a sufficient statement. Why did France close her markets then, and not before? And why was she successful in doing so then and not before? Why does she change her policy again some time later on? There are, not always, but often, economic factors that account for this.

If economic history by ignoring politics can give a sterile and misleading account of history, political history in which political events are pulled like a rabbit out of a hat to announce that now everything has changed, a new phase has been entered and so on and so forth, is not satisfactory either. In a way it suffers from the same problems. If we are trying to explain economic phenomena, it is to my mind desirable to try and make endogenous to the economic account those political issues that obviously intervene and help shape the course of economic history. I do not mean to say by this that every political issue is reducible to its economic components. But they are in many cases to some extent grounded on or shaped by those factors. It is the task of any historian, and here is where the issue of general history might come in, to bring them together and not to treat the one as somehow sovereign or privileged in the account of historical change.

**One of the key factors behind the transformation of the Dutch rural economy was the fact that economic development in the Dutch Republic, or to be more precise Holland, was not 'hindered' by a feudal structure.<sup>8</sup> That is, in effect, a political 'fact'. Does that mean that for this question politics were, in some respect, fundamental in explaining economic development?**

Yes, if you say that this absence of a well-developed feudal system in Holland must be accepted as a political fact. If one were writing an economic history from, say, 1500 onwards, inevitably one would have to accept this absence of feudalism as a political fact and start out from there. But anyone who is looking at the history of medieval society would have to ask himself why feudalism was not so important in Holland. Then it ceases to be a political fact and becomes a problem that requires explanation. And there might be many factors, including political factors, that contribute to such an explanation. But, there will also be economic ones.

Slicher van Bath wrote an article that I have always found quite intriguing, called 'Boervrijheid'.<sup>9</sup> In it he pointed out that farmers, or peasants, were free of feudal controls, not always entirely, but to a large extent, not only in Holland, but in several other parts of Europe: in the Alps, in parts of Sweden, in northern England. He had a whole list of regions. And then he asked himself: 'What do all these areas have in common?'. Some were in mountains, others in marches. They were not contiguous but

spread out all over Europe. So, clearly, he had a bit of a challenge to explain it. Ultimately, he identified the one thing these areas all had in common as, what we would today call, the occupation of ecological niches, in which dairying, or live-stock raising was the dominant form of subsistence for the population, rather than settled arable agriculture. He went on to try and draw a link between the economic basis of these communities and the political forms that controlled or failed to control them. I do not mean to argue here that his argument is persuasive in every way and that this is the only explanation. But it is an example of a systematic comparative history trying to find the links between what, on the face of it, are unlike, different phenomena. He tries to find certain underlying patterns. I believe that is an important task of the historian.

**When one talks about economics and politics, one of the main themes in the history of early modern Europe is the emergence of the mercantilist state. Do you regard the Dutch Republic as, in some ways, a mercantilist state?**

I do not often use the term mercantilism. Not only in application to the Dutch Republic, but also more generally in discussing European history, perhaps because the -ism at the end suggests something much more structured and well-defined both in our eyes and in the eyes of those who practised mercantilism than I think it really was. Still, the term has its uses and I think it is fair to say that the economic thinking of the seventeenth and early eighteenth century that we usually describe as mercantilism had a clear influence on policy makers in the Dutch Republic. There were many occasions when the same kind of 'mercantilist' policy was discussed and even implemented in the Dutch Republic as we see elsewhere. In this sense, the Republic was part of its time and influenced by its environment and not something ideologically opposed to the world around it. However, there were clear limits, in comparison with other countries, to the implementation of these policies in the Republic. They usually were not carried so far, they were not so drastic, and they were not so successful.

That raises the question of what it was about the Dutch Republic that made mercantilism not fully at home there. I think, to the extent that it was not fully at home in the Republic, it was not because its leaders nurtured some alternative vision and were ideologically opposed to mercantilism. I think they faced objective situations which made it difficult to pursue the economic advance or strength of their society via the same kind of measures that many other European states attempted to implement.

**We will now embark on a different subject, the rise and fall of the Dutch economy. Let us start with your position as a historian, once again. I do not think you would like to be described as a 'trade historian', whatever that may mean. You have written about agriculture, about transport, about urbanization, you published a**

**general survey of the European economy between 1600 and 1750. Is this by accident, purely a matter of personal preference, or is it because you think that these subjects are more important than trade, which has always aroused so much interest in other historians?**

First I should say that, although you are correct that I would not call myself a specialist in the history of foreign trade, in my current project, which is to write a general economic history of the Dutch Republic, together with Prof. Van der Woude, it became my task to write the chapter on trade.<sup>10</sup> I became fascinated by this subject and wrote a chapter that was much too long for our book and will have to appear, or at least part of it, in some other form. So I have spent a lot of time recently on the subject and I find it enormously interesting. However, you are right in that I came very late to spending a lot of time in doing research on this subject myself. That has to do not so much with a feeling that trade is unimportant, but with a feeling that it has to be placed in a context.

What is interesting to me about trade is not the flows of goods across the seas and the rich merchants who finance it and draw their profit from it, but the domestic economies that give rise to these trade-flows both through their own production and export and through their needs for and ability to acquire goods from abroad. The pay-off of trade history is to be found in some kind of territorially based, economy-based history. It seems to me it was precisely in this area that the economic history of the Dutch Republic had so many gaps and was most in need of development. The trade history seemed to float unconnected to the domestic economic history of the country. What I wanted to know was how trade changed society and how society gave rise to trade. I did not want to treat trade as some kind of disembodied essence that floats around the world and sometimes settles on one place and sometimes on another. It seemed to me that to understand the totality of the economy, we needed to know more about agriculture, but also, about industry, domestic distribution, urbanization and demography. All these relatively domestic issues needed to be fleshed out further so that we could understand the totality of the economic history of the Republic.

**When trade has been placed in its context, would you agree with Jonathan Israel that the rich trades were more important, in whatever way one should measure their importance, than the bulk trades, or do you still think that the bulk trades were more important?**

I would prefer not to answer this question in this form, because of the dichotomy 'rich trade' - 'bulk trade' that I find not to be a useful distinction. As I mentioned in my talk last week, the specific character of the substance traded is by itself not of critical importance. What is important, as I mentioned then, is the profitability of trade. That



is terribly important for any dynamism in trade history. Trade is not going to grow if people cannot make a profit from conducting it. Secondly what is important is innovations in markets. Trade is intimately connected with markets. If there is an integration of distant markets and a reduction of the costs of trade, of what are called the transaction costs that are associated with the movement of goods from one place to another and with their distribution, this can have tremendous consequences for the volume of trade and for the introduction of new goods in distant markets. These things have to do with economic change and development and they interest me much more than the lucky event that a trade in some precious commodity, which had been located in Antwerp or in Lübeck, now happens to have landed in the pockets of a merchant in Amsterdam. That fact in isolation does not strike me as a terribly interesting one. Placed in some broader context, it might end up being important.

By itself, as the chief aim of one's research, I am not sure why we should be terribly interested in that. Unless one's chief interest here is not so much economic history, but the history of certain types of people. If you say, 'I am really interested in the élite merchant families of Europe and I wonder how it is that they all gathered in Amsterdam for a time before moving on', and then you find that 'This seemed to be the best place to carry on business in silk, diamonds and furs' well then you have explained why a European merchant élite moved to, or sent some of its members to, Amsterdam. But if you are concerned with the dynamics of economic development of a society, then that might be a useful fact or it might not, and we need to know more. That cannot be the end of the story.

What is interesting and important about the fact that these traders are in Amsterdam and that they control certain commodity-flows is not whether these are rich or bulk goods, it has to do with the profits of those goods, with the innovations of those merchants in developing those trades, and ultimately with the connection between those trade-flows and domestic production and consumption. Once you have made all those connections, then trade history, to my view, becomes interesting and important. But the very terminology, rich trade - bulk trades, élite merchant - other merchants, does not seem to me designed to lead us into those interesting forms of investigations. It seems to invite to stop short of that, to stand back and be satisfied with the knowledge that rich and powerful figures have suddenly appeared.

**I suppose what you think is interesting in the economic history of the Dutch Republic is what one has called the 'Dutch miracle', the old question of the rise, and the decline, of a small economy in the northwestern part of Europe. The last part of our interview is concerned with questions related to this problem: How could one explain the rise of the Dutch economy, which factors were important in it? and secondly: How could one explain its decline, or its relative decline? Was**

**it more or less ‘inevitable’ for such a small state as the Dutch Republic to loose its primacy once the other, bigger states, France and England, were ready to compete with the Dutch or were there ‘internal’ reasons for it?**

You use the word primacy. That complicates any answer that I can give. Last monday we did not much discuss the use of the term ‘primacy’. If we take the simple-minded definition of being a single dominant element in European trade, more important than any other single nation, then I think it can be said that the Dutch Republic held that position in most trades and that it was inevitable that it would loose it. But, all kinds of things, you might say, are inevitable. Herodotus said that good fortune never seems to stay long in one place. It always moves. Then there are two subsidiary questions that have to be asked, which are: If it was inevitable that it was to loose primacy, was it also inevitable that it should loose prosperity? And, secondly: If it was inevitable that it was to loose primacy, when did it happen, and what was the occasion for it to happen? That is, when something is inevitable but will not happen for two hundred years it is a lot different than it being inevitable and it could happen any minute now. So the question of the timing of this loss of primacy and the fact that the Dutch lost a lot more than primacy makes the question, I think, historically more interesting.

**Should we not begin by trying to explain what it was that gave the Dutch, I do not think one should say primacy only in trade, but such a flourishing economy, before discussing its decline? In your book on Dutch agriculture, you stress the importance of specialization in agriculture. That is one of the basic pillars. The other pillars one can find in your work are the existence of a highly developed and technologically advanced industry, the Dutch trade-system and the availability of peat as a cheap source of energy.<sup>11</sup>**

Those are what I would regard as important factors. They are all domestic factors that in my view help explain why the foreign trade of the Dutch Republic became something other than, and more than, the foreign trade of a Lübeck, or for that matter of an Antwerp, in the sixteenth century. Which gets us back to the matter we were discussing before. When trade-flows are connected to a productive economy, when they foster economic specialization and increase the productivity of the population, they become a more interesting and more lasting phenomenon than when this is not the case. That is why I have always been interested in examining the domestic economy rather than remaining fascinated by the flows of trade, and not penetrating beyond the port cities, or even the harbours of the port cities, to what lay behind them.

But there is another matter that is important and I try to stress this in my book on European urbanization.<sup>12</sup> To put it more simply than I would like to: at the time of the

Dutch Revolt there was an opportunity for the diversion of certain kinds of trade from their established routines. Political history plays an important part in this. When Professor Israel or anybody else says that this is not simply a matter of economic history, I agree. And since I am not capable of doing full justice to the political side of the story, I certainly do not have any objection to someone else doing so, fleshing it out and thereby making this whole history richer and better.

The multi-centred urban system of the sixteenth century, which was to a very large extent related to the political structure of the Habsburg Empire and its rivals, faced a crisis: a political crisis, but also, in certain regions of Europe, an economic crisis. This crisis undermined the strength of long-established cities like Antwerp, Augsburg and Genoa, that had exercised economic leadership both in the Mediterranean and in Northern Europe. In the context of that crisis the revolt of the Dutch against Spain took place. And without trying to present a narrative of this history I simply want to say that the coming together in time of an independent Dutch Republic, with its established, albeit relatively modest, trade and this 'decentring' as Braudel called it, this 'instability of the multi-centred urban system of Europe' as I have called it, created possibilities for Holland and Zeeland that they would not have had at other times.

This is an historically unique situation. This unique opportunity did not necessarily have to fall into the lap of the Dutch Republic, there were rivals. If we talk about élite-merchants and rich trades, one might say that given these unique opportunities all these merchants landed in Amsterdam and once they were there with their contacts around the world and their large amounts of capital, they turned Amsterdam into something it had never been before. This seems to say that wherever such merchants land, they create, as though by their simple presence, a new economic power. To me the sudden emergence of so many new economic initiatives in the Dutch Republic, and especially in Amsterdam, from the 1590s on has to be understood in the larger context of rivalry and its 'window of opportunity'.

The Dutch Republic had advantages that were based on some of the characteristics its economy had acquired in the past. Here the so-called bulk trades and the commercial tradition with its shipbuilding and its regular market-activity rather than the use of periodic fairs, imparted on the new, now much more expanded trade that occurred after 1590, a character that it had never had before in one located in a town like Lübeck, Antwerp or even Venice. It is this economic, but to some extent also political and institutional, structure of the new Republic that gave it advantages in the competition during this unsettled period.

To the extent that we want to speak of a 'Dutch miracle', it was the coming together of these things, this specific conjunction, that was, I would not want to say 'miraculous', because I think it can be explained, but at least unique. Now, something that is unique cannot be explained theoretically very easily. You cannot find comparable elements

in the story. One can use theory to deal with certain aspects of this problem, but when one integrates all the different things happening at the same time into some kind of totality, as an historian must, one ends up with an irreducible element of creativity. Theories and model-building ultimately at some point fail us. They can be used to structure the argument, set the terrain and say how things are related to each other, but I think we can not quite use a structured, model-building theoretical approach to show how all these things came together.

**Could one say, to put it rather bluntly, that the explanation for the decline of the Dutch Republic - be it absolute or relative - can be given by just reversing all the factors that are normally mentioned in explaining its rise: a fast growth of the population giving way to demographic stagnation and even decline: a specialization in agriculture becoming a hindrance to further industrial development because it absorbs the peasants in the countryside and thereby deprives industry of its supply of cheap labour: an industrial supremacy based on technological superiority and competitive wages coming to an end because of technological stagnation and high wage-rates: a supremacy in international trade disappearing because fundamental changes in international political and economic relations undermine the Dutch position as the carriers of the world? Did not in time all the factors that promoted Dutch economic growth in one way or another become obstacles to its further development?**

If you want to suggest that there is something inherent in the process of growth that gives rise to its opposite, I would like to place some question-marks after such a statement. To a certain extent it is true. But that is a bit like saying: 'What goes up, must come down', as I did in answering your earlier question.

The problem as I see it, is that no economy that is growing keeps on growing on the same basis forever. But that does not mean that once basis A has ceased to be a real growth-stimulus, the economy ceases to grow. What often happens, is that there are new stimuli to growth that succeed A, and that in fact the very earlier growth of A makes possible its own destruction, but also the creation of growth-element B, and so on and so forth. This does not mean the growth necessarily continues forever, but it can go on for quite a long time. Normally economists believe that to have developed a system of, say, railways puts an economy in the position of developing technologies and advancing market-integration that will make it easier to adopt automobiles and airplanes and that an economy without railroads will find it more difficult to do so. To take us back to the seventeenth century we might ask: 'Why are the growth-stimuli of the first half of this century, as they became exhausted, in so few instances replaced by new ones?', 'Why is this transition not made?'

To my view one of the limitations of Dutch economic history in discussing the later 'stagnation and decline phase' is that it is almost always cast in terms of: 'Why could the old growth-sectors not have continued on?' or: 'How long did the old growth-sectors last?'. 'Did they really begin to decline in 1670, or did they remain important to 1730 or even 1780?'. It is as though the economic history of the whole Dutch Republic, from its political beginning to its political end should be seen as the rise of a certain complex of factors in the first half of the seventeenth century and what happened to those same factors through time. There is hardly any room in this account for the discussion of change and innovation after 1650. It boils down to a well-rounded story of a sudden creation of a certain complex of economic factors and then the fate of those factors as time goes on.

This economic 'rise and decline' seems to fit with the political history of the rise and decline of the state. This also implies that the economic history of the nineteenth century has to be seen as something new because then the state is something new. I think that we have here an example of the undesirable influence that political history - via its dominant role in establishing periodization - often has on our ability to understand that certain important economic or social characteristics transcend political breakpoints and that economic history does not always have the same periodization as political history.

It is worth considering two facts. First that although there are important new developments in the Dutch economy having to do with its achieving primacy in the decades after the revolt, there is a strong degree of continuity. This primacy was built upon its earlier achieved commercial, industrial and agricultural abilities in the first two-thirds of the seventeenth century and in fact going back to fifteenth century. The more information we get about the Late Middle Ages, the more we see that there is a long continuity. But this system is no longer capable of developing itself any further after, roughly, the middle of the seventeenth century. Then, and this is the second fact worth considering, there is a real breakpoint and we see new initiatives and important changes in the structure of the Dutch economy. It is to a very large extent a new departure. It ends up by not being a very successful one, but it is a new departure nevertheless. When one only regards it as the lingering on of an old system, one misses the essence of the late seventeenth-century and early eighteenth-century Dutch economy. To the extent that Dutch economic history has been constrained by a political periodization we have not been able to see clearly some of the most important changes in it.

**But whatever periodization one chooses I think one cannot avoid the question why the Dutch, having the most advanced economy in the seventeenth century, had no 'industrial revolution' in the eighteenth century, while the English did.**

No one would deny there was no industrial revolution in the late eighteenth century in the Netherlands. But the attempt to explain this fact becomes a little different if one uses the periodization I suggested. One then does not quite so quickly simply observe that already long before the beginnings of an industrial revolution in England economic growth in the Dutch Republic seems to have petered out and that a conservative attitude of holding on to an old system which obviously would preclude the initiatives of modern industrialization would somehow account for this 'missed opportunity'. Then, to my mind, it becomes a little more interesting. If the Dutch are building colonial trade, trying to develop a West-Indies trade and are re-orienting their trade both to more distant and more nearby markets, just as the English were doing in the eighteenth century, then suddenly Dutch economic history looks a lot different in relationship to industrialization than it did before. But I should not pursue this point much further, it is not enough developed in my mind.

The main reason I raise it is that the death of any kind of history, including economic history, is to be forever wedded to the same old questions. If they are not the right questions the whole literature becomes sterile. Therefore it is important that we should have books like Jonathan Israel's. They raise issues people thought had been settled long ago and shake us up a bit to look at things again. That is terribly important. However, I think that if we do that without a theoretical apparatus the discussion cannot be extended very far. It then ends up in absolute statements and visions. 'My vision of history is this': I hate that statement. It precludes discussion. You cannot take it apart and improve on it.

**I think in his latest book Van Zanden throws some new light on the 'rise and decline issue' by stressing the importance of the availability of cheap labour in the rise of the Dutch economy and then relating its disappearance to the specialization that took place in Dutch agriculture.<sup>13</sup> In his opinion the Dutch rural economy of the sixteenth century (at least till around 1580) was still characterized by a system of 'proto-industrialization'. In the process of specialization that he thinks began at the end of the sixteenth century the undifferentiated peasantry was absorbed into specialized activities in the countryside or else moved into the towns. In this interpretation of economic developments during the sixteenth and seventeenth centuries specialization became an obstacle to the formation of a Dutch industrial capitalism because it precluded the further development of a system of 'proto-industrialization' which according to him is the only system 'gekenmerkt door een eigen dynamiek die kon leiden tot de overgang naar een "moderne industrie"'.**<sup>14</sup>

Well, I do not agree with that. This is a complicated issue. A great deal of what he says

in his description of the Dutch economy in the sixteenth and early seventeenth century I agree with. I think he has added to our understanding of that period. But he loads far too much explanatory weight on the process you just mentioned and I think he describes it in a too one-sided way. In a nutshell I guess the basic problem is this: I describe sixteenth-century rural development as one of 'specialization', while he describes it as one of 'proletarianization'. To a certain extent they are the same thing. My specialists, at least some of them, were commercial farmers, they owned or rented their land and owned their cattle and equipment. No one would call them proletarians. But those who were not farmers increasingly were divorced from the land and found employment in a whole variety of rural crafts and services, not in proto-industry in the sense we usually use the word, but, to use a modern term, one might say that they developed a specialized service-sector for commercial agriculture and for certain industries and trades that were carried on in the rural economy.

I have always preferred to emphasize the specialized character of this because to a large extent this rural, non-farming population was not simply day-labourers working on farms, that is, they were not simply an agricultural proletariat working on estates or on the large farms of others which I think Van Zanden tries to suggest a large part of them were. Rather they were predominantly craftsmen or else working in the transportation sector, in food-processing, or in other sectors related to food-production. They worked for wages or piece-rates, or they were 'zelfstandigen', independent operatives. No one would claim they were necessarily rich. But in my view this specialization gave them the means to live in a way that was better than the desperate poverty of the very small peasant with not enough land and without the ability to invest in improving the productivity of his land and capital-stock.

So by stressing specialization rather than proletarianization I was trying to suggest that this differentiated occupational situation in rural Holland was a kind of success-story, that it indicated a measure of economic development. Well, by stressing proletarianization Van Zanden wanted more to speak of the exposure to market forces and the vulnerable position of people having no longer direct access to the means of production, i.e. land.

He regards this as very important because of a second factor, which again he gives more importance than I think sensible, that is self-provisionment, especially the self-provisionment of food, in a pre-industrial economy. An economic word for it is 'dualism': on the one hand a household is engaged in production for the market, its members work for wages and enter the market with their goods, while on the other hand it produces at least a portion of its food on its own farm, so that for these goods it is not dependent on the market.

I think this dualism is not unique to a pre-industrial or even a pre-capitalist economy. Families in modern society ordinarily have a kind of dualism too. There is

self-provisionment in the home of all kinds of goods and services that one might buy on the market. Instead people organize their households in such a way that they are able to produce a certain amount for themselves by, for example, preparing food and taking care of the children. But Van Zanden in his analysis pushes this issue very far. Once people no longer have access to their own means of food production - to some extent, he is not very clear on when exactly that point is reached - this dualism breaks down and they are utterly dependent on the market. Then in his view, given the conditions of that time, they are no longer capable of reproducing themselves. He thinks their employers can never pay them enough to permit them to acquire the food, clothing or housing they could provide more cheaply when they still had direct access to the land.

So to the extent that specialization, or as Van Zanden would say 'proletarianization', continues, it knocks down the pillars of future economic development. Once it has proceeded far enough, labour has become too expensive to be competitive. It then cannot reproduce itself whereby demographic growth, which can also be an important source of economic dynamism, weakens.

Well there are all kinds of questions one might ask about this model, but let me be very short and say why I prefer to speak of 'specialization'. I believe that the process of specialization, through its efficiency- and productivity-increasing effects, could permit production to take place on a competitive base and could provide a standard of living for wage-earners that was above subsistence.

## Noten

- \* Het interview met Professor De Vries, die thans als hoogleraar is verbonden aan de University of California Berkeley, werd afgenomen op 19 februari 1992 in het gebouw van het NIAS te Wassenaar. Voor publikaties van De Vries zie de literatuurwijzer.
- 1. Dat het met de beoefening van de economische geschiedenis niet zo goed gaat, zou bijvoorbeeld kunnen worden afgeleid uit het feit dat in overzichtswerken als P. Burke, ed., *New perspectives on historical writing* (Cambridge 1991) en H. Beliën en G.J. van Setten, ed., *Geschiedschrijving in de twintigste eeuw* (Amsterdam 1991) in het geheel geen aandacht wordt geschonken aan recente ontwikkelingen op dit vakgebied. Dat diverse beoefenaren van de economische geschiedenis zelf ook niet erg optimistisch zijn gestemd, blijkt bijvoorbeeld in de volgende publikaties: J.L. van Zanden, 'De toekomst van het vak', *NEHA-Bulletin* 2 (1988) 7-12 (en de daar genoemde literatuur); P.W. Klein, 'Eigentijdse terugblik', *NRC-Handelsblad* 30-1-1991 Bijlage 10 jaar W&O, 13 en D. Coleman, *History and the economic past. An account of the rise and decline of economic history in England* (Oxford en New York 1987).
- 2. R. Forster, 'The achievements of the Annales-school', *Journal of Economic History* 38 (1978) 58-75.



3. Voor de kritiek van Israel op Braudel zie het interview met Israel in dit nummer en de publikaties genoemd onder de eerste noot bij dat interview.
4. J. de Vries, 'The classics in translation', *Reviews in European History* 1 (1975) 468-473. Zie ook idem, 'Spotlight on capitalism', *Comparative Studies in Society and History* 21 (1979) 139-143.
5. J. de Vries, 'Het wereldmodel van Wallerstein', *Theoretische Geschiedenis* 3 (1976) 105-122. Bespreking van I.M. Wallerstein, *The modern world-system. Capitalist agriculture and the origins of the European world-economy in the sixteenth century* (New York en Londen 1974).
6. Voor een economische analyse die een uitgewerkte kritiek op de a-historische aard van de economische wetenschap, en met name de neo-klassieke economie, behelst, zie bijvoorbeeld M. Finley, *The Ancient economy* (2e druk: Londen 1985) en vooral W. Kula, *Théorie économique du système féodal; pour un modèle de l'économie polonaise, 16e-18e siècle* (Parijs 1970). Van dit oorspronkelijk in 1962 in het Pools verschenen boek bestaat ook een Engelse versie: idem, *An economic theory of the feudal system. Towards a model of the Polish economy 1500-1800* (Londen 1972). Ook in het in noot 13 genoemde boek van Van Zanden wordt gepoogd een economisch model te construeren dat meer met specifieke historische omstandigheden rekening houdt dan de gangbare neo-klassieke modellen.
7. J. de Vries, *Barges and capitalism* (Utrecht 1981).
8. Zie bijvoorbeeld J. de Vries, 'On the modernity of the Dutch Republic', *Journal of Economic History* 32 (1973) 191-202.
9. B.H. Slicher van Bath, *Boerenvrijheid* (Groningen 1948). Deze tekst is ook opgenomen in: *Economisch-historische herdrukken, zeven studiën van Nederlanders* ('s-Gravenhage 1964) 272-294.
10. De (voorlopige) titel van dit boek dat in 1992/1993 op de markt zal verschijnen is: *A new economic history of the Dutch Republic, 1500-1815*.
11. J. de Vries, *The Dutch rural economy in the Golden Age, 1500-1700* (New Haven en Londen 1974) passim en idem, *The economy of Europe in an age of crisis, 1600-1750* (Cambridge 1976) passim.
12. J. de Vries, *European urbanization, 1500-1800* (Londen 1984).
13. J.L. van Zanden, *Arbeid tijdens het handelskapitalisme. Opkomst en neergang van de Hollandse economie 1350-1850* (Bergen 1991). Zie voor een uitvoerige analyse van dit boek de bespreking ervan door Van der Wee in dit nummer p. 79 e.v..
14. Van Zanden, *Arbeid tijdens het handelskapitalisme*, 28.