

The Dutch Republic. Its Rise, Greatness, and Fall 1477-1806

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In April 1616 Hugo Grotius, in his capacity as head of a delegation from the States of Holland to the Amsterdam city council, treated, or subjected, the council to what Jonathan Israel in his 'The Dutch Republic. Its Rise Greatness and Fall 1477-1806...pp.439-40, has described as an immense harangue.⁽¹⁾ In an effort to persuade Amsterdam not to oppose the church policies of the Oldenbarnevelt regime, Grotius, we are told, warned that too much insistence upon doctrinal conformity might bring upon the public Church the fate of the Dutch Anabaptists 'who already have so many sects among them that there is hardly anyone who knows their number, or all their names'.

If religion is what is done with one's solitude, then, of course, we shall never know all that there is to be known, or even all that we might wish to know, about Dutch Anabaptism, or any other sect or church, in the early seventeenth century, or in any other century. Nevertheless, readers of *The Dutch Republic* will get to know a great deal more about the number and names of Dutch Anabaptism than was known apparently to contemporaries of Grotius, and than they are likely to have come across in any single volume history of the Dutch Republic in the early modern period.

Thus, among many other things, readers will learn that there were, in fact, no fewer than six main confessional blocs within Dutch Anabaptism by the 1590's - the Waterlanders, Old Frisians, Old Flemish, Young Frisians, Young Flemish and 'so called High Germans'; they will learn something of the size or importance of these blocs in relation to each other, to other religious groups, and to the population of the Republic as a whole; how they were distributed geographically throughout the Republic, and where their largest concentrations of strength lay; the names of some of their most notable adherents in literature and the pictorial arts, and of some of their leading theologians; something also of the nature of the differences and distinctions between them, in doctrine, organization and life-style; they will learn also, if they pursue the fortunes of the Dutch Anabaptists further, something of the reconciliation and continuance of these differences and distinctions, and of their further refinement, from the time of the Amsterdam rift of 1664, the 'so-called Lammerenkrijg', down to the end of the eighteenth century, during which time Dutch Anabaptism was reduced from six main confessional blocs to three - the Lambs, the Sunists (in turn divided into moderate and conservative wings), and the ultra-orthodox remnants of the Old Flemish and Old Frisians; they will learn too that Dutch Anabaptism declined precipitately in numbers over a century and a half, so that by 1809 it constituted between 1.4% and 1.8% of the population of the Kingdom of the Netherlands, in

numbers 30,890 or 30,780 (depending on whether East Friesland alone is excluded or East Friesland and the former Generality Lands are excluded), whereas in the 1640's Dutch Anabaptists numbered about 75,000. or about 5% of the population of the Republic.

I have taken from the author's treatment of Dutch Anabaptism to illustrate several of the most immediately striking features of the book as a whole. It is illustrative in the first place of an extraordinarily comprehensive and informed historical curiosity, astonishing in the range of its erudition and the grandeur of its scale. All aspects of Dutch history, even the most recondite, are confronted and subject to close and keen scrutiny. As might have been expected, given the author's track record, economic and political matters, or, as he might prefer, politico-economic matters, are prominent. But intellectual and cultural matters also loom large. Treated for the most part in separate chapters, there is, nevertheless, a consistent attempt to connect cultural and intellectual developments with what is happening elsewhere in the Dutch economy, in Dutch society and in the Dutch political world, and, where it seems to the author to be appropriate, in the world outside the Dutch Republic. In particular, an effort is made to connect developments in Dutch art to the successive phases in the development of the Dutch economy perceived by the author as having occurred in the seventeenth century. One of the most intriguing of the connections suggested is, if I understand the passage correctly (p.559), to the effect that the switch to monochrome, predominantly brown and gray pictures in the early 1620's, in many genres, may have been affected by the collapse of Dutch trade with those areas of America and southern Europe which provided the substances from which Dutch painters made their most brilliant red, blue and yellow pigments: as a consequence those pigments became both scarce and expensive. The suggestion may of course prove to be not so much a red herring as a pigment of the imagination, but it seems to be a suggestion worth taking seriously.

In chronological terms, the emphasis in the work is where it is usually to be found in studies of the Republic in the early modern period, and where arguably it should be placed, on the period between c.1588 and the death of William III in 1702. Well over half the text is devoted to this period. The eighteenth century gets more space than it usually gets, and there are some welcome features, particularly the innovative treatment of the Dutch Enlightenment. But on the whole the treatment of the eighteenth century lacks something of the authority, though not the clear-cut confidence, characteristic of the earlier sections of the work, and lacks too something of the earlier pertinacity in following things through, as well as command of detail.

Massive in its scope, the work is also rich and dense in its detail, particularly in its statistical detail. Like its predecessor, *Dutch Primacy in World Trade 1585-1740* (Oxford, 1989), it has many statistical tables. *Dutch Primacy* had 99 tables, the present work has 52, 48 of which are new, or rather are additional to those previously published in *Dutch Primacy*. In this respect, the contrast with Professor Israel's successor volume in *The Oxford History of Modern Europe*, E.H.Kossmann's, *The Low Countries 1780-1940* (Oxford, 1978), could not be more marked. Kossmann provides no statistical tables for a period in which statistical data are more voluminous, and more reliable, than they are for the early modern period of European history: a case, one supposes therefore, of deliberate abstinence, of *tabula erasa*.

Kossmann's work is no worse for the lack of statistical tables and is easier reading. Israel's would be much impoverished without them but is sometimes heavy going. He draws frequently upon statistical tables, especially in matters social and economic. They are necessary, the author would argue - has argued - to help to illustrate, to underline, to flesh out, even 'to confirm', what is already suggested by a mass of non-statistical evidence, and have to be set within - are set within - a specific historical context. For the most part, the tables are made to perform the role, and to serve the purposes, for which they were intended, and they are usually evaluated within a particular and detailed historical context, but not always. There are instances, not numerous, perhaps, given the size of the work and the variety of its themes, the multiplicity of its tables and the frequency with which they are invoked, when what is in the tables does not sit easily with, indeed contradicts, what is asserted in the text, or is subject to an interpretation other than that offered by the author. Some of the discrepancies between table and text are trivial, some have more substance and bear upon some of the major themes of the book.

Thus, on p.15, Holland in the late fifteenth century is described as having 'a uniquely high percentage of her population living in towns'. Yet on the same page, Table 1, ('The population of the main provinces of the Low Countries in 1477'), shows Holland with 45w/O of its population urban - only 1% more than Gelderland incidentally - whilst Overijssel is shown in the table as having 48% of its population urban, and is described later in the text (p.113) as having 52% of its population urban in the late fifteenth century.

Or, take the case of Haarlem, and of other inland towns of Holland, in the first half of the sixteenth century. On p.119 we are told that 'all inland Holland towns either stagnated or grew only marginally after 1500.' Yet, according to Table 3 (p.114) ('The population of the main cities of the Low Countries 1300-1560 (estimates), the population of Haarlem between 1500 and 1560 went up by some 30% (from 11,500 to 14,000), whilst in the same period the population of Delft went up by some 33% (from 10,500 to 14,000), and Gouda by just over 28% (from 7000 to 9,000). Admittedly the rate of growth of these towns was very much less than, on a completely different scale from, that experienced by Amsterdam (125% increase), or Rotterdam (c.60% increase), or Enkhuizen (128%), in the same period. Israel's main point still stands, therefore: the maritime towns of Holland grew vigorously, much more vigorously than its inland towns, but, on the basis of Table 3, Haarlem, Delft and Gouda can hardly be described either as having stagnated, or as having experienced only marginal growth, unless by growth is meant annualized growth, which would raise problems about descriptions of growth made elsewhere in the volume.

Table 3 raises more problems of the same kind in connection with statements in the text. It is noted on p.119, and the reference is to Table 3, that there was impressive growth in Utrecht and Groningen between 1500 and 1560, but that more common was the tendency to stagnate, as was the case with Zwolle, Kampen Deventer and Zutphen. On p.308 a rather different story is told: the non-maritime towns of the north Netherlands - among which are mentioned specifically Deventer, Zwolle, Utrecht and Groningen- stagnated, 'for an entire century, down to 1590'. Yet, according to Table 3, between 1500 and 1560, the population of Deventer went up by c.31% (from 8,000 to 10,500), that of Zwolle by c.43% (from 7,000 to 10,000), of Utrecht by c.73% (from 15,000 to 26,000), of Groningen by c.66% (from 7,500 to 12,500), and of Nijmegen by c.37.5% (from 8,000 to 11,000).

Or, take the cautionary case of Middelburg in 1622. It is described on p.379 as a sizable city, with over 30,000 inhabitants in 1622. The reference is to Table 22 (p.381), ('Catholic population of cities outside Holland and Utrecht'), where the estimated total population is given as 35,000. In Table 12 (p.328), ('Urban expansion in Holland and Zeeland 1570-1647 (estimates)'), however, Middleburg's population in 1622 is given as 25,000. It begins to look as if 'over 30,000' has been cited in the text because it is more or less half way between the 35,000 of Table 22 and the 25,000 of Table 12. There seems no reason for doing this, unless in the belief that the truth lies usually more or less equidistant between two extremes, which seems an unlikely explanation, given the author's noted trenchancy. On the evidence provided, it is possible only to say that estimates for the population of Middleburg in 1622 range between 25,000 and 35,000, a margin of error of 40%, and it would have been better to have left it at that.

Sometimes, too, a crucial element in the historical context seems to have been overlooked, with the consequence that a misleading, sometimes an erroneous impression, has been conveyed. Thus, in dealing with changes in the distribution of wealth in Overijssel in the course of the late seventeenth century and the first half of the eighteenth century, Professor Israel notes that the number of small independent farmers diminished, and refers to Table 48 (p.1018), ('Changes in distribution of wealth in Overijssel 1675-1758'). The Table indicates a fall in the percentage of artisans and small farmers from 46.5% in 1675 to 42.7% in 1758. But given that the population of Overijssel apparently rose from some 70,678 in 1675 to 132,124 in 1764 (Dr. Joh. de Vries, *De economische achteruitgang der Republiek in de achttiende eeuw* (Leiden, 1968), p.167), the numbers of artisans and small farmers would seem to have increased by something of the order of 72%. [1675 - 46.5% of 70,678 = c.32,865; 1764 - 42.7% of 132,124 = c.56,417; an increase of c.23,552, or c.72%].

A more important problem arises from the treatment of Dutch urbanization and de-urbanization. Here, although what is said in the text matches the data, it arguably fails to bring out the most crucial implications of the data. On the evidence provided, there can be doubt that Professor Israel is right in stating that the population of the principal cities of the Republic, and of Holland in particular, 'rose during the period 1647-1672 at an impressive rate (p.620), and that there was an impressive growth of Holland's cities between 1647 and 1688 (p.622). But there can be no doubt either, on the basis of Table 30 (p.621), ('The population of the main Holland cities 1635-1700'), that far and away the most impressive growth occurred between 1647 and 1672, an increase of the order of 34%, as against a mere c.1.2% between 1672 and 1688. Indeed, between 1672 and 1688, only Rotterdam of the six main cities of Holland appears to have increased its population, the other five seem to have stagnated. Admittedly there was a slight surge forward in the population of Amsterdam between 1688 and 1700, but the other five main cities, according to Table 30, declined.

In the light of these data, there is a case for dating the beginning the long-term decline of Holland's cities, and the reversal of the long-term trends towards urbanization, in 1672 rather than 1688. That is not say, however, that 'the process of de-urbanization was continuous from the late 1680's'. (p.858) Between c.1700 and c.1720 there seem to have been population recoveries in Amsterdam, Leiden, Haarlem and Delft. (Compare Tables 30 and 44 (p.1007) ('The population of 10 Dutch maritime and industrial cities, 1688-1815') Indeed, on the basis of Table 44, one might argue, as Professor Israel has argued (p.1006), that the end of the urbanization process occurred in the middle decades of the eighteenth century.

However, whatever the time-scale for Dutch de-urbanization, and its precise chronology, its scale, even the mere fact that it had occurred at all, marks out the Dutch Republic when set against the general trend in Europe in the eighteenth century towards urbanization - though not as emphatically, it would seem as Austrian Brabant, or the main cities of Flanders and Brabant, in the first half of the eighteenth century. In Austrian Brabant. the proportion of the population dwelling in cities fell by 11% between 1700 and 1755 (p.1012), and, according to Table 47 (p.1012) ('The population of the main cities of Flanders and Brabant 1615-1754'), the population of the main cities of Flanders and Brabant fell by 20% between 1690 and 1755. Yet, elsewhere (p.1006), it is noted that the proportion of the Dutch population living in the Republic's 30 largest cities 'having remained almost unchanged between 1690 and 1730, contracted from c.36.3% of the total population in 1730 to 32.8% in 1755, a fall of 3.5% over a period of 65 years. And in roughly the same period, between 1700 and 1749 (Table 44 (as above), and Table 30 (p.621) ('The population of the main Holland cities 1635-1700'), the combined populations of Amsterdam, Leiden, Rotterdam, Haarlem, and Delft - the only cities for which data between 1700 and 1749 appear to be available - fell by c.14%.

These figures, so far as they relate to the first half of the eighteenth century, are difficult to square with the assertion that the process of de-urbanization in the Austrian Netherlands was only remotely comparable (p.1011), was of only remotely comparable severity (p.1012), with what occurred in the Dutch Republic. Rather the reverse seems to have been true for more than half of the eighteenth century, when de-urbanization in Austrian Brabant, and in the main cities of Flanders as well as Brabant, appears to have been of a significantly higher order of severity than that experienced in the Dutch Republic in the same period. If this is so, then it adds grist to the mill Professor Israel's mill - that the middle decades of the eighteenth century were decisive in the chronology of Dutch de-urbanization, not simply in terms of the Dutch Republic but relative to its southern neighbour.

It is time to turn to non-numerate matters. Readers of Professor Israel's previous work will have come to expect not only weighty scholarship, but strong, clear, forcibly expressed, independent judgments, well-supported by evidence, which take issue often with received wisdom and with long-established orthodoxy: it has made for lively reading. The present work will not disappoint. There is much to command attention and respect, and there is much that invites comment, or that provokes disagreement.

There is in the first place the repetition of the argument advanced in Dutch Primacy that the explanation for Dutch dominance in world trade lies not in its dominance of the Baltic trades but in its conquest of what is

described as the 'rich trades' - trades in sugar, spices, dyestuffs, silks etc. - as well as Spanish American silver. I lack the knowledge to come to a view on this matter, but the case as presented by Professor Israel has force and seems persuasive. I make only three general points.

The first is that only an informed reader, a reader familiar with the reception given to Dutch Primacy by Dutch historians, would be aware that Professor Israel's case is hotly contested (2) or, indeed, that there is a contrary view. The second is prompted by the evidence Professor Israel provides of the extent to which immigrant wealth, derived from the Baltic trades, the so-called *moedernegotie*, suckled the birth and infancy of the 'rich trades'. There seems to me to be present here something of the chicken-and-egg problem. Granted that without the fundamental restructuring of the Dutch overseas trading system in the 1590's, the recent influx of immigrants and skills could not have been absorbed (p.311), there seems also to be a case for saying that without the influx of immigrant wealth, the fundamental re-structuring of the Dutch overseas trading system might have been delayed, and would have been much more difficult to realize.

My third point concerns perceptions and their importance in the policies of the Dutch Republic. For whatever the realities of the Dutch trading system, there can be no doubt that the Baltic trades continued to be regarded in the Dutch Republic, and outside it, in the seventeenth century and beyond it, as the source and mainspring of the country's economic primacy and prosperity, as well as its maritime strength. The herring fishery, in particular, was regarded with particular reverence, as valuable as gold - 'a Mine of Gold' -, and enjoyed unique protection in the Dutch economy as the only commodity which could be loaded or unloaded, day or night, weekdays, Sundays, and holidays, throughout the year. Also, such perceptions informed and determined decisions in foreign policy. When in 1718 the Dutch were agonising over whether or not to join the Quadruple Alliance, what caused them, and Holland and Amsterdam in particular, to dig in their heels and refuse to accede, was their inability to secure from Britain and the other signatories to the alliance a guarantee of the highest possible security for the Baltic trades, rather than, as is argued (p.998), their deep concern about the sluggish revival since 1713 of Dutch trade with Spain and Spanish America.(3)

But the most striking of Professor Israel's challenges to orthodoxy is his interpretation of the Dutch Revolt. In brief, the argument is that the Netherlands north and south of the great rivers were already largely separate entities before the Revolt. The Revolt essentially confirmed an already existing, underlying separation. The unification of the northern provinces was both inherently logical and eventually irreversible. The iron logic of the situation meant that the non-maritime provinces of the north were sucked into Holland's sphere of control. The outcome of the Revolt was the necessary conclusion to the previous history of the region and not, as Pieter Geyl argued, the product of particular military and strategic factors.

There is considerable strength in the argument. Professor Israel makes a strong case that large differences between the provinces of the north and the south existed before the Revolt, though it may be argued that he underplays the existence and strength of integrating intellectual and judicial forces before the Revolt.

More to the point, or at least to my point, is the difficulty I see created by the admission that the fate of the Revolt was several times in the balance and might have been crushed: had Leiden fallen in August 1574, the Revolt as a whole might have been crushed - 'With the entire Revolt in the balance'.(p.181); 'if Orange had been a less skillful and tenacious leader the Revolt might well have been overwhelmed militarily'.(p.183-4). There seems to be an uncomfortable conjunction here of the contingent and the necessary, unless of course the Revolt is to be reduced to a blip in the inexorable progress towards duality, interesting but essentially irrelevant.

The religious dimension to the Revolt also comes in for provocative treatment. At the outset of the Reformation in the Netherlands, Catholic commitment among the people was weak, 'as in England', or not as in England you follow Scarisbrick et al. Massive defection followed, which affected both the elites of society and the population in general. By the early 1560's 'Protestantism, open, semi-concealed, or Nicodemist, saturated the entire country'.(p.141) That will seem a puzzling claim to those familiar with the widely-held view that only a small proportion of the population at this time was committed to Protestantism,

and that it took nearly 100 years, until 1650, for the situation to be reached when Protestantism could lay claim to the allegiance of about 50% of the population of the Dutch Republic.

Religion figures prominently in Professor Israel's treatment of the seventeenth century. There is a well-informed account of the nature, extent, and the limitations of tolerance in the Republic, in practice as well as in theory, and of its growth in the eighteenth century, in respect of Catholics, Lutherans, and especially Jews, for whom, 'decline or no decline, the Republic was...a land of opportunity', albeit limited opportunity, hard-won, by no means universal, and under persistent pressure, as seems to have been the case of tolerance generally in the Dutch Republic.

More arresting, however, is his treatment of the divisions within the Reformed Church: not just the familiar Remonstrant Contraremonstrant division of the first decades of the seventeenth century, but the later divisions between Voetians and Cocceians. For Professor Israel these later divisions constituted an integral and important part of political conflict, and of 'party-faction' conflict, in the later seventeenth century, with repercussions which were felt throughout Dutch society. It is a post-Roorda, post Namierite approach to Dutch politics in the period, and is refreshing.

Refreshing also, though mostly in the sense of invigorating dissent, is Professor Israel's treatment of Dutch foreign policy under William III and after. Dissent arises principally from the priority attached by the author to economic factors in determining the Republic, and Holland and Amsterdam in particular, to join a general coalition with the Habsburgs and to prepare for the invasion of England in 1688, and in determining the Dutch role in the so-called War of the Spanish Succession. For Professor Israel the decisive factor, the pivot upon which everything turned in 1688, was the economic war between France and the Dutch Republic, just as, in the so-called War of the Spanish Succession, the reasons for the Republic's involvement in the war, and for its determination to continue with the war, in the face of unprecedented damage to the Dutch trading system, were, in the first place, and fundamentally, concern for the survival and recovery of the Dutch trading system.

To begin at the end, it might be argued that what determined the role of the Dutch in the so-called War of the Spanish Succession was neither the interests of Holland nor the traditions of William III, but the strategy of the coalition. Alternatively, or additionally, it might be argued that too sharp an antithesis is being made between the interests of Holland and the traditions of William III. Serious and debilitating conflicts between the two had arisen in the 1680's, but there was a point, a level, at which they coalesced - were subsumed - in a common concern to achieve an enduring strategic security and, above all, in a determination to avoid a repetition of the trauma of 1672. The spectre of 1672 haunted Dutch foreign policy in 1688, when the threat of a renewal of the Anglo-French collusion of 1670 and 1672 seemed real to the Republic and was widely feared in the Republic, and among its leaders. This spectre, and this fear, seem to me to have been the true pivot upon which Dutch foreign policy turned in 1688, and turned again in the so-called War of the Spanish Succession.

Part of the evidence for this latter claim comes from the French political agent in the Republic in 1706, Helvétius. Helvétius is cited in support of the view that, despite the enormous damage done to the Dutch trading system by the war, there was solid support among merchants, manufacturers, and the public, and in the States of Holland, for going on with the war, and in the context of the development of the argument that what brought about consensus was the conviction that if the Bourbon Succession was not reversed, the old trading system would be done for anyway. Helvétius certainly provides support for the view that there was solid support in Holland in 1706 for going on with the war, and for the view that the union of the Bourbon thrones was seen by the Dutch to pose a great threat to their trading system. What he also writes, however, is that that consensus in favour of going on with the war had more to do the trauma of 1672, and a determination to avoid its repetition, than with the damage being done to the Dutch trading system. I quote the passage:

'Un autre motif beaucoup plus essentiel qui les a embarqués dans la ligue, c'est celui de la liberté, pour laquelle ils sont prêts, disent-ils de risquer non seulement leur commerce, mais tous leurs biens et leurs vies mesmes. Elle n'a pour remparts que les Pais Bas Espagnols contre les forces redoutables de la France, qui malgré cette barrière les a mis à deux doigts de leur perte dans la guerre de 1672 que seroit [?inevitable] si cette monarchie réglant à son gré le gouvernement de celle d'Espagne venoit à entreprendre une seconde fois la conquête de la Hollande....Ils reconnoissent qu'une bonne paix est l'unique remede qui puisse reparer les breches faites à leurs finances et à leur commerce, mais ils sont persuades qu'une paix mal concue, mal cimentée, et peu fidellement observe ne serviroit qu'à introduire la politique de France en Espagne et serrer par ce noeud l'union des deux couronnes à augmenter leurs forces déjà trop redoutables et à préparer aux Provinces Unies des dangers infiniment plus terribles pour elles que les incommodites qu'elles essuient aujourd'hui.'[\(4\)](#)

It is difficult to see this passage as other than a clear echo of the priorities of Dutch foreign policy as understood and practiced by William III, and as enunciated by him to the States of Holland on the 4th. of November 1683, when he declared that, although commerce was the pillar of the state, he would not risk the state for trade and, if the state was lost, commerce would also be lost.[\(5\)](#) It was the traditions of William III, rather than the interests of Holland, that defined the reasons for Dutch entry into the so-called War of the Spanish Succession, for their continuance in the war in 1706, and afterwards, and shaped decisively the kind of peace the Dutch would seek.

In the event, the Dutch did not get the peace they wanted, or that William III had envisaged. After 1713, as before, the Dutch were between a rock and a hard place, driven on the one hand by the need, as they perceived it, never to separate from Britain, and, on the other, by the need at all costs to avoid involvement in war, which might follow from too close a connection with Britain. As Professor Israel shows, for some decades at least after 1713, the Dutch were able to follow in their foreign policy a course which protected essential Dutch interests. But there can be no doubt that in terms of the pecking order of the European power system, the Republic had declined by 1713, and was seen to have declined.

Decline is an unavoidable theme, a dominating theme, in discussing the Dutch Republic in the eighteenth century. Historians continue to disagree about the nature and extent of the decline, about when and where the rot started, about when and where the rot became apparent, but the fact of a decline is accepted.

Professor Israel has much to say about decline: indeed, the last section of his book is entitled 'Age of Decline 1702-1806'. It cannot be denied that the title is appropriate, though, as Professor Israel is at pains to make clear, the 'Age of Decline' was also in important respects, and for a considerable part of the period, an age of continued vitality. For he argues convincingly that the Dutch Enlightenment, rooted in the technological and other scientific advances in anatomy, botany and medicine of the Early or Pre-Enlightenment, made the Dutch the instructors of Europe in these fields, and continued to be an influential force in Europe generally until 1740 or 1750. In the course of the argument he writes of the Dutch Enlightenment and the Dutch Huguenot Enlightenment as distinct and, in some respects, separate phenomena, of a cultural divide between the intellectual flowering in the French language, on the one hand, and Dutch and Latin, on the other.

In what is said about the Dutch Enlightenment there is much that will be unfamiliar to English readers. There is also undoubted force in the distinction drawn between the Dutch Enlightenment and the Enlightenment of Bayle and others, but less force than appears at first sight. It is true that very few Huguenots, including the Huguenot elite, knew Dutch, though Basnage did. It is also true that a great deal of the material concerned with the late Cartesian and Cocceian movements was in Dutch. But much of it was in Latin, which was accessible to the Huguenot elite. The process, therefore, whereby Dutch religious and intellectual controversies were 'screened out' from the European agenda of the Enlightenment, may have had less to do with the linguistic barrier of Dutch, than the pressures exerted upon Huguenots by the Walloon

church not to get involved in Cocceian-Voetian disputes and related matters, to which reference is made, and perhaps to a natural prudence on the part of recently arrived immigrants not to give hostages to fortune by getting involved in Dutch party and factional conflicts. In any case, there were holes in the screen. Leclerc, if he can be regarded as a Huguenot, an honorary Huguenot perhaps, was a friend of Van Limborch, who is said to have acted as a 'taalbemiddelar' for him, and he was also a friend of Anthonie van Dale, one of the Early Enlightenment figures, who wrote in Latin as well as Dutch.⁽⁶⁾ Leclerc also acknowledged the stir caused by Balthasar Bekker's *De Betoverde Wereld*, and the importance of making it known immediately to those who did not read Dutch, by providing a lengthy extract in French for readers of his *Bibliothèque universelle et historique* in 1691, the year of the work's publication.⁽⁷⁾

But Professor Israel is a bit shaky on his Huguenots. He is too dismissive of the contributions of Huguenots to the Republic's role as the hub of the European Enlightenment, reducing them to 'additional factors', and he mentions not at all their role in introducing Europe to English science, English culture and English history. At the same time, however, as he underestimates, and fails to acknowledge the importance of some Huguenots, he grossly inflates the importance of one Huguenot, Rousset de Missy.

Rousset de Missy, who is best known as a leading journalist and as an Orangist agitator in the Orangist Revolution of 1747-51, is described as radical, a participant in the compilation of 'arguably the most sensational text in the Radical Enlightenment', *Les Trois Imposteurs*, indeed, the standard-bearer of its tradition, a fierce critic of regents and regent corruption as well as of traditional thought since 1719, and an eager advocate of the dissemination of Locke political ideas.

It would be wearisome - to me certainly - to go over ground already ploughed.⁽⁸⁾ Suffice it to repeat that on the evidence presently available, all of these descriptions of Rousset de Missy are either unclear, as in the description of him as a radical, or, in the case of the other descriptions cannot be justified in the light of available evidence.⁽⁹⁾ Even the existence of 'The Radical Enlightenment', still seems to me to be in question.

Difficult to justify too is the description of the Orangist Revolution of 1747 as 'a full scale revolution', as difficult as it is to accept the view that 1702 witnessed 'a fundamental change in the structure of Dutch politics'. 'Fundamental' is an over-used word which crops up with unusual frequency to describe the recurrent spasms in the Dutch body politic between Stadholderate and Stadholderless regimes. What may strike the reader, from the abundant evidence which Professor Israel provides, is the extent to which at these moments of change including the Second Orangist Revolution and the Batavian Revolution - change was defined as the restoration of some golden or better or mythical past: revolution perhaps, but revolution largely in the sense of a return to a previous point of departure. The Batavian Revolution was a further development of the tendencies manifested in the movements of 1672, 1702-3, 1747-8 rather than (as has often been supposed) a fundamental new beginning linked to revolutionary trends elsewhere in the late eighteenth-century western Atlantic world'(p.1105), and (rather than is supposed) the first major European manifestation of the wider Atlantic revolutionary upheaval engulfing the western world in the wake of the American Revolution.(p.1119)

A brief word is called for in respect of the treatment of the economic decline of the Dutch Republic. Here again Professor Israel is characteristically forthright about a complex and contentious problem. The economic Golden Age is held to have lasted broadly from 1590 to 1740. Long before that, however, the gold had become tarnished. A long-term deterioration of the Dutch economy had set in between 1672 and 1677, though the permanent, irreversible decline of Holland as a maritime and industrial power commenced only in, and around 1688, with the onset of the 9 Years War, from which time dates also the long-term decline of Dutch cities. Even in the early eighteenth century, however, there was some light amidst the encircling gloom: Dutch commerce continued to be strong, shipbuilding along the Zaan, and some manufactures reached their peak, and the Republic continued to be an important centre of European finance. From the 1720's, however, the Dutch trading system was crumbling, and the main Holland towns declined rapidly in their trade and industry. Economic decline, both absolute and relative, and widespread social decay, were the grim realities of the Republic by the 1740's and thereafter in the eighteenth century.

It is not the only view of Dutch economic decline, nor the prevailing view, but it is a view that is well argued and will surely provoke debate. It would have been useful, however, if it had been made clear to readers that other views exist.

It would have been useful too in this connection if Professor Israel had brought together the various explanations that historians have provided for Dutch decline, and given his view on them. Granted, as is asserted, that the root cause of decline lay in external factors, in the damage done by the growth of strongly protectionist economies in many European states in the eighteenth century, and, in the longer term, in the greater damage caused by the Republic's lack of the natural and imperial resources which were available to sustain Britain's economic growth, what of other factors?

Many factors have been suggested as contributing to Dutch economic decline. Professor Israel mentions several, but he does not take them very far. He mentions, for example, 'moral decline', as a contemporary obsession, which it was, but not the fact that it has been the subject of lively debate among Dutch historians. He mentions also 'the astounding increase in Dutch investment abroad', and adjudges it as both a symptom of Dutch economic collapse, and a factor strengthening the economies of the Republic's competitors, particularly Britain. These carefully worded comments suggest that he knows more than he is saying. In that event, it is a pity that he did not say more, for what he does say raises a number of matters that deserve ventilation.

There is in the first place the question of the extent to which investment abroad by the Dutch in the eighteenth century had increased over the seventeenth century, when there had also been investment and speculation in foreign enterprises and foreign funds. Laying that aside, it seems clear that Britain's industrial development was only marginally assisted by Dutch capital and would have occurred without it. Moreover, even if Dutch capital had not been exported abroad in the eighteenth century, it is by no means certain that it would have been invested in risky industrial enterprises. Many of those who invested abroad in British funds in the eighteenth century were small investors - spinsters, widows, retired military officers, orphanages, clerics. It seems reasonable to suggest that such people were looking for security, stability and absence of risk, for immediate and certain returns on their capital. It seems very unlikely that such investors, with such requirements, would have settled for something more risky, or that in a country where the social prestige of merchants stood much higher than that of the industrialist, that they would have invested their money in Dutch industry. In any case, which industries? It would have been valuable to have had Professor Israel's views on these matters.

Similarly, something could have been said about other factors which have attracted the attention of historians: the importance of levels of indirect taxation; the high costs of labour; the increased costs of fuel, specifically of peat, allegedly the equivalent in importance to the Dutch economy in the seventeenth century of Groningen gas in the twentieth; to declining demand as an inhibitor of technological inventiveness; to the preponderance of the commercial tradition; to the lack of a strong central authority, for granted that its lack

had not prevented Dutch economic prosperity in the seventeenth century, was its lack an impediment in the eighteenth century when the Republic faced increased foreign competition? Or, if unchanging structures are an irrelevance, what importance is to be attached to the argument that a tendency towards oligarchisation and ossification in governing circles, towards a more self-enclosed regent governing class, towards a narrowing of the channels of social mobility, induced in the Republic a kind of arteriosclerosis of the body politic, a drowsy, incestuous complacency, which inhibited initiative, inventiveness, enterprise and vision. Perhaps the fate of the Republic would have been better if its fate had been worse. Enterprise and rejuvenation are sometimes evoked by sudden collapse where they are not evoked by steady erosion. The Dutch did not enjoy, or maybe were spared for too long, sudden collapse. And when rejuvenation came, it came first in the shape of political rejuvenation.

It is a tribute to Professor Israel and his work that after 1130 pages a reader should be asking for more. More will undoubtedly be forthcoming. Meantime, however, admiration and pleasure have to be registered for a staggering achievement, the culmination - at least for the moment - of a decade or so of remarkable creativity. It is on a scale which matches the importance of its subject, and it is carried through with unflagging zest. It is certain to become compulsory reading for students of Dutch and European history in the early modern period. Those who read it will emerge not only much better informed about the emergence of the United Provinces as a great power and its decline in the eighteenth century, about its thought, religion, politics, culture and society, and their complex interplay, in the early modern period, but more important they will take away a determination to know more, and to re-examine what they thought they knew. That has certainly been my experience.

If I have seemed unduly critical, my defence is that the work has been consistently stimulating, that the author rejoices in polemics, and that I could have gone on longer. The decline in Dutch universities in the late seventeenth and eighteenth centuries, for example, or more specifically the contraction in student numbers, takes on a rather different aspect, seems less of a problem grounded exclusively in the circumstances of the Dutch situation, if attention is not placed exclusively on foreign students, but takes in Dutch students, and is placed in a European context which saw a widespread contraction in student numbers in many European universities in the early eighteenth century. Or, to turn to the southern Netherlands, which is invoked to point up the contrasts and similarities with the north, and to establish the relationship between the two. If the cultural barriers between the south and the north remained intact in the eighteenth century, they did not obstruct all mutual cultural enrichment: Dutch flower painting and Dutch landscape painting continued to inspire and to find imitators in the Austrian Netherlands. Further, on the evidence provided the Enlightened initiatives of the Brussels regime in the 1750's and 1760's seem characterized more by frustration than by achievement. But there was stimulation to both trade and industry from both Jan van Brouchoven, count of Bergeyck, government minister in Brussels at the beginning of the eighteenth century, and Charles Alexander of Lorraine, governor from 1741-80, both of whom imposed vigorous mercantilist programmes. It was a striking example of what could be achieved with energetic leadership, driven partly by the realization of the Habsburgs that if the Austrian Netherlands were to be exchanged for Bavaria, a better bargain might be struck if the lands were put in good order; and it was in striking contrast to the lack of energetic leadership in the Dutch Republic.

Professor Israel draws attention to the effectiveness of neighbourhood watches in reducing blatant street prostitution and all forms of violent crime, including rape, in Amsterdam in the seventeenth century. If, indeed, rape was reduced, as distinct from having become more difficult to prove, it seems possible that of more consequence was the system of street lighting invented by Jan van der Heyden, and which lit up the city with 1800 public lanterns in 1670. (pp.681-82). I cite this, in an already swollen review, in order to make the point that Professor Israel has shed much light himself. He has illuminated some of the darker corners of Dutch history and shown many of its familiar landmarks in a new light. Also, though he has rejected Pieter Geyl's view of the Dutch Revolt, his own work is an excellent demonstration of Geyl's dictum about history being an argument without end.

The book is also a snip at £25. Oxford University Press deserves our thanks and other university presses

should take note.[\(10\)](#)

Notes

1. Jonathan I. Israel, 'The 'New History' versus 'Traditional History' in interpreting Dutch world trade primacy', in 'Een fraaie synthese op een wankele basis. J.I. Israel, Dutch primacy in world trade.', (Bijdragen en Mededelingen betreffende de Geschiedenis der Nederlanden, 106/3 (1991), 472-4. [Back to \(1\)](#))
2. J.L. van Zanden, 'De 'nieuwe visie' van Israel', and Leo Noordegraaf, 'Vooruit en achteruit in de handelsgeschiedenis van de Republiek' in 'Een fraaie synthese...', B.M.G.N., 106/3 (1991), 451-68 Neither van Zanden nor Noordegraaf appears in the bibliography to the Dutch Republic. [Back to \(2\)](#)
3. Ragnhild Hatton, Diplomatic relations between Great Britain and the Dutch Republic 1714-21 (London, 1950), pp. 174, 181, 185-87. Hatton, which does not appear in Israel's bibliography, would have been useful in another respect, in helping to date the first occasion on which Britain broached with the Republic's leaders the possibility of a marriage between one of the daughters of the later George II - he had three - and the Prince of Orange, later William IV. Hatton makes it clear that the matter was broached in 1721, and it is possible that it had been broached earlier in 1718. ((Hatton, p. 219 and Hugh Dunthorne, The maritime powers 1721-1740. A study of Anglo-Dutch relations in the age of Walpole. (New York/London, 1988), pp. 19-20) In any event, the first occasion on which it was broached was not 1728 (Israel, p. 992). [Back to \(3\)](#)
4. M. van der Bijl, 'De Franse politieke agent Helvetius over de situatie in de Nederlandse Republiek in het jaar 1706', B.M.G.N., 80 (1966), 186. [Back to \(4\)](#)
5. G.H. Kurtz, Willem III en Amsterdam 1683-1685 (Utrecht, 1928), pp. 67-68. [Back to \(5\)](#)
6. De "Bibliothèque Universelle et Historique" 1686-1693. Een periodiek als trefpunt van geleerd Europa. by Hans Bots, H. Hillenaar, J. Janssen, J. van der Korst and L. van Lieshout (Studies van het Instituut voor intellectuele betrekkingen tussen de Westeuropese Landen in de zeventiende eeuw Amsterdam & Maarssen, 1981) vol. 7, pp. 5, 22, 26, 84, and J.J.V. de Vet, Pieter Rabus (1660-1702) Een wegbereider van de Noordnederlandse Verlichting (Studies... Amsterdam/Maarssen 1980), vol. 6, pp. 205-6. [Back to \(6\)](#)
7. De Bibliothèque Universelle..., p. 90. It may be noted that Rabus Boekzaal was not unknown in France. In 1699 Jean Paul Bignon, director of the Académie, ordered a complete series of the periodical from the Rotterdam bookseller, Reinier Leers. Moreover, the Acta Eruditorum, which extracted from the Boekzaal, was well known in England and France, 70 copies of the periodical going to one English bookseller alone in September 1691. (de Vet, supra cit., 144 and A. h. Laeven, De 'Acta Eruditorum'. Onder Redactie van Otto Mencke. De Geschiedenis van een Internationaal Geleerdenperiodiek tussen 1682 en 1707. (Studies... Amsterdam & Maarssen, 1986), The debate about Bekker, therefore, may have been more widely known in England and France than Professor Israel seems to suggest. This does not affect his central point about apparent lack of impact. [Back to \(7\)](#)
8. G.C. Gibbs, 'The Radical Enlightenment', British Journal for the History of Science, xviii (1984), 67-79. [Back to \(8\)](#)
9. Professor Israel writes on p. 1074 of Rousset de Missy 'being eager to spread the influence of John Locke'. He may have been, but there is no evidence that he was in the letter from Rousset de Missy to Bentinck which is cited in support of the assertion. On my reading of the letter, the only eagerness recorded is the initial eagerness of Bentinck to see Locke's *Traité du Gouvernement* published, and the subsequent eagerness of the book's designated publisher to see published a book long promised and already advertised. (See, J. Rousset de Missy aan Willem Bentinck, Amsterdam, 26 juni 1748 (Briefwisseling en aantekeningen van Willem Bentinck, Heer van Rhoon. deel II. Van de Preliminieren van Aken, 30th April 1748 tot aan de dood van Willem IV, 22 Oktober 1751, uitgegeven door C. Gerretson en P. Geylt 's-Gravenhage, 1976), 2 vols, ii (eerste stuk), 71-72. [Back to \(9\)](#))
10. I have come across a few misprints, miraculously few in a book of this size, and completely trivial. I mention them in case they have not been picked up, and in order that they can be corrected in subsequent re-impressions, editions, and translations: p. 75, for Map 5(a), Map 6(a); p. 210, the text of

the articles of the Union of Utrecht has two references to 'His Majesty', not one; p.252, the reference to Table 51 seems to be in error- I don't know what the reference should be; p.616, the reference is to Table 28, not 27; p. 1001, the reference is to Table 45, not 44; p.1021, last paragraph, l.3, should read 'Among the provinces only Holland surpassed Utrecht..', not only Utrecht surpassed Holland', and, on the same page, for Table 48, read Table 49.[Back to \(10\)](#)

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