J.I. ANDRÉS UCENDO, *La fiscalidad en Castilla en el siglo XVII: los servicios de los millions, 1601-1700,* Bilbao, Servicio Editorial de la Universidad del Pais Vasco, 1999, pp. 266.

The study of taxation is without doubt one of the central issues for an understanding of the Castilian economy at the beginning of the early modern period. As is well known, there is a long and consolidated historiographical tradition which sees the tax system of Castile – the heart pf the Hapsburg monarchy – as the main cause for its economic and political decline in the seventeenth century. Notwithstanding this, however, there are still many lacunae in our knowledge of Castilian taxation and of the mechanism through which it exercised such a profound influence on the region's whole economy. Much of our conventional wisdom on the subject still derives from interpreting the data of contemporaries rather from an analysis of the documents.

The crisis in colonial trade and the fall in domestic production, difficulties in state finances and problems ensuing from the sale of offices, all features which marked the history of Castile from the end of the sixteenth century and throughout the following century, emerge from the economic treatises that appeared at that time and which sought to explain what had determined the end of a period of expansion and the beginning of decline. In order to find the means to restore the kingdom's old prosperity, neo-scholastic philosophers, theoreticians about the reason of state, jurists and functionaries and arbitrators of every extraction sought to describe and interpret the event, with very different results.

The re-reading of these texts, especially in recent years, (cf. See among the most recent contributions, *Pensamiento y politica economica en la epoca moderna* edited by L.A. Ribot Garcia and L. De Rosa, Actas Editorial, Madrid 2000), has nonetheless shown with increasing clarity how misleading many of these interpretations have been for a deeper understanding of the subject and point to the need for well-documented research on Castile's economy and tax system in the seventeenth century. This is the premise to Ignacio Andrée Ucendo's study which from a broad historiographical perspective sets out to fill the wide gaps in our knowledge about the subject by examining the *servicios de milliones,* the main source of crown revenue in the seventeenth century.

Ucendo starts by reconstructing the historical series of *servicios de milliones* collected in Castile from 1601 to 1700, which is essential for an understanding of the economic effects that the adopting of this tax system had on the economy. We should point out that is the first time that a historical series of this kind has been reconstructed and published, the fruit of a long labour of reconstruction hampered by the fragmented nature of the sources but made possible by the wealth of documentation kept in the General Archive of Simancas.

Nonetheless, as Ucendo notes on page 157, the lack of reliable statistical data on fundamental variables like national wealth means we are unable to study

the relation between taxation and the economic system in purely quantitative terms and make it necessary to adopt a diverse and differentiated approach according to the social and territorial units involved affected by the tax. For example, the study shows that the burden of *milliones* was especially felt in urban areas, as has already been observed in the case of *alcabala*, the duty levied on goods subject to transaction, which was at first calculated in the region of 5% and afterwards raised to 10% of the value. This observation reinforces the traditional view according to which taxation has been seen as one of the main causes of the crisis in the towns in the sixteenth and seventeenth centuries.

An indicator of this crisis was the transformation in revenue composition of the Castilian towns which recorded a progressive reduction of income from the renting of crown lands, royal and personal taxes, rents and dues and a corresponding growth in tax farming and duties on consumption and commercial transactions, a process we can also observe in Spanish Italy. Added to the unfavorable economic situation, poor administration in the collection of royal and personal taxes and dues but above all an increase in the rate of taxation which began with the revision of the *alcabala* between 1560 and 1575 was followed by that of *servicios de milliones*. The combination of these two factors led to growing indebtedness with the Crown and increasing recourse to revenue farming as well as a symbiosis between the Crown's need to guarantee tax revenue and the urban oligarchies' need to find new opportunities for financial mediation and investment arising from the transformation.

To have a fuller picture of the situation with regard to urban finances the book also reconstructs the series giving the values of *milliones* deflated according to the variation in the value of silver money, the *real*, which is particularly useful for an understanding of how monetary problems at the time affected the Crown's financial capacity.

By making a comparative study of these historical series, the book enables us to identify clearly the phases that characterized Castile's tax system. However it also takes account of several conceptual aspects related to *servicios de milliones*.

In the last twenty years there has been lively debate about this kind of taxation and its political significance. From a theoretical point of view *milliones* were not tax proceeds but a loan, a fresh sum of money which the general parliament of Castile, the *Cortes*, granted to the King for a fixed period of time and under certain conditions which the King was obliged to honour. For their part, the *Cortes* agreed to control not only the collection of the money but also the way in which it was spent. Interpreted in this light some historians have begun to see the *Cortes* as a body that exercised a substantial power of control over the Sovereign.

When this debate arose in the 1990s, Ucendo clearly sides with those who argue that the Crown continued to mantain a much firmer control over *millones* than has generally been perceived. Ucendo supports his argument in the part of the book which examines the way *milliones* were collected, where he shows

that, since 1601, the Crown had exercised an effective control over all phases and that such control was even greater after 1632. As a result, by the mid-seventeenth century *milliones* had become to resemble much more an ordinary tax than a loan voted periodically by parliament.

This overall reading of the political signicance of *milliones* seems convincing, although we should point out that it is based on an analysis that Ucendo carried out mainly on data concerning the first half of the seventeenth century. Such an analysis would have been more complete if equal attention had been paid to the second half of the seventeenth century. At the same time a more detailed study of the tax collectors involved in collecting *millione,* whose role was the focus of much contemporary economic debate, would have given a fuller picture.

In conclusion, this book considerably furthers our understanding of Castile's tax system in the early modern period, enabling us to dispense with some older interpretations of the subject. It is to be hoped that other studies based on a careful examining of available quantitative data will follow this one so that we will get an even more precise idea of taxation and its effects on the economy in the region which was the heart of the Spanish monarchy.

Gaetano Sabatini
University of L'Aquila

J. De Santiago Fernandez, *Política monetaria en Castilla durante el siglo XVII.*, Junta de Castilla y León, Valladolid, 2000, pp. 298.

Seventeenth-century Castilian monetary history was characterized by a fundamental imbalance: as the inhabitants of the country knew too well, silver coins (the famous *real*) had virtually disappeared from monetary circulation, so it was necessary to rely on the widespread use of low-value copper pieces (the *maravedí*) in day-to-day economic transactions. The fact that this happened in the country which owned the world's richest silver mines posed the intriguing question of understanding the reasons for this paradox, which has attracted the attention of researchers since them.

Not surprisingly, this issue has occupied a privileged place in the agenda of Spanish economic-history research. In his books on the price revolution in Castile, E. Hamilton studied the monetary policy adopted by the Crown and, a few years later, A.Domínguez Ortiz analyzed Castile's monetary system in one well-known chapter of his famous *Política y Hacienda en el reinado de Felipe IV.* The influence of both authors has been, and still is, considerable, and it can be said that both established the basic framework followed later by most historians.

The interest in the problem was kept alive during the rest of the twentieth century. In the 1970s, for example, J. Vilar worked on Castilian *reales*, while the

monetary reforms introduced in the reign of Carlos II were described by Collantes and Merino. More recently, in the 1990s, the striking stability of the *real*, especially when we compare it to the frequent devaluations of this kind of coinage in other European states during the century, has been analyzed by A. Motomura, J. De Santiago and J.I. Andrés. In their turn, other authors, like Garcia Guerra and J. De Santiago, have focused on the *maravedí*, describing the way the Crown's policy affected this kind of coin, and the consequences this had on the Castilian economy.

This dual approach has been completed with the articles of J. Sánchez Belén and J. I. García De Paso, who have studied the main principles of the monetary policies in the reigns of Carlos II and Felipe IV from a chronological perspective, and with a number of works on other aspects, such as the articles by G. Murray on the Segovian mint, probably one of the most important in the country, and M. Ruíz Trapero.

One thing that the aforementioned works have in common is that they have all treated different and interesting aspects of the monetary question, but so far we do not have a global and general view of the problem. The main objective of *Política Monetaria* is precisely to fill this gap, offering a broad and complete analysis of the devlopment of the country's monetary system during the Habsburg period.

The argument of the book may be summed up as follows: *maravedí* devaluations were caused by the need to fund the Crown's expensive foreign policy. According to the Pragmática de Medina del Campo of 1497, the *maravedí* had to include a small percentage of silver, but in the reign of Philip III and in the first years of his son and heir, Philip IV, large amounts of pure copper pieces, without any silver content, were issued. This method was widely employed and it prompted a marked rise in copper prices, so from 1636 the governement opted for a second alternative: *resellos*. These were operations in which the Castilians were obliged to carry their copper pieces to the mint. Once there, the legal value of the coins was increased. The owners received the same nominal value they had taken to the mint, plus the transport costs. Obviously, the rest was the profit the Crown got from the entire operation.

The issue of low-value copper coins and the *resellos* had at least three important consequences. First, they caused rises in price levels. Second, to reduce them, these inflationary measures were often accompanied by deflationary ones as in 1628, 1642 and 1664. This sequence of inflationary/deflationary phases introduced an element of uncertainty which seriously damaged the country's economic life. Third, copper manipulations were one of the main reasons for the disappearence of the *real* from Castilian monetary circulation, which can be considered as one of the best historical confirmations of the famous Gresham's Law. As is easy to imagine, nobody would exchange their precious *reales* in return for low-value copper coins at the legal rate. To do this, it was necessary to offer an extra payment in *maravedí* terms, the *premio*.

In the reign of Charles II a new phase began. In contrast to the traditional view, which portrays this period as a phase of unremitting crisis and decline, modern research has shown that, in fact, the Castilian economy began its recovery, while a group of gifted politicians supported long-needed fiscal and economic reforms. From the monetary point of view, it should be remembered that *maravedí* manipulations ended in 1665, and in the eighties a group of reforms, which stabilized the monetary situation and paved the way for the Bourbon changes, were introduced.

In his explanation of the relationship between *maravedí* alterations and price levels, De Santiago includes not only monetary factors. As Paulino Urgorri remarked in an important and influential article of 1951[1], apart from the *resellos* and the issues of pure copper coins, the fall in agricultural output was one important factor behind the inflationary moments. De Santiago does not miss this interesting and basic connection, and he considers a further factor too. The importance of confidence as a guarantee for the stability of a well-balanced monetary system is well known. Studies on modern hyperinflationary processes have tested this factor, and have shown that lack of confidence tends to increase the velocity of money circulation and inflationary pressures. Something similar happened in seventeenth-century Castile. The Castilians knew that *maravedí* manipulations were often followed, with a few months of delay, by deflationary measures, which were very harmful for those who owned copper pieces. The best way of avoiding them was to get rid of these coins before a possible deflation, but this increased the speed of circulation and the *premios*, as the experience of the months previous to the deflations of 1642 and 1664 shows.

The book offers impressive evidence to support the traditional view according to which the frequent monetary changes affected the *maravedí*, while the valuable and precious *reales* were left untouched until 1686. There is little doubt that this policy was deliberately pursued by the Crown, so it is necessary to understand why the government opted for mantaining at all costs the stability of the *real*, especially when we consider how in other European states silver pieces were constantly devalued. Although De Santiago does not offer a clear explanation, he suggests that, as the *real* was used to pay the big expenses the Crown had to face outside Castile, its devaluation would have increased them, so from the point of view of government finances it was better to preserve the *real* with the same characteristics established at the beginning of the period. It should be remarked that this suggestion is in line with the conclusions reached by researchers like A. Motomura, E. García Guerra and J.I. Andrés, who have worked on the topic.

Although the book is sound, some aspects would have deserved a more detailed treatment, especially in the light of the latest research. For example, De

[1] Urgorri P: 'Ideas sobre el gobierno económico de España en el siglo XVII'. *Revista de la Biblioteca, Archivo y Museo del Ayuntamiento de Madrid.* 1951, págs 123-230.

Santiago seems to be convinced that the issues of pure copper pieces in the first years of the century exerted strong inflationary pressures. However, a different view of the same issue can be found in a recent article written by J. García De Paso about the monetary reforms introduced in the years of Charles II[2]. According to this author, the relationship between *maravedí* alterations and price rises was weaker than is usually thought until the mid-twenties. Many sources show that the lack of means of payment was a problem acutely felt in the country during the first years of the century, and it is quite possible that the low-value copper coins helped to mitigate it.

In other cases, we find rather harsh judgments. The author offers a dark portrait of Lerma's period, supporting the traditional and highly negative view of Philip III's rule but, after the recent publication of the Feros' study on this important figure, we would dare to say that the Duke's period deserves at least some revision.

In any case, however, these remarks should not hide the obvious merits of the book. The author has achieved the objectives set out in the introduction. In *Política Monetaria* we find a complete and convincing view of the country's monetary problems, which includes a broad range of factors. Furthermore, De Santiago explains the consequences of these problems for Castilian economic and social life with an easy and clear style which is one of the most important advantages of the book. Considering all this, it can be said, in short, that the reading of this work will undoubtedly be a great help not only for all those interested in the fascinating subject of Castile's monetary problems but, in a broader sense, for all researchers in seventeenth-century Castilian social and economic history.

José Ignacio Andrés Ucendo
Basque Country University

D. E. DUNCAN, *Hernando de Soto. A Savage Quest in the Americas.* University of Oklahoma Press, Norman and London, 1997, (pbk.ed.;1st ed. 1996), pp. xxxvii, 570.
C. HUDSON, *Knights of Spain, Warriors of the Sun. Hernando de Soto and the South's Ancient Chiefdoms.* The University of Georgia Press, Athens and London, 1998, (pbk.ed.; 1st ed. 1997), pp. xxii, 562.

With the acquisition of the territories the Spanish had colonised or merely discovered, the United States inherited an historical tradition which is honoured worthily by U.S. scholars and cultural institutions. The latter have devoted

[2] Garcia De Paso, J: "La estabilización monetaria en Castilla bajo Carlos II". *Revista de Historia Económica.* 2000, pág 70.

commitment, ability and money to the Spanish past of these regions which enables U.S. Hispanic-American scholarship to achieve first-rate results. This is aided by the abundant documentation produced by the officials and by the subjects of the King of Spain in every corner of the Empire. This is the case in the research on Hernando de Soto, the Spanish *conquistador* who, after having taken part in the conquest of Central America and Peru, was nominated Governor of Cuba, and went on to explore and conquer the south-eastern regions of the present-day United States. In a country so eager for antiquity, de Soto appears even in primary-school text-books as the discoverer of the Mississipi, and there is so much interest in him that in the 1930s - during the Great Depression - John Swanton persuaded Congress to spend tens of thousands of dollars on an expedition to retrace de Soto's journey on a scientific basis. This led to a Final Report of the United States De Soto Expedition Commission which established a route that has been generally accepted by scholars until the publication of the two books being reviewed.

The two books in question are worthy representatives of the long U.S. tradition of books on Hernando de Soto. These two books are very different in kind, contents and in presentation technique. The first is a biography of the *conquistador* written by a non-specialist who has brought together all the available printed matter on the subject into a pleasant, lively book. Contrary to the tradition of the English-speaking world, where Hernando de Soto has the reputation of being a generous knight of many virtues, unlike the other *conquistadores* who are deemed bloodthirsty and mercenary, in this book the truth about de Soto is re-established: Duncan shows how de Soto pursued wealth just like all the other adventurers, and how, like them, when it was necessary to achieve his ends, he killed and tortured the unfortunate natives.

Much of Duncan's book is about de Soto's economic activities. Books on the Spanish Conquest have illustrated with a wealth of detail the consequences of the plundering and robbery that went on to the detriment of the Indians, but Duncan concentrates on de Soto's activities as a "Spanish Conquest entrepreneur", always careful to invest the proceeds of the raids in production and trade, only later to reinvest them in the exploration and conquest of new territories. De Soto arrived in Central America with Pedrarias Dâvila at the end of 1513: in 1517, with two other *conquistadores* he formed a company both to trade in native slaves and those supplies the *conquistadores* and the colonizers needed, and to exploit gold deposits, for which they employed the Indians given to them by the authorities. De Soto's profits were reinvested in the conquest of Peru: he took part in the capture of Cajamarca in 1532, sharing in the distribution of a fabulous booty. With his fame and his gold, de Soto was granted by the Emperor the right to conquer the unexplored territory to the north of the Gulf of Mexico, known as La Florida, nowadays part of the United States. In order to undertake this mission, de Soto was appointed Governor of Cuba. Duncan is clever to point out the connection between the conquest of Peru and that of the territory which

is nowadays part of the United States. In 1539, de Soto set sail from the Bay of Tampa on his expedition inland, which took him to the Appalachian Mountains.

For the final, and to the North American reader the most stirring, part of Hernando de Soto's life, Duncan uses the research of Charles Hudson, the author of the second book under review. Hudson is an anthropologist who has been involved in research for some thirty years. He soon became interested in de Soto's chronicles because of his archaeological work on the Indians of the South-East of the United States. In 1976, Hudson published *The Southeastern Indians*, an introduction to the culture of the native peoples of the South-East which investigated their prehistory, their social and cultural institutions and their history. But he soon realised there was an enormous gap in the history of the populations he was studying. The point to be clarified concerned the connection between the elaborate Late Prehistoric Mississipi cultures that erected mounds and the considerably more primitive peoples of the South-East in the eighteenth and the nineteenth centuries: Cherokes, Choctaws, Chicksaws, Creeks, Catawbas and others. Normally, reconstructions of the historical events of that region ignored the sixteenth century and gave very little information on the seventeenth century. Moreover, it seems that already by the eighteenth century the Indians themselves had no recollection of the changes that took place in the region in the course of the previous two centuries. Filling this void, therefore, was a task that could not be put off if an understanding of the history of the peoples of the South-East of the United States were to be reached, and Hudson began the fascinating and useful research, the results of which are presented in this book. Starting with the largest corpus of historical information on the South-East of the United States in the sixteenth century, i.e. information on de Soto's expedition, Hudson aimed to reconstruct the expedition's operations and the route it took so that he could link this information to the considerable amount of archaeological evidence collected in just over a century. The many archival and archaeological findings had by now rendered obsolete the route reconstructed by Swanton in 1930, and so Hudson pursued every possible clue to de Soto's passage so that he could make a more reliable reconstruction of the journey. As Duncan states on page 246 of the book reviewed above, no scholar has spent more energy, enthusiasm and passion in following Governor-General de Soto across the South-East of the United States than Hudson has. This hard work has been well rewarded, and has enabled Hudson to make substantial corrections to Swanton's route: he has established that, when the Spanish moved from the Carolinas towards Arkansas, they followed a more northerly route than the one Swanton mapped out. Furthermore, Hudson suggests new localities for several of the expedition's key episodes, such as the point where Hernando de Soto first saw the Mississipi, and the site of the battle of Mabila, which was disastrous for the Spanish expedition. Once the route of the expedition had been mapped out with sufficient reliability, Hudson could then draw up a picture of the different peoples of the South-East in the sixteenth century, their cultures and their settlements. From this point, he

first goes back in time to examine how the sixteenth-century native societies had developed from the early Mississipi societies, and then goes forward to demonstrate how the world of the South-East visited by Hernando de Soto - after a terrible, irreversible crisis caused by the impact of the Europeans - had developed into the more familiar world of the eighteenth century.

Francesco D'Esposito
University of Chieti.

L.M. Enciso Recio, *La Europa del siglo XVIII*, Peninsula, Barcelona, 2001, pp.843.

This book is a faithful and critical portrait of one of the most innovative centuries the history of the world has ever known. Innovative from a political point of view – one has only to think of the two great revolutions, the American and the French, to stress its importance; from a demographic stance since this was the century when the population, which had already began to rise in the previous century, increased its growth rate and paved the way for its future explosion; from an economic view point, because this was the century which saw the beginning of the industrial revolution, that is, the transition from decentralised and small workshop production to factory production; from a technological point of view, due to the diffusion of water-powered machinery and above all to the invention and use of steam powered machinery; from the point of view of economic thought, because this was the century when economics progressed as a logical and systematic discipline with Adam Smith; from a philosophical viewpoint, with the increasing importance of the study of utopia as a reflection of the social changes that Europe was undergoing; and from a juridical point of view, with the theories of Montesqieu, Filangieri and others.

Divided into five parts, the work investigates demography and society; economic reform and reality; the Enlightenment; the transformation of states; diplomacy and war. Each part is sub-divided into chapters and paragraphs in which single issues are analysed in depth. Enciso has in fact carried out extensive research on the study of the eighteenth century and this book is a synthesis of the great experience he has accumulated in the field and a testimony to the wide range of studies he has conducted over a lifetime, which are presented with a wealth of documentary evidence and an absolute mastery of the subject in a coherent and complete picture.

Whether or not we accept as the beginning of the century the periodisation suggested by P. Hazard (1680 or 1715?) it is a fact that, like the century which has just come to an end, the eighteenth century was a century studded with wars. Yet this was not the definition given by most contemporaries who had clearly before them what had happened in the previous century. Regarding the eighteenth century, the opinions of both contemporaries and historians are often

conflicting, as Enciso points out. The former, for example, defined it as "chatty, charlatan, showy"; Michelet extolled its virtues calling it a "great century"; Renan scorned it, convinced that "during it little thinking was done". Several historians, moreover, have viewed it as an "enterprising and turbulent century", "contradictory and passionate", while positivists, nationalists, liberal democrats and socialists have not hesitated to identify it as "the prologue to the revolutionary era", "the century of reform", "the century of the revolutions". For Enciso, on the other hand, who may be described as a traditional liberal, a liberal in the Crocean sense and whose approach is thus free from ideological bias, the eighteenth century was one of great complexity, which is difficult to delimit, both in relation to its beginning and its end. In other words, it was a century of transition during which the process of modernisation got under way, preparing for the contemporary period, and one in which we may trace the origins of the many problems that torment the world we live in.

But this is not the place to dwell on the great problems that dominate general historiography; rather we should focus on the way in which the major economic, demographic, and social problems have been collocated in the complex historical process reconstructed by Enciso. We should say straightaway that the analysis of economic and social problems occupies a far from marginal place in the book, accounting for little less than half of it, and their treatment goes well beyond the space actually reserved for them. Even when he deals with problems which seem unrelated to economic life, Enciso does not fail to point out the economic and social causes which underlie them or which have helped to influence or determine non-economic situations and phenomena. Despite this, he never falls into Marxist or pre-determined interpretations and his reconstruction is firmly anchored in the awareness of the complexity of any historical process and of the interaction of various factors which dictate human history.

Within a framework of sectoral interdependence, Enciso addresses the problems which marked the various phases of the century in agriculture, industry, trade, banking, finance, the stock exchange, financial speculation as well as in economic and financial policy, providing a lucid interpretation of events. It is no accident that his analysis begins not with agriculture as is traditionally the case, but with demographic issues. It is clear that Enciso ascribes a determining role to such a question in the century's economic and social changes; not only does he dwell on quantitative data and geographical trends but he stresses the fact that not all contemporaries appeared to have grasped the importance of the breakdown in the status quo. Montesquieu, Mirabeau senior, Voltaire and Buffon were of the opinion that the century would witness a huge fall in the earth's population while Hume and others ventured the opposite point of view. Enciso also points to the counterpoint between rural and urban population and to the tendency of the latter to prevail over the former.

Again, in the thirty or so pages devoted to industry, Enciso does not merely provide a picture of industrial organisation at the end of the seventeenth century

and of the typologies of productive systems, with all their light and dark spots, but he also offers a stimulating insight into what were the beginnings of the industrial revolution and of its impact on economic thought at the time.

In the chapter on the "new agriculture", the problem as to whether it was a matter of reform or of revolution is clearly raised and analysed with particular intelligence, revealing the wide range of proposals advanced by contemporaries to increase agricultural production and to enhance its role in political and economic life.

In effect, as emerges from all the chapters devoted to economic and social problems, Enciso's main objective – which he has fully succeeded in – is to highlight the influence of contemporary thought, identifying proposals, ideas and contradictions and thereby increasing our understanding of the great upheaval which men were forced to live through at the time as a result of the transition. In this respect Enciso's work is innovative and original, raising new issues and invoking new comparisons and research.

I should also point out the fact that unlike other scholars, Enciso has looked at Europe with the spirit of our times, as a unit in which there were many and varied national forces at play during the century. He has not allowed himself to be influenced by his national origins, giving his country, Spain, a larger role than that it actually played in the great events of the century. Enciso's Europe, in other words, is by no means a Europe dominated by Spain.

In conclusion, we can say that, this is an original and effective portrait of a significant and very special century which, for better or for worse, laid the bases of our present-day civilisation. The book deserves to reach a wider public and it is to be hoped that future editions and translations will increase its worth with the addition of an index for names and important topics.

Luigi De Rosa

A.P. JACOBS, *Los movimientos migratorios entre Castilla e Hispanoamérica en el Reinado de Felipe III, 1598-1621*. Rodopi, Amsterdam - Atlanta, 1995, pp.336.
I. ALTMAN, *Transatlantic Ties in the Spanish Empire. Brihuega, Spain and Puebla, Mexico. 1560-1620*. Stanford University Press, Stanford, California, 2000, pp.254.

These two books should be on the book-shelves of every historian of early modern times. They are an excellent example of two different approaches to the crucial issue of Spanish emigration to America at the beginning of the early modern period, a phenomenon which made possible the conquest of the American continent and its successive integration into European economy. Auke Jacobs' book presents an overall analysis of migration from Spain to America, with a total reconstruction of migration during the almost 24-year-long reign of Philip III, stating how many people emigrated, where they came from and how

many went back to Spain. It is an impressive research project, which has taken many years to complete: two years were spent solely in gathering material in the *Archivo General de Indias* in Seville. With this research, Jacobs seeks to overcome the limititations of most monographs on this subject i.e. the fragmentary and spasmodic use of rich archive sources that contain material on the Spanish peopling of the New World in order to achieve an exhaustive analysis of the subject. An enormous quantity of information is used. Unlike other research of this kind, which until now has used almost exclusively the *Libro de asientos de pasajeros a Indias*, Jacobs' research uses - as well as the *Libro* which, for the period analysed, contains only 3 pieces in the Seville archive - 130 pieces concerning *Informaciones y licencias de pasajeros*. This source includes the precious *informaciones de limpieza de sangre*, intended to establish whether the person who requested permission to go abroad satisfied the necessary legal and religious requirements; it therefore supplies a considerable amount of data concerning those who intended to emigrate to the New World. Jacobs has also used 2 pieces with the crew-lists of the ships escorting the convoys and the lists of the soldiers on board; there were generally some missing on the return voyage, because many soldiers and sailors deserted once they reached the American ports. He has analysed about seventy pieces concerning the reports of the inspections (*visitas*) of the ships before sailing and the proceedings of the trials of captains accused of or discovered hiding stowaways, trials which took place in the *Casa de la Contratación*. Lastly, to analyse migration back to Spain, Jacobs studied 170 pieces containing passenger lists for the ships that sailed from America to Seville, and about 20 pieces containing various other material.

By examining this considerable number of sources, Jacobs discovered that 25, 616 emigrants left Spain for America between 1598 and 1621: 18,904 emigrated legally (3,158 of whom went to America for the second time after having gone back to Spain for a certain period) and 6,712 emigrated illegally (6,185 soldiers and sailors who escaped in the American ports and 527 stowaways according to the proceedings of the trials of some ships' captains). If we bear in mind that the impressive work of P. Boyd-Bowman (the USA scholar who has made most progress in this type of reconstruction and in whose wake Jacob's work follows) has documented for the entire sixteenth century the presence of 56,000 emigrants in the New World, the magnitude of Jacob's book becomes fully apparent. The volume, with 88 detailed tables which summarise all the data of this extensive research; contains in the appendix some 22 letters written by emigrants to their relations: Jacobs discovered these letters among the documentation of some of these relations who, encouraged by these very letters, had requested emigration permits.

However, rather than making a detailed examination of the result of Jacobs' research, we would rather dwell upon some general conclusions and some new aspects which are of particular significance for research on European emigration to the New World. A feature of this book is that Jacob's estimate of the migration

flow from Spain to the New World in the early modern age is lower than that of other scholars' estimates. P. Boyd-Bowman is of the opinion that a mere 20%-25% of emigrants left traces in chronicles and archive documentation and that therefore between 200,000 and 250,000 Europeans migrated to the New World in the sixteenth century. Jacobs, on the contrary, calculates that to the 25,616 emigrants for whom he has documentation, not more than 40% of this figure should be added to cover clandestine emigrants or emigrants who do not appear in any records. Jacobs is convinced that between 1598 and 1621 not more than 37,000 people emigrated, both legally and illegally, with an average of 1,500 emigrants per year. By extension, Jacobs claims that between 1492 and 1650 not more than 175,000 people emigrated to the New World (105,000 in the sixteenth century; 37,000 in the period he studied directly and 33,000 between 1621 and 1650). This estimate is well below M. Moerner's estimate of 438,000 emigrants in the period from 1506-1650. Moerner relates the number of emigrants to the number and size of the ships which set sail from Seville for the Indies, figures he takes from the research of H. and P. Chaunu. On the grounds of this reconstruction, Moerner argues that in the sixteenth century more than 240,000 people set sail for America, with an average of 2,400 emigrants per year, and that in the first half of the seventeenth century almost 200,000 emigrants went to America, with an average of 3,900 emigrants per year. Jacobs questions these figures, starting with the obvious assumption that ships did not always sail with their full capacity of passengers. To prove his point, Jacobs presents us with a number of considerations stemming from the direct analysis of documentation concerning the number of voyages, the size of the ships and the ratio between the size of the crew and the ship's tonnage. These considerations are extremely useful in reconstructing voyages between Seville and America.

What Jacobs questions about the estimated number of Spanish migrants put forward by P. Boyd-Bowman, and M. Moerner, and also by R. Koneztzke, R. Carande and J. Friede, is the fact that they all assume that there was a very large number of clandestine emigrants. Jacobs claims that the estimates of the above-mentioned historians implicitly assume that emigration control by the officials designated to this task was totally insufficient. According to Jacobs, the situation was vastly different: *Contratación* control was efficient; it had its shortcomings of course, but certainly not to the extent that legal emigration accounted for a mere 20%-23% of the overall number of emigrants. Therefore, according to Jacobs, clandestine emigration cannot be ascribed to a presumed laxity on the part of the officials in charge, who failed to carry out the necessary controls, so much as to what he defines as *emigración laboral*, "emigration through labour": soldiers and sailors from the ships who, as has been pointed out, deserted when the ships docked in America. At most, in addition to the recorded departures, there could be others, both Spaniards and other nationalities, who may have set sail from ports such as Lisbon, London, Bordeaux, La Rochelle and Amsterdam and, via Brazil and the French, English and Dutch colonies in the Caribbean,

reached the Spanish dominions in America: we need only remember the growth of the community of Jewish converts who emigrated from Lisbon to Mexico City.

According to Jacobs, those historians who suggest that there were large numbers of emigrants to the New World do not give due consideration to the huge expenses that had to be borne by every emigrant. Jacobs maintains that the travelling expenses - already, indeed, hefty in themselves, but even heftier for civilian clandestine emigrants, who were neither part of the ships' crews nor of their militia - constituted a stumbling-block to Spanish emigration to America. For Jacobs, the emigration flow cannot have been as great as some estimates would have it, because it was a free movement in which emigrants, with their own means, had to look for somewhere that could offer them more than Castile could. In America there was no labour market into which emigrants could become integrated: unlike in the other European colonies, in Spanish America the authorities had not created a plantation economy in which they could employ Spaniards: they preferred to use slaves for this work, firstly Indians and then blacks. Only those with an administrative or an ecclesiastical office and their entourage could count on concrete prospects for the future.

As a consequence of his considerations on the number of Spanish emigrants to the New World, Jacobs challenges the idea that the depopulation of Castile was due to emigration to America. He argues that only in a very few cases can emigration have had a negative influence on Castile's population growth. Moreover, the probable negative influences of emigration to America on population growth in Spain were further diminished by a considerable return movement, which scholars have paid only scant attention to until now: they have focused almost exclusively on the out-going migration flow. Jacobs is the first person to attempt to calculate the size of the return flow, and this is one of the novelties of this excellent book. After having examined the lists of passengers returning from the Indies, Jacobs finds documentation for 3,694 people who returned to Spain. From that figure, taking into account the overall number of return voyages, Jacobs estimates that some 6,700 people returned to Spain during the reign of Philip III. For about half of them, it was a temporary return, because during the period studied 3,158 people set sail a second time for America. In conclusion, the return flow represented almost 17% of the whole emigration movement, although only half of those who returned to Spain actually remained there.

The other book reviewed, *Transatlantic Ties in the Spanish Empire* by Ida Altman, is vastly different. It is a book of social history which focuses on the local impact of emigration, aiming to demonstrate the repercussions of emigration from Europe to the New World on both the society the emigrants left behind and the society they had emigrated to. Unexpected aspects of Spanish emigration emerge from this book. Ida Altman had already published research on emigration from a small specific area. In *Emigrants and Society. Estremadura*

and Spanish America in the Sixteenth Century, (Berkeley, 1991), by analysing the case of Trujillo and Caceres in Estremadura, Altman showed that emigration was closely linked to the local community's socioeconomic and family models and its deeply-rooted structures, and that the movement of people from these communities had a strong impact on the society they left, while also contributing to the formation and development of society in America.

In this new book, Altman examines emigration from a small town in New Castile to a town in Mexico. Between 1550 and 1630, about 1,000 people left the town of Brihuega to go to Mexico, settling for the most part in Puebla de los Angeles, the second most important town in Mexico, and the most important for textile products. Here the Spanish settlers were a crucial factor in the development of local industry. Altman compares the society of the towns the emigrants left and that of the towns in which they settled, focusing on the modes of settlement and the circumstances of the move (chapter 1); economic life in the Spanish town of Brighuega and the Mexican town of Puebla (chapter 2); public life (chapter 3); religious life (chapter 4); family relationships and marriage strategies (chapter 5); social relationships (chapter 6). By showing how people grouped together, and how economic and family connections were formed among those who emigrated from Brihuega to Puebla, this new research on the implications of emigration in the early modern age provides further evidence of how local patterns of Spanish society were transported and adapted to the Indies. Even though with a different emphasis, this book, like that on the Estremadura emigrants, shows "the interrelatedness - indeed, the inseparability - of socioeconomic and cultural patterns in the early modern Hispanic world and the need to identify and examine these patterns in their proper context; that is local society" (p.193).

Brihuega was a medium-sized town, with a population of about 4,000. In the Middle Ages, it had been an important textile centre, but by the second half of the sixteenth century the textile industry was on the decline. These circumstances encouraged a considerable emigration flow: many inhabitants of Brihuega moved to other towns in the region or else emigrated to the Spanish colonies in America. Other reasons prompted the inhabitants of Brihuega to leave their town. For many years Brihuega had been part of the Archbishopric of Toledo, but about 1570 the town was removed from episcopal jurisdiction and the citizens had to choose between the lesser of two evils: becoming part of an unknown seigniory or purchasing their freedom, thereby beginning a regime of self-government. Self-government was reached, but revealed hitherto hidden rifts and antagonisms, especially between old and new Christians. This difficult situation contrasted with the success of those who had taken a chance and emigrated, either to the surrounding region or to the New World, mainly to Puebla de los Angeles, a town which was growing rapidly. Emigration began about 1550, and reached its peak some twenty years later, when many of the first wave of emigrants went back to Spain to collect their families and set off again for America together. During the following forty years, about 600 people

left Brihuega to emigrate to Puebla. In this period, the composition of the group of emigrants changed: the percentage of young bachelors remained high throughout the sixteenth century, but later it fell drastically.

Considered in the broader context of the movement of people from Castile to the Indies between the sixteenth and the seventeenth centuries, the number of emigrants from Brihuega to Puebla was unusual both for the size and the cohesion of the group. This community was sufficiently large to enable it to maintain its own identity for a long time, while at the same time contributing to the development of Puebla. However, according to Altman - who bases her work on this premise - this community's experience was not at all abnormal, but one which threw significant light on the whole history of Spanish emigration to the New World, revealing some little-known aspects. Emigration from Brihuega to Puebla, which Altman examines, brings out an unusual aspect of Spanish America's peopling and colonisation: this book reveals a society which is much less aristocratic than that we are used to seeing in the Spanish colonisation of the New World, a society where there was in force a work ethic of the sort generally attributed to the colonisers of the non-Hispanic part of America: "the story of the Briocense immigrants in Puebla was very much a tale of success achieved through hard work and collective effort that unified the community" (p.190). The Puebla community was made up of families and individuals of different class, status and wealth, but the connections which united the immigrants had the effect of lessening the economic differences between them. The Brihuega immigrants generally offered their fellow townsmen on arrival work, credit or a marriage, and in return they expected them to contribute their labour and their knowledge.

The model of emigration which emerges from this book is vastly different from the Estremadura model. At Caceres and at Trujillo, the decision to emigrate was generally due to the work of a few powerful individuals who recruited people for their enterprise: Nicolàs de Ovando at the beginning of the process of penetration into the New World, Francisco Pizarro some twenty years later, and Gonzalo de Las Casas, the son of one of the first conquerors and *encomenderos* in Mexico. The fact is that in the case of Estremadura we have a confirmation of the traditional vision of Latin-American historiography, where the conqueror was not only the protagonist of the military enterprise but also the organiser of colonisation. On the contrary, the Brihuega emigrants were peasants, artisans and entrepreneurs who did not have powerful connections in Mexico or at court. Nor had they taken part in the conquest. Although a few held public office in Puebla or had been part of the municipal administration in Brihuega, no one was a top official, a high-ranking prelate, or an *encomendero*, and only a few were *hidalgos* who, once they had emigrated, behaved no differently from the others; they too working in connection with the textile industry.

This aspect of Spanish emigration to the New World is in many respects new. Altman owes a lot to Enrique Otte who in 1966 published a series of "Cartas privadas

de Puebla del sigli XVI" in the *Jahrbuch fuer Geschichte von Staat, Wirtschaft und Gesellschaft Lateinamerikas*. We are indebted to this great scholar who, among the many results of his hard-working life spent researching in the *Archivo General de Indas*, presents us with the portrait of a coloniser: an ordinary man who - for better or for worse - without any epic dimensions, goes to America to secure for himself a better existence with the labour of his own hands so that he can then get his family to join him. Taking her cue from these letters and in order to achieve the noteworthy results of this book, Ida Altman has undertaken a huge amount of research in more than a dozen Spanish and Mexican archives: general archives (Archivo General de Indias, Archivo General de Simancas, Archivio Historico Nacional and Archivio General de la Naciòn), local archives (in Brihuega and in Puebla) and notarial, judicial and parish archives on both sides of the Atlantic.

Francesco D'Esposito
University of Chieti

M.LeMay-E.R. Barkan (eds.), *U.S. Immigration and Naturalisation Laws and Issues*. A documentary history, Greenwood Press, Westport Conn.-London, 1999, pp.XLVI-336.

This book is the worthy and not separable companion of another work published around twenty years ago by E.P.Hutchinson.[1] Both studies deal with U.S. immigration. Hutchinson reconstructs the main issues in the debate which went on both inside and outside the American Congress on the measures proposed to tackle the complex problem of immigration. The present work, on the other hand, is based on the approved documents.

There is nonetheless a chronological difference. Hutchinson's book begins with the 1797 debate in Congress and ends with 1965; Le May-Barkan's study goes up to 1999 and recalls that, even before the first Alien Act of 1798, various provinces had approved measures on the subject of immigration, although the principle of free immigration and free entry prevailed. One of the provinces with the highest number of measures was Massachusetts (1700, 1722, 1731, 1756).

Another difference concerns the way the documentation regarding the legislation on immigration has been arranged. Hutchinson divides immigration legislation into eight sections reflecting the different stages in the immigration policy adopted by Congress. The phase ending on the eve of the War of Secession, which was basically tolerant about the question of entry into the Confederation, was followed by another which sought to organise federal

[1] *Legislative History of American Immigration Policy 1798-1965*, (University of Pennsylvania, Philadelphia, 1981), pp.XV-685.

control on all arrivals (1861-1880). The following years up to the eve of the first world war were those which saw the development and perfecting of the system of controls. This was followed by a period of restrictive regulation (1913-1929) which prevailed during the Great Depression. The second world war and its aftermath marked other phases which witnessed a more open policy for immigrants and which for Hutchinson ended with the Maccarran-Walter Act.

The periodisation adopted by Le May-Barkan, on the other hand, is quite different. The entire documentation is divided up into four not eight sections. The first goes up to 1880 and is considered the colonial period which was characterised by unlimited entry into the country. Another phase is defined by unlimited immigration but limited naturalisation and goes up to 1920. The third is brimming with restrictions and rules for political refugees and with reform. Finally the last section deals with the immigration problem.

From what we have said it is clear that anyone studying the history of immigration to the United States needs to consult both of these books, which go together to form a complete picture of immigration policy. These are two very valuable study tools which are indispensable for anyone who intends to research into either the history of the United States and those countries which drew on emigrant flows for their population growth and economic development, or the history of immigration policies in general.

Luigi De Rosa

R. VAN UYTEN, *Production and Consumption in the Low Countries, 13th-16th Century*, Ashgate-Variorum, Aldershot-Burlington USA-Singapore-Sidney, 2001.

This is a collection of 13 essays which were previously published between 1962 and 1982 in several Dutch and foreign journals and which concern the history of various aspects of production and consumption patterns in different regions and towns in the Low Countries from the thirteenth to sixteenth century. In particular, they deal with Bramant, Flanders, Burgundy, Bruges, Ypres etc., but they also dwell on questions regarding the whole country. Overall, the thirteen essays reflect van Uyten's cultural interests which have gradually changed focus over the years.

At first, his research centred on productions trends and quantitative history but subsequently it focused on comsumption. As van Uyten himself admits, these are two aspects of the same phenomenon. Demand has two sides to it: it stems from the need to conserve more but also stimulates greater production to meet the increased consumption needs; production and consumption are therefore indissolubly linked to one another. Nonetheless, while promoting production, demand brings into play other factors and interactions and there

are many variables which affect production. These concern the size, quality and organisation of the work force, the level of technological development, the organisation of trade, the availability of fixed and circulating capital, the relative stability of the currency and its purchasing power, the quality and rate of taxation, the efficiency of adminstrative institutions and procedures, political stability or instablility, the conditions of peace or war, the national and international conjuncture, external factors (climate, catastrophes, competitiveness and progress in other products). There is an equally large number of social variables since consumption is also affected by mentalities, fashion, literacy levels and the efficiency of social-welfare facilities.

In his work van Uyten has taken account of most of these factors. The essays included in the book show, in the majority of cases, the complexity of the forces at play within each sector, period and region and provide a stimulating insight into the Low Countries' economic and social history, which is all the more interesting if we consider the time span under study.

We may also add that van Uyten has sought to give the book an overall cohesion, arranging the essays not in the order in which they first appeared but according to subject: first come those dealing with production and consumption in general terms, reflecting the country's economic and social structures, then the essays on the cloth industry and finally those on drink consumption. Moreover the author has sought to overcome the essays' somewhat fragmented and disconnected time span with a very good introduction which unifies them. We may therefore conclude that, although this is not a social and economic history of the Low Countries, the book is a plausible picture of the country's economic development in the period under study. I use the word plausible, confident that the work will arouse new interest in research and/or in the debate about one of the most important phases in the origin of the Low Countries' economic and social growth.

Luigi De Rosa

P. ZABALA AGUIRRE, *Las alcabalas y la Hacienda Real en Castilla*, Universidad de Cantabria, 2000, pp. 349

Amongst the taxes collected in Castile during the sixteenth century, the famous *alcabala* was the most important. This helps to explain the interest that many distinguished scholars have always shown in its study. For example, the *alcabala* has been analyzed, from a global perspective, by R. Carande, M. Ulloa, Ruiz Martin and Dominguez Ortiz in their classic works on the Castilian fiscal system. From a more specific approach, other historians have studied different aspects of this tax. Its political meaning has been the focus of the book by J.J. Fortea on the *alcabala*, the cities and the fiscal policy of Felipe II[1]. Other works have studied its collection in some districts, like Córdoba (J.J. Fortea), Alava

(L.M. Bilbao), and Burgos (P. Zabala), and to these we should add the frequent references to the *alcabala* we can find in the regional studies of authors such as B. Bennassar, A. Marcos, Martín, F. Chacón, A. García Sanz, J. Montemayor, J. Gelabert, D. Ringrose, B. Yun or L. Lanza.

The main objective of *Las alcabalas y la Hacienda Real en Castilla* is to increase and complete our knowledge of the *alcabala* through a quantitative approach which has been relatively uncommon so far. The book is focused on the way the *alcabala*'s burden was distributed amongst Castile's tax districts *(partidos)* during the second half of the sixteenth century, the methods used to collect it, and the fiscal policies adopted by the most important Castilian cities, in order to have a better understanding of the influence of this tax on the Castilian economy.

The *alcabala* introduced in 1342, consisted of the right to levy 10% of the sale price of every product, although there were many exceptions. For collecting it, the most common method had been, since 1536, the *Encabezamiento General,* an agreement between the King and *Cortes* (the Castilian national assembly, made up of eighteen royal cities) in which the former gave up his right to levy the *alcabala* in return for a fixed amount of money paid by the Kingdom annually. The amount of the *encabezamiento* was divided amongst the eighteenth royal cities, and then each one of these cities distributed portions to the various towns in their districts. In the last stage, each town divided its portions amongst the guilds, merchants and surrounding villages. As we can see, this method gave the Castilian cities a high degreee of control over the tax, but the King always preserved an important way of overseeing the Kingdom: the *Averiguaciones.* These were inquiries made in all places where *alcabalas* had not been alienated to the nobility in order to know how the cities arranged the collection of the tax (and it is interesting to remark that P. Zabala Aguirre shows that nearly 40% of the *alcabala* paid by the Castilians was in the nobility's hands). As is easy to suppose, the *Averiguaciones* provide an important source of information for the study of the tax, and, in fact, the *Averiguaciones* of 1557-71, 1579-84 and 1590-95 have been the basic documentary source used by the author.

It is well known that, during the sixteenth century, the country's economic gravity centre changed from Old Castile to New Castile and Andalusia. This trend affected the *alcabala*: as Zabala Aguirre shows, the part of the *Encabezamiento General* paid by Andalusia/New Castile grew from 56.8% in 1557 to 66% at the end of the century. If we consider that the *alcabala* was an indirect tax, and both regions were the most urbanised areas of the country, this result is not surprising. And is not surprising either to discover, as Zabala Aguirre does, that the increase in the burden of the tax (measured in nominal terms) affected mainly the cities in the southern half of Castile. In general terms, therefore, it can be said that during the second half of the century, the impact of the tax fell on the

[1] Fortea, J.J., *Monarquía y Cortes en Castilla. Las ciudades ante la política fiscal de Felipe II,* (Vallabolid, 1990).

urban areas of the centre and south of the country; in cities such as Seville, Madrid, Jerez de la Frontera, and Toledo. Of course, as the author correctly points out, this picture has many qualifications, but it is sound and it deserves to be considered as one of the most important results of the book.

How did the *alcabala* affect the country's economy?To give an answer, Zabala Aguirre has used demographic, price and agricultural output data.The former give us a more complex and accurate view of the *alcabala*'s impact. Measured in *"per vecino"* terms. Andalusia and New Castile were again, the heaviest taxed regions. However, there was a clear contrast between the 2,880 mrs paid by an Andalusian *vecino* in 1590-6 and the 1,317 mrs paid by his new Castilian counterpart in the same period. The last sum was only slightly higher than the *"per vecino"* contribution in Extremadura and Old Castile, as a result of the demographic growth of New Castile, which matched the rise of the *encabezamiento* in the region.

When price and agricultural output data are included in the analysis, two basic conclusions emerge: first, as L.M. Bilbao remarked in 1987[2], the traditional view of a continuous aggravation of the *alcabala*'s burden needs revision. This is not to deny the existence of such an aggravation, but to emphasize the existence of different phases and trends. In particular, it seems that the most important rise was experienced during a few years after 1575, when the *Encabezamiento General*'s amount grew from 456 million *maravedis* to 1,400. This increase caused a well-known political turmoil and, after an intense debate, in 1578 an agreement was reached to cut down the *Encabezamiento* to 1,018 million *maravedis*. This last amount was maintained during the rest of the century. If we remember that this was a phase marked by strong inflationary pressures, then it is possible to argue that the increase in the *Encabezamiento* was less important than has traditionally been thought.

But the book not only intends to give a broad and global view of the *alcabala*. Zabala Aguirre is also interested in the study of the way the cities managed their *encabezamientos*, which was important for Castile's economic, political and social life.

At the beginning of the period it was common to collect a big part of the *encabezamiento*, farming out the *alcabala* paid on foodstuffs, while the rest was collected through apportionments amongst urban guilds and merchants (in the cities) and rural dwellers (in small villages). The rise of the *Encabezamiento General* in the last decades of the century prompted the introduction of changes. Before 1557-61, for example, many products had been exempted from the *alcabala*, something increasingly difficult after that date. In the same way, guilds and merchants had to pay a bigger part of the tax. This would have had a bad effect on the economies of cities such as Toledo, Córdoba and Cuenca whose

[2] Bilbao Bilbao, L.M., 'Ensayo de reconstrucción de la presión fiscal en Castilla durante el siglo XVI', in *Aciendas foralse y hacienda real*, Universidad del País Vasco, 2000, pp. 37-62.

important textile industries were heavily taxed. Finally, one of the most striking changes took place in the rural areas of the country, where a trend in which the *alcabala* was paid directly by the inhabitants of the little villages and towns developed. As a result, in these areas the *alcabala* nearly became a direct tax, levied on the wealth of people, rather than an indirect one.

But there were certain limits the urban oligarchies had to consider when they decided how to meet the increase in the *Encabezamiento General*. One of the main tasks of this social group in the complex field of local politics was to guarantee urban supply. Obviously, the concession of exemptions from the *alcabala* was a good method of doing this, but, at the same time, it was equally clear that a too-generous policy of exemptions could threaten the incomes obtained from the tax. The best proof of this can be found in the case of wheat. In order to ease the supply of this basic product it had been very common to exempt it from the *alcabala* before 1575. After that date, however, wheat began to pay the tax, but it was necessary to preserve its many exemptions. In general terms, therefore, it seems clear that during the entire period urban oligarchies had to balance their desire of obtaining good fiscal incomes from the *alcabala* with the need of preserving urban supplies.

In short, P. Zabala Aguirre demonstrates in her book that the rise of the *Encabezamiento General de Alcabalas* fell on the more urbanized areas in the southern half of the country. The burden of the tax was heavier in the cities than in the small towns and villages scattered all over the country, and there are good reasons to consider that this would have damaged the delicate economies of cities such as Toledo, Cuenca and Córdoba. It is interesting to remark that these findings are in line with the results obtained in other works, like the article by P. Saavedra on the *alcabalas* of Galice, the study of the *servicios de millione* by J.J. Andrés, and the article by M. Sebástian and J. Vela on the fiscal burden at the beginning of Felipe IV's reign[3]. Taken together, these studies open an interesting new perspective for the analysis of the relationship between Castilian taxation and the country's economy. Judged by the *milliones* and *alcabala* standard, it seems likely that the rise in the fiscal burden might well have been less important than has been thought. If this were so, the real problem would lie in the existence of a rigid fiscal system whose impact fell disproportionately on the urban and more dynamic areas of the country, with serious consequences for the Castilian economy.

José Ignacio Andrés Ucendo
Basque Country University

[3] Saavedra, P. *A facienda real na Galicia do Antigo Rexime,* (Santiago de Compostela, 1993); Andres Ucendo, J.I., *La fiscalidad en Castilla en el siglo XVII: los servicios de millones, 1601-1700,* (Universidad del País Vasco, 1999); Sebastian, M. y Vela, J., 'Hacienda real y presión fiscal en Castilla a comienzos del reinado de Felipe IV', in *Poltica y hacienda en el antiguo regimen*, (Murcia, 1993), pp. 553-67.