

The 'New History' versus 'traditional history' in interpreting Dutch world trade primacy

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Reconsidering a sizeable work completed several years before, any author (and certainly this one) is apt to wish that he had been more precise here, supplied more information there, and so forth. It is natural also to think again about the validity of the concepts and interpretations employed. I should be less than truthful were I to pretend that I was then, or am now, absolutely sure of my ground at every step in my argumentation. This process of reconsidering is of some significance since it is likely to have a bearing both on the subsequent development of the author's own work and on that of those who react to it. Consequently, I am grateful for the opportunity to enter into dialogue with two such eminent economic historians as J. L. van Zanden and Leo Noordegraaf and, as it were, be helped by them in reconsidering. Both critics confer some praise, as well as a good deal of criticism, and I, in turn, would like to commence by affirming my regard for the breadth of their researches and the expertise they have shown in many relevant areas of study. Although I have fastened on what seem to me to be the main points of their criticism — and rejected them, at times rather forcefully, it is by no means my intention to suggest that their critical remarks are entirely lacking in force and validity. On the contrary, I readily accept that I have often failed to make my points in *Dutch primacy in world trade* as fully and clearly as I ought to have done, that I failed to use a number of relevant books and articles, and that my statistical tables reveal several (albeit mostly fairly minor) slips and discrepancies of presentation¹.

At the heart of Van Zanden's criticism is his attempt to demolish my analysis of the development of the Dutch overseas trading system during the period 1621-1647 what I call phase III, a period which he sees as 'mischien wel de klassieke jaren van bloei van de Hollandse stapelmarkt'. For Van Zanden, as for Braudelians and serialists generally, this quarter of a century is the culmination of a long process of material expansion, the most glowing and expansive phase of Dutch prosperity during the Golden Age. To interpret it as I do as a period (at any rate in the sphere of Dutch European commerce) of severe depression lasting more than a decade, followed from around 1633 by a slow, halting recovery thus as a period in which there was very little net growth, is to clash head-on with (and threaten) one of the most cherished doctrines of the New History. It is not surprising that Van Zanden gives high priority (and considerable space) to his attack and presses it hard, describing my discussion of the era 1621-1647 as *het dieptepunt* of the book.

To demonstrate that this was the 'classic' era of growth for Dutch trade and shipping (as well as for the Dutch economy generally), Van Zanden cites the *convooien* and *licenten*, or customs, returns for the years 1621/1647, claiming that the figures show that these rose by 52% in the case of the area under the jurisdiction of the Amsterdam admiralty college, 64% for that of Rotterdam, and 90% for that of Zeeland. At first glance this appears convincing enough and might seem to underpin Van Zanden's reference to the *wankele historisch-statistische basis* of my study. Yet, when one looks more closely it is evident that Van Zanden is profoundly wrong here. Moreover, the several errors in his argument are, as I see it, so typical of the inadequacies of the serialist approach to history generally, and the distortion to which I believe it is inherently prone, that

1 Some of these faults I hope to have corrected in the revised and slightly expanded Dutch version of the book, Jonathan I. Israel, *Nederland als centrum van de wereldhandel, 1585-1740* (Franeker, 1991).

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I shall high-light the mistakes he makes in attempting to criticize my argumentation to illustrate once again the basic weaknesses which, I contend, do so much to weaken and damage our overall grasp of history.

While superficially cogent, Van Zanden's argument here rests on several different fallacies and misrepresentations. In the first place, I am certainly not talking about the general movement of the economy, or Dutch trade as a whole in my book. Serialists may insist on emphasising the overall trend. But my argument is that one masks and overlooks what is really happening in an mercantilist age if one concentrates on overall trends. It is a central part of my thesis that whilst the period 1621-1647 was one first of slump and then of continuing difficulty and relative stagnation for Dutch European trade, it was one of accelerating expansion for Dutch colonial trade and also for home industry (and the Zeeland transit trade to the Spanish Netherlands)². Since this expansion was reflected in the *convoaien* and *licenten* returns allowance has to be made for this which Van Zanden pointedly does not do. Then, as I go to some pains to explain, Zeeland was in a very different situation from Holland at that time because the high war-time inland tariffs and the Dutch naval blockade of the Flemish coast (not letting Dutch ships through to the Flemish sea-ports) artificially but very dramatically, boosted Zeeland's transit traffic via the Scheldt estuary to the Spanish Netherlands³. Thus the 90% increase for Zeeland is not just totally irrelevant to any test of the validity of the arguments in my book but directly supports them. Then Van Zanden is at fault in including 1647 in his calculation because a significant part of the increases, which he claims, occurred in that very year when the Spanish embargoes were lifted, when there was a sudden but massive expansion in Dutch European trade, and when the new phase in Dutch trade expansion, phase TV (1647-1672) began⁴. Each of these errors weakens Van Zanden's case. But the core fallacy which undermines it completely is this: his reasoning makes no allowance for the drastic drop in levels of trade in 1621, an abrupt downturn which I have written about in numerous of my publications and which is confirmed by the customs returns themselves, as well as much other evidence. In the case of the Amsterdam admiralty jurisdiction the *convoaien* and *licenten* returns for 1620 were 777,380 *ponden*. In 1621, from April onwards, *convoaien* and *licenten* were collected at an increased rate under the new war-list. Yet returns were 811,309 *ponden* only marginally more than in 1620. Even if we are generous to Van Zanden and follow Westermann in accepting 30% as the level of the inflating effect of the new customs list, it is clear that most of the alleged 52% overall increase at Amsterdam between 1621 and 1647 which remains (after my previous qualifications have been taken into account) does no more than reflect a slow recovery back towards the levels of trade pertaining in 1620. However, in reality, as I explain in the book, the real corrective should be higher than Westermann's 30%, in my opinion more in the region of 60%⁵. This means that there can not have been any significant increase in levels of Dutch European trade as reflected in the *convoaien* and *licenten* returns at Amsterdam or Rotterdam between 1620 and 1646. Van Zanden cites the Amsterdam, Rotterdam and Zeeland returns because he thinks they support his

2 Jonathan I. Israel, *Dutch primacy in world trade, 1585-1740* (Oxford, 1989) 156-165, 171-187, 190-196.

3 *Ibidem*, 113,156; see further on Zeeland's gains during the 1621-1647 period, Jonathan I. Israel, *The Dutch Republic and the Hispanic world, 1606-1661* (Oxford, 1982) 87-89, 235, 384.

4 My reading of Becht's figures diverges strikingly from Van Zanden's. As far as I can see, the increase in the Rotterdam returns between 1621 and 1646, far from the 64.5% claimed by Van Zanden, was in fact well under 50%, see H. E. Becht, *Statistische gegevens betreffende den handelsomzet van de Republiek der Vereenigde Nederlanden gedurende de 17e eeuw (1579-1715)* (The Hague, 1908) table no. 1.

5 Israel, *Dutch primacy*, 281 -282.

criticism. They do not. He neglects to mention the returns for the North Quarter and Frisian admiralty jurisdictions. Is it possible that this is because the North Quarter returns, even without making any allowance for the drop in 1621, conspicuously failed to rise above the level of 1622 until after 1642 while the Frisian returns for the period 1622-1647 actually fell⁶ ?

Van Zanden backs his statistical test with the contention that the Spanish embargoes which I identify as the most important factor damaging and restricting Dutch European commerce between 1621 and 1647 were in fact largely ineffective. In support of this contention he cites Kernkamp's two venerable volumes and quotes Kernkamp's conclusions to the effect that all the Spanish measures were systematically evaded by Dutch merchants. I must confess I find this resort to Kernkamp not a little bizarre. Having spent a sizeable chunk of my academic career, over six years, researching the effects of the embargoes in Spanish, Belgian and German, as well as Dutch, archives, and having written literally hundreds of pages modifying Kernkamp's conclusions⁷, I fail to see what possible logic can there be to quoting him in this context? If Van Zanden chooses to ignore what I have written modifying Kernkamp that is up to him. But the evidence for the effectiveness of the Spanish embargoes, and their wider impact throughout southern Europe and the Baltic is so extensive, so multi-faceted, backed by such an enormous mass of data, that I do not believe that there is any arguable basis on which a historian today can still claim that Kernkamp was right and that the embargoes were largely ineffective. This is not to say that there was not a great deal of smuggling and clandestine traffic to both Spain and Portugal. My argument is not that the embargoes rendered Dutch trade with the Iberian Peninsula impossible but that it made it so much more difficult, costly and risky than before that the advantage in this trade swung away from the Dutch to the Hanseatics and above all the English who (unquestionably) dominated the Spanish trade from 1630 to 1647 but immediately lost this control once the Spanish embargoes against the Dutch were lifted.

Since the publication of *Dutch primacy* further relevant source material has been published, including more commercial contracts and deeds relating to Amsterdam Sephardi merchants active in the early 1620s. An interesting feature of these sources is that they confirm that even before the Spanish checking procedures were tightened up with the introduction of the *Almirantazgo*, in 1624, a high proportion, possibly most, ships, skippers, and crews hired by Amsterdam merchants for voyages to Spain and Portugal after April 1621 were non-Dutch, often English, Hanseatic or French but including also even Norwegian vessels. Hiring foreign crews and ships reflects a considerable pressure on the Dutch trading system partly because it meant exceptionally high freight charges and partly because it meant dispensing with the services of Dutch vessels and crews. Among the testimony we have which proves conclusively the non-Dutch background of crews hired by Amsterdam merchants in the early 1620s for Iberian voyages were a substantial number of declarations and deeds concerning Scottish skippers and seamen⁸. This sustained and frequent resort by Amsterdam merchants to Scottish ships and crews is just one more of a hundred proofs of the deep impact of the embargoes.

6 Becht, *Statistische gegevens*, table no. 1.

7 See Jonathan I. Israel, 'A conflict of empires: Spain and the Netherlands, 1618-1648', *Past and present*, LXXVI (August, 1977) 48-62; Israel, *The Dutch Republic*, 93-94, 134-154, 204-221, 282-292, 336-347; Jonathan I. Israel, 'The Dutch merchant colonies in the Mediterranean during the seventeenth century', *Renaissance and modern studies*, XXX (1986) 90-99; Jonathan I. Israel, *Empires and entrepôts. The Dutch, the Spanish monarchy and the Jews, 1585-1713* (London, 1990) 64-70, 101-131, 133-162, 189-212, 233-245, 371-384, 428-433; Israel, *Dutch primacy*, 127-136, 197-203.

8 See, for example, 'Notarial records relating to the Portuguese Jews in Amsterdam up to 1639', *Studio Rosenthaliana*, XXII (1988) 59, 64, 189; XXIV (1990) 68, 217, 220, 222, 225; XXI (1987) 109.

Most Dutch economic historians, and not least Van Zanden, are convinced that the period 1621-1647 was one of expansion and prosperity for the Dutch trading system and represents 'perhaps the classic years' of that prosperity. In fact, it was a time when the Dutch Levant trade collapsed almost completely, when Dutch trade with Spain and Portugal largely collapsed, when Dutch trade with Italy was seriously reduced, and when Dutch Baltic trade was also to some extent adversely affected. There is a very great error which I believe is inherent in the serialists' habit of relying on runs of statistics without paying enough attention to the specific historical circumstances which shaped those figures. The figures are indeed highly problematic. I readily concede that if my analysis of the 1621-1647 period rested exclusively on the statistical evidence available, or even mainly on such evidence, the chances would be that my evaluation of them would be as wrong as Van Zanden's. My being certain that Van Zanden has got it wrong stems not from any statistics but from thousands of pieces of nonstatistical, documentary evidence which have a bearing on the question. Numerous contemporary observers attest to the severity of the recession in Dutch trade and shipping during the 1620s and early 1630s, and the continuing weakness especially of Dutch Iberian and Mediterranean commerce down to 1646. Of course, serialists and Braudelianists refuse to take such 'impressionistic' evidence seriously, considering it to be intrinsically inferior to quantifiable data (even though, ironically, Braudel himself utilized great quantities of impressionistic evidence, using statistics merely to illustrate his points — in this respect rather as I do myself). What the serialists have done is to treat the insecure as reliable discarding the indispensable check! If my statistical base is 'shaky', so is Van Zanden's and anyone else's engaged in studying a period for which so few really reliable statistics are available. I am convinced, nevertheless, that I am on much surer ground than Van Zanden because my method never allows the rows of statistics validity except when they are placed in, and rigorously examined against, a background of very large quantities of non-statistical evidence. It is, turning the Braudelian-serialist philosophy of history upside down, the non-statistical evidence, taken *en masse*, which is reliable. This is the dependable guide and check which enables us to interpret so problematic and difficult a statistical source as the *convooien* and *licenten* returns correctly. In 1625, for example, the Remonstrant preacher Paschier de Fijne, claimed that Holland was suffering from a general commercial recession which, according to him had been in progress not only for the last four years but even longer:

siet doch hoe zijn de huysen alreede afgheslagen? Hoe zijn alle koopmanschappen, neeringen en ambachten verslapt? Men hoort huysluyden, koopluyden, ambachts-luyden jae soldaten, matrosen en allen klagen: die wat veel in schuyten en op waghnen reyst die weet oock dat ick hier waerheyt segge⁹.

De Fijne implied that the recession was more than four years old because he was not interested in proving how much economic damage had been caused by the resumption of the Spanish war; rather he was trying to argue that religious intolerance, and especially the persecution of the Remonstrants, was responsible for the general severe down-turn in trade. But, whatever his

9 [Paschier de Fijne], *Silvere vergulde naelde bequaem om af te lichten de vliessen van de oogden der Hollandtsche regenten* (S.l., 1625) A4; in another pamphlet published the previous year, De Fijne wrote 'slaen wy de oogden door het geheele lant zien wy niet op alle plaatsen de koopmanschappen, neeringen ende ambachten verminderen, verslappen en te niet gaen? Hooren wy niet de Zee-luyden, de Borgers, de Huysluyden en alle menschen klagen?' see Paschier de Fijne, 'Een Broederlicke Vermaninge', *Eenige Tractaetjes* (2 vols.; Amsterdam, 1735-1736) ii, 5.

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motives for discussing the depression, his line of argument would have been pointless had it not been generally obvious to his readers that Holland had indeed for several years been plunged in the worst recession in living memory. For Dutch European trade, climbing back from the recession of the 1620s was to prove a slow and painful business which scarcely even began until about 1633. It was not until 1647 that, very suddenly, a tremendous new impetus and dynamism came into the Dutch overseas trading system. The statistics too, as I have shown, tell this story. But because of the problematic nature of such statistics it is only because of an immense mass of other, non-statistical evidence that we can be certain that this *is* the story which the statistics tell.

Much of the rest of Van Zanden's criticism arises from a misunderstanding. He notes that in 1585-1590 which I characterize as a time of difficulty for the Dutch trading entrepot, and again in 1621-1647, that the Dutch textile industries grew vigorously, as if this somehow invalidates my argument that these were periods when Dutch seaborne commerce was seriously obstructed and impeded. But his criticism here is entirely beside the point. I too stress the vigour of textile manufacture during these two periods in my book, as would any historian. When Van Zanden asserts that this expansion, and other signs of vigour in the domestic economy, prove conclusively that the *economische conjunctuur in Holland* was one of overall expansion, I can only conclude that he has failed to grasp my argument. Whether or not the *economische conjunctuur* in the Braudel-serialist sense of the total volume of activity was one of expansion or contraction in 1585-1590, or in any other period, is an issue about which my book makes no claims whatsoever. On the contrary, one of my principal arguments is that the Braudelian notion of the overall trend at a particular point in time, *economische conjunctuur*, is an almost completely useless concept in a historical context in which parts of the Dutch economy were growing vigorously and large parts stagnating or collapsing. Absolutely nothing which Van Zanden says detracts in any way from the force of my argument that these were years in which the Spaniards re-occupied much of Gelderland, Overijssel, Drenthe, and Groningen, cutting the overland and river routes to Germany, in which Philip II imposed a general embargo on Dutch ships and goods in Spain and Portugal and seized a large number of Dutch ships attempting to trade clandestinely with Spain, and in which Leicester experimented with a ban on trade with the Spanish Netherlands. Despite the massive influx of immigrants and new skills from Flanders and Brabant in the late 1580s there was, and could be, no immediate major expansion of Dutch seaborne commerce such as began in the early 1590s. There were just too many obstructions and restrictions. Van Zanden seems to think that the *convoeien* and *licenten* returns for the Zeeland admiralty jurisdiction, the only set available for these years, confirm that there was a significant upswing. Once again, he is mistaken. The Zeeland returns were heavily disrupted in 1584/1585 by the Spanish sieges of the Flemish towns and Antwerp, cutting off Zeeland from its trading hinterland. There was a brief recovery in 1587/1588 when great quantities of provisions and other supplies flowed into the Spanish Netherlands in part nourishing Parma's preparations to invade England in combination with the Armada sailing from Spain and Portugal. But over the next few years the Zeeland returns fell steeply, dropping from 450,533 *ponden* in 1588 to 291,018 *ponden* in 1590¹⁰. Hardly evidence of vigorous expansion !

A rather important issue is raised by Van Zanden's re-assertion of the traditional view that

het hoogtepunt van de bloei van de vooral voor de uitvoer werkende Leidse textiel moet eerder gezocht in de jaren tussen 1635 en 1655 dan rond 1670 zoals Israel doet.

10 Becht, *Statistische gegevens*, table no. 1.

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In fact what I argue is that the high-point comes in phase IV, the period 1647-1672, as whole, not 'around 1670'. But I let that pass. Clearly what Van Zanden wants to show is that the high-point of Leiden's production of textiles co-incides with his 'classic years' of prosperity in the general Dutch economy in the period before 1650, that is that it comes somewhat earlier than I maintain. With regard to the two most valuable branches of Leiden's textile output, *lakens* and camlets, which accounted for the great bulk, about four-fifths, of the total value of Leiden's textile output in the 1650s and 1660s, the textiles which chiefly counted in Dutch overseas trade, Van Zanden's contention does not stand up for one moment. Every year from 1635 to 1647 Leiden's *laken* output was below, usually far below, 16,000 pieces annually; from 1647 until 1672, *laken* output exceeded 16,000 pieces in every single year. His contention is even more palpably wrong in the case of camlets. Camlet output at Leiden was of little significance before 1647, climbed to around 30,000 pieces yearly by the late 1650s and then climbed to its zenith, 50,000 pieces yearly which was consistently sustained in the years 1667-1671¹². If Van Zanden looks again at the total value of Leiden's textile production in the middle decades of the seventeenth century, he will see that it did not fall after 1654 but continued to rise. Far from the years before 1647 being part of the high-point of Leiden's performance as a textile producer, Leiden only began to assume her classic role at the end of the 1640s.

I turn now to Leo Noordegraaf's criticism. I propose to be rather brief in commenting on his various objections to my tables and statistics. Here and there his remarks are justified; more often they are beside the point or somewhat 'nit-picking' complaints about small discrepancies which do not materially affect my argument. He spends several lines complaining that my table 6.17 fails to bear out what he says is my contention that 'In 1654 de waarde van luxe-textiel meer dan 9 miljoen gulden bedroeg en in 1665 meer dan 10 miljoen'. But here he has simply misconstrued the meaning of the English text according to which the 'value of Leiden's output... had risen to over nine million guilders and by 1665 to over ten'¹³. Given that table 6.17 says nothing at all about overall output, or connects with this latter statement, the discrepancy which Noordegraaf discerns here lies only in his own misunderstanding of the English. His complaint about table 5.23 concerns a discrepancy so minor that it hardly seems worth discussing.

More significantly, Noordegraaf explains what he calls the *hap-snap karakter* of my statistics with the remark that while for serialists statistics are the heart of the exercise 'vormen zij voor Israel, evenals in de 'traditionele' economische geschiedenis, niet veel meer dan illustraties'. Here he hits the nail on the head. But what was presumably meant as a cutting remark, I choose to take as a compliment. For, as I argued above, I believe it to be essential to restrict statistical work to a subordinate role, subservient to the discipline and guidance of the non-statistical evidence. In the early modern context the inevitable result of the serialists' cult of statistics is to separate quantifiable data from the rest which is then discarded, generating a make-believe world resting on a thoroughly unreliable and insecure statistical base. It also leads to segregating those dimensions of human experience that are more readily quantifiable from the rest, thereby

11 N. W. Posthumus, *De geschiedenis van de Leidsche lakenindustrie* (3 vols.; The Hague, 1908-1939) ii, 883-886.

12 *Ibidem*, ii, 930-931; Israel, *Dutch primacy*, 263; readers will be interested to see that recent research on seventeenth-century Leiden published since my book appeared entirely confirms my argument that it was not in the 1630s or early 1640s but 'rond 1650, toen de textielindustrie haar bloei periode begon' and that Leiden's best period continued to around 1671, see P. Nagtegaal, 'Stadsfinanciën en stedelijke economie. Invloed van de conjunctuur op de Leidse stadsfinanciën, 1620-1720', *Economisch- en sociaal-historisch jaarboek*, L (1989) 101, 105, 132.

13 Israel, *Dutch primacy*, 260.

further increasing the Braudelians' proneness to distortion and error. I do not wish to deny that serialist methods have some validity provided that they are appropriately used. They can tell us about population movements, wages, prices, patterns of consumption and so forth. But are not such results really only modest building blocks which then need to be taken up and absorbed into much more elaborate and complex interpretative patterns of thought? There is no way that there could ever be a truly wide-ranging, general history which endeavours to integrate all the dimensions of human experience based primarily on quantifiable data and serialist concepts. Such an integrative general history can be achieved only by incorporating the results of statistical work into, and checking it against, an immense mass of non-statistical evidence. My dismissing Van Zanden's claim that the high-point of Leiden's production came before, not after, 1650 is clearly justified by the statistics, but is not *based* on statistics. It is always the non-statistical evidence which tells us which interpretation of the figures is right. The mass of non-statistical evidence tells us that until the late 1640s Leiden textiles showed little capacity to invade Leiden's classic markets — France, Spain, Spanish America and Turkey. The English were not worried about competition from Leiden textiles in overseas markets until after 1647. On the basis of the general evidence, the notion that Leiden's best period as a textile producer came before 1647 is, I would say, not only unquestionably wrong, but actually absurd.

Noordegraaf maintains that my book is not a systematic encounter with *serialisme* and that I have ignored many of the topics and trends, including demographic movements, with which serialists have been preoccupied. I have ignored, he says, numerous aspects of the work of serialist historians which in his view and theirs, have a fundamental bearing on the problem of Dutch world trade primacy. Again he is right. In much of my published work I have ventured to criticize Braudelian notions and approaches but only insofar as these seemed to impinge directly on issues on which I was working. I have never sought to engage in a thorough, systematic encounter with serialism. To do so I would need to devote a vast amount of time and energy to studying data, issues, and methods which are largely marginal to my own concerns as a historian. In any case, from the point of view of the general historian—the point of view which interests me—I am unconvinced of the need for an exhaustive encounter with serialism. A more limited test is enough. If serialism can not throw light on, or relate to, as it can not, the great decisive turning-points in the history of a phenomenon such as Dutch world trade primacy, then it is obvious that it is incapable of playing more than a subordinate role in the study and writing of general history. The most crucial shift in the realisation of Dutch world trade primacy was, surely undeniably, the explosive conquest of the rich trades during what I term phase I, the period 1590-1609. In these years the Dutch for the first time broke into, and in most cases quickly gained a dominant position in, the East India, Caribbean, West Africa, Russia, and Mediterranean trades. What can serialist studies in historical demography, prices, wages, consumption, etc. teach us about this tremendous and rapid restructuring of the Dutch world entrepot? Clearly not very much. I notice that neither Noordegraaf nor Van Zanden have anything to say about this decisive transformation, my discussion of which is arguably the point at which my analysis most sharply diverges from the Braudelian approach.

Both Noordegraaf and Van Zanden criticize me for providing no source evaluation and criticism except in the case of the *convooien* and *licenten* returns. Source evaluation is indeed indispensable to all historical study. But I do not agree that formal source criticism, of the kind beloved by serialists, is broadly necessary in works of general history. Of course, there can be no competence in historical study without developing a critical sense in the handling of sources. But formulating one's evaluations of the sources is only necessary, indeed only possible, in

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narrowly framed monographs of restricted scope and specialized character, or else more widely-ranging works based on a very restricted range of sources, and rigid segregation of the dimensions of human experience, such as the works serialists produce. General history in my sense is defined above all by the need and impulse to incorporate and integrate as broad and varied a basis of source material as possible but without interfering with the flow and momentum of the drama being described. Plainly, it is intrinsic to such an ideal of history, resting on an immensely varied mass of sources, that all source evaluation, formal and informal, is essentially workshop stuff, preparatory activity, which has no, and can have no, place, except in a very occasional instance, for some very particular reason, in the finished product.

As was to be expected both Noordegraaf and Van Zanden rush to the defence of the Baltic bulk trade as the principal factor in the making of Dutch world trade primacy. Noordegraaf questions whether the value of the bulk traffic was really as modest in relation to the rich trades as I suggest. So let me make a few brief comparisons. In the 1660s when the Dutch world trade empire was at its height, Dutch grain imports from the Baltic oscillated at around 30,000 lasts annually¹⁴. Grain prices fell steeply during that decade, but if I take a fair average, well above the lower ranges of grain prices recorded for the decade total annual grain imports from the Baltic in the 1660s had a value of around £2,700,000¹⁵. If I bend over backwards to be as generous as possible to this Baltic grain trade for which our serialists have such reverence, I could assess it at 3 million guilders yearly. The annual value of the North Sea herring catch to the Dutch entrepot in this period was of a comparable order of value, also worth between two and a half and three millions yearly¹⁶. Baltic grain and North Sea herring were basic elements of the Dutch overseas trading system. I have never sought to pretend otherwise. But, whether our serialist friends like it or not, bulk trades such as these were relatively modest items in the overall structure of Dutch overseas commerce during the decades when Dutch trade hegemony was operative, that is after 1590 and before 1740. During phase IV (1647-1672) when the Dutch overseas trading system was at its height Baltic grain and North Sea herring put together were worth substantially less than just the textile output of Leiden without even mentioning the rest of Dutch textile production. The annual value of the Dutch 'silver' fleets returning from Cadiz laden with silver, dyestuffs, and other Spanish American products as well as Spanish wool, regularly reached ten millions, exceeding the combined value of Baltic grain and North Sea herring¹⁷. The value of Dutch imports from the Levant on the 'Smyrna' convoys also regularly exceeded the combined value of grain and herring. Dutch imports from the East Indies often exceeded fifteen million guilders yearly by the third quarter of the seventeenth and in some years topped twenty millions, representing six or seven times the value of Baltic grain¹⁸. There can not be any serious doubt that the entire Dutch

14 J. A. Faber, 'The grain trade, grain prices and tariff policy in the Netherlands in the second half of the seventeenth century' in: W. G. Heeres, e. a., ed., *From Dunkirk to Danzig. Shipping and trade in the North Sea and the Baltic, 1350-1850* (Hilversum, 1988) 28; Israel, *Dutch primacy*, 214.

15 The average price of rye, the principal Baltic grain, for the decade was in the region of five guilders per hectolitre, or £150 per last; the wheat price was a little higher, see Faber, 'The Grain Trade', 25,27; Faber calculated that in 1753 when Dutch grain imports from the Baltic were running at a somewhat higher level than during the third quarter of the seventeenth century, total Dutch grain imports from the Baltic, then 34,000 lasts, were worth only four million guilders which, despite the considerable decrease in the overall value of Dutch imports since 1670 amounted to no more than 10% of Amsterdam's total trade, see J. A. Faber, 'The decline of the Baltic grain trade in the second half of the seventeenth century' in: Heeres, e. a., ed., *From Dunkirk to Danzig*, 37.

16 Israel, *Dutch primacy*, 305.

17 *Ibidem*, 257-258.

18 *Ibidem*.

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Baltic bulk trade, including timber and naval stores, was worth only a small fraction of the Dutch rich trades.

Devotees of the bulk traffic regularly remind one that it was still the custom in the second half of the seventeenth century to call the Baltic grain trade the *moedernegotie*. So it was. But the meaning of the term *moedernegotie* in the seventeenth-century context is a rather different one from the meaning pertaining before 1590 when it really was by far the biggest and most important strand of Dutch overseas trade. In the seventeenth century the Baltic grain trade was still the *moedernegotie* in the sense that it was literally the 'mother' commerce from which everything else had subsequently sprung. A similar meaning can be discerned in the phrase 'source ende wortel van de notabelste commercie ende navigatie deser landen' used by Johan de Witt to describe the grain trade in 1671. It is also worth noticing that the phrase *moedernegotie* is mostly employed in the seventeenth century by interested parties, businessmen, seeking government help or support for the grain trade. But these included hardly any of the great merchant princes of the Dutch Golden Age. These specialized overwhelmingly in the rich trades leaving the bulk trade to small merchants.

The rich trades far surpassed the bulk traffic in value. They were also much more important than the bulk traffic in servicing industrial activity geared for export. Noordegraaf endeavours to demolish my thesis that the bulk traffic was of relatively minor significance in this respect and lists numerous examples of interaction between Baltic bulk commerce and Dutch industrial activity which he says I ignored. But here, once again, his criticism springs from failure to understand my argument and is completely beside the point. He insists, for example, that Baltic grain provided raw materials used in the breweries, distilleries, malt-houses and a variety of other establishments. But aside from the gin distilleries which only began to produce large quantities of spirits for export after 1672, the rest were only significant in connection with domestic consumption and the home market. The latter, however, is expressly excluded from the subject-matter of my book in the preface. I made it crystal clear that my arguments have no bearing on consumption in the domestic market and that my book only discusses Dutch industries 'insofar as they bear directly on Dutch performance in overseas markets'¹⁹. If we stick to the point which my book is concerned with, then it is clear that there is no basis whatever for Noordegraaf's criticism. Practically all the principal raw materials essential to those industries which produced the Dutch Republic's manufactured exports in the period when the Dutch trading system was at its zenith — Spanish wool, Turkish mohair, Swedish copper, Indian and Persian raw silk, Spanish American and Levant dyestuffs, sugar, diamonds, American tobacco, and so forth — were components of the rich trades. By criticizing me with his list of breweries, distilleries, malt-houses, and the like, Noordegraaf has in fact neatly confirmed the truth of my thesis that bulk trade was relatively unimportant in stimulating manufactures for export. And what shall I say of his complaint that 'breweries' only receive two entries in my index? He must know himself that it is absurd to criticize someone for not doing what they have said they are not going to do when your own complaint confirms the validity of their reasons for not doing it.

Finally, I wish to consider the question of whether the book really reverts to pre-world war II 'traditional' history writing as Noordegraaf and Van Zanden assert. One of Noordegraaf's most cutting remarks is that

19 *Ibidem*, ix.

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afgezien van de kritiek op het Braudelisme lijkt het boek qua strekking nu erg veel op de traditionele geschiedschrijving waarin handelsbloei en stapelmarkt altijd al graag in het kielzog van het succes van de Nederlandse Statenbond zijn gezien, maar waarin de latere pluspunten van het Braudelisme nog schitteren door afwezigheid.

How far is this justified? It is true that the book has features which at first glance might appear to signify reversion to an old-fashioned style of history-writing. The emphasis on the political dimension, avoidance of technical jargon and serialist concerns the subordination of statistical to non-statistical evidence, absence of formal source evaluation, and the bringing in of all kinds of military, naval, and diplomatic facts, might seem to bear this out. But I can not agree with Noordegraaf's characterization of it. Actually, both Noordegraaf and Van Zanden see its most striking feature in what they call my giving the political dimension primacy in the shaping of economic life. Noordegraaf refers to 'het primaat van de politiek', Van Zanden to my 'these van de dominante invloed van de staat op de ontwikkeling van de internationale handel in de vroeg-moderne periode'. Yet I have not in fact sought to assert the primacy of the political sphere any more than my earlier book, *The Dutch Republic and the Hispanic world* asserted (as was claimed by various critics) the primacy of the economic sphere in political history. Both are based on the same approach to history-writing.

It was not my intention to write an 'economic history' in which primacy is accorded to political factors. I do not believe in 'economic history' or in 'political history'. What I try to do is write general history, in this case the history of a maritime and trading hegemony which as I see it was shaped by a complex mixture of economic and political factors as well as some social and cultural factors which are not properly either 'economic' or 'political'. Human thought and activity is a single continuum in which economics, politics, culture, religion, and social life are always involved and always inextricably interacting. The question which had primacy, the economic or the political, in the shaping of Dutch world trade hegemony is, to my mind, entirely meaningless since what matters is precisely the complex interaction between the spheres. My most fundamental objection to Braudelism and serialism is precisely that these philosophies of history segregate the dimensions of human experience as they segregate sources and historical data, rigidly classifying certain types of source and quantifiable data as intrinsically more reliable and useful than other sources and evidence and suitable for detailed study in isolation. All historical segregation, including that practiced by historians of ideas when they ignore the political and economic context in which ideas interact produces only technical history of limited value which is open to disparagement as philistinism and which undermines our grasp of our past and cultural heritage unless firmly subordinated at all times to a higher concept of general history.

Dutch primacy in world trade is an attempt to write general history. It emphasises the political and military dimensions as all general history must do but it nevertheless sees the phenomenon of Dutch world trade primacy as being based in the first instance on economic factors, above all the functional need in early modern Europe for a central entrepot or reservoir of goods, cheap freightage, specialized ship-building, dominance of the herring fishery, low interest rates and an exceptionally favourable home agricultural base. Political factors are not accorded primacy although they are emphasized. Moreover, most of these so-called 'political' factors, when analysed, are seen to be an inextricable mix of the political and economic which are ultimately rooted in economic reality. For example, I stress the importance of the cohesiveness of the States of Holland for the advancement of Dutch commercial interests abroad. But I also ascribe this

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remarkable cohesiveness of the States of Holland above all to the fact that the main early maritime assets of the province were not located in one or another of the big towns for the most part but, as with the herring fishery and the Baltic grain trade, located in widely disparate parts of the province and numerous small ports. A key political factor is thus ultimately explained as an economic factor, and so I could go on.

General history of this sort, striving to combine as wide a range of evidence, statistical and non-statistical, as possible, drawing the main dimensions of human activity into a single interplay, may be far removed from the world of the Braudeliens and serialists but that may not necessarily mean it is 'traditional' in character. If any pre-world war II historian did in fact ever write a history of the Dutch overseas trading system setting up a broad interplay between economic and political factors, basing the periodization in on actual dramatic shifts in trading performance (which was my main criterion), I remain to this day unaware of it.

Recensies

J. P. A. Coopmans, A. M. D. van der Veen, ed., *Van blauwe stoep tot citadel. Varia Historica Brabantica Nova Ludovico Pirenne dedicata* ('s-Hertogenbosch: Stichting Brabantse regionale geschiedbeoefening/Het Noordbrabants genootschap, 1988, 349 blz., f42,50, ISBN 90 72526 03 1).

Het is waar, zoals op de achterzijde van het boek staat, 'de grote variëteit aan onderwerpen en invalshoeken die in deze bundel tot uiting komt, ligt in de lijn van de veelzijdige persoonlijkheid van Louis Pirenne'. Een aantal van 29 bijdragen van 32 auteurs met in totaal 1000 noten, daarvan zal de lezer niet snel een overzicht kunnen krijgen.

Dr. L. P. L. Pirenne nam in 1988 als rijksarchivaris in Noord-Brabant afscheid van het ambtelijke leven. Die functie vervulde hij sedert 1963. Daaraan voorafgaand was hij gemeentearchivaris van Den Bosch. Pirenne nam talrijke initiatieven en vervulde vele bestuursfuncties, zoals uit het bijgevoegde curriculum vitae en het inleidend artikel van J. P. A. Coopmans en H. Th. M. Roosenboom blijkt. De lijst van publikaties laat zien, dat Pirenne zijn kennis ook in geschrift uitdroeg.

De inhoud van de bundel met artikelen van zoveel bekenden van Pirenne is chronologisch gerangschikt. Samenhang kan men willen zien of niet. Vele bijdragen hebben betrekking op aspecten van de lokale geschiedenis met Den Bosch (12 bijdragen hebben daarop min of meer van doen) als hoogtepunt. Andere plaatsen zijn Antwerpen, Boxtel, Breda, Eindhoven, Gemert, Helmond, Hoogstraten, Leuven, Oosterhout, Turnhout en Waalwijk. Een thematische verdeling valt ook te maken (economische geschiedenis, kerkgeschiedenis, militaire geschiedenis, rechtsgeschiedenis, sociale geschiedenis, demografie, diplomatiek, heraldiek, biografie en broncommentaar). De bijdragen moesten — zo groot in aantal — natuurlijk gering in omvang blijven. De artikelen hebben een gemiddelde omvang van acht en een halve bladzijde. De beperkte omvang betekent, dat de auteurs weinig ruimte nemen voor inleidingen. De lezer wordt verondersteld bijvoorbeeld de samenstelling van het Bosse stadsbestuur te kennen voor hij aan een artikel begint. De artikelen maken een verzorgde indruk en geven ondanks hun geringe omvang veel informatie.

Enkele wil ik naar voren halen. Wie met archiefmateriaal uit de Bataafs-Franse periode heeft gewerkt of met gedrukte bronnen uit die periode, weet hoe rijk de informatie is. Daarom stelt P. M. M. Klep in 'Kanttekeningen bij het Bataafs-Franse beschrijvende bronnenmateriaal in Nederland, 1795-1813' met verbazing vast, dat de Bataafs-Franse beschrijvingen van het maatschappelijk leven zo weinig zijn gebruikt. Noch van daarvoor noch van daarna is materiaal van vergelijkbare kwaliteit en kwantiteit beschikbaar. Hij pleit voor (verder) gebruik van dit materiaal, dat vergemakkelijkt kan worden door verbeteringen in de ontsluiting (archiefinventarissen, verbetering van de eigentijdse ontsluiting, een complete bronnengids van die periode, verdere broncommentaren en bronnenpublicaties). De talrijke voorbeelden van enquêtes ondersteunen het nuttige betoog in sterke mate.

Een ander artikel, dat zich nog enigszins buiten Brabant afspeelt, is dat van R. A. D. Renting, 'Immigranten uit Noord-Brabant en Limburg te Rotterdam, 1573-1811'. De auteur geeft voor Rotterdam steeds met vergelijkende cijfers voor Amsterdam aan, hoeveel trouwpartners uit Noord-Brabant en Limburg naar de Maasstad kwamen. Uit de 10 tabellen zijn verschillende conclusies te trekken. In vergelijking met Amsterdam kwamen verhoudingsgewijs minder