

# THE DEMAND ELASTICITY FOR WHEAT IN THE 14TH TO 18TH CENTURIES\*

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## RESUMEN

Una muy baja elasticidad de la demanda de trigo –la llamada Ley de King– es una de las suposiciones más aceptadas entre los cliómetras. Pero hay una escasa y contradictoria evidencia que la pruebe. Las estimaciones de Robert Fogel confirman la hipótesis; pero las de Karl Gunnar Persson la rechazan. El propósito de este artículo es resolver este debate a través de una versión mejorada del procedimiento de Fogel, con datos de precios y producción de varios países de Europa Occidental en las edades medieval y moderna. Los resultados confirman las estimaciones de Persson.

**Palabras clave:** elasticidad, demanda, trigo, precios, producción

## ABSTRACT

A very low demand elasticity for wheat –the so-called King’s Law– is one of the most commonly accepted assumptions made by cliometricians. But there is

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little and contradictory evidence to contrast this. In fact, Robert Fogel's estimates confirm the hypothesis whereas Karl Gunnar Persson's reject it. The aim of this article is to shed new light into this debate through an improved version of Fogel's procedure, using price and production data for several Western European countries in the Premodern Age. The results confirm Persson's estimate.

**Keywords:** elasticity, demand, wheat, prices, production, King's law

**JEL classification:** N01, N13, N53

## 1. INTRODUCTION

In 1914, the French economist R.A. Lehfeldt published a little known paper with the title «The demand elasticity for wheat» in *The Economic Journal*. In the first few lines he describes the objective of the article: just like «every engineer's pocket-book contains tables of the entropy of different substances» wouldn't it be desirable if all economists had a «pocket-book with tables of the elasticities of different goods» so that the demand elasticity would abandon «the limbo of abstractions»? In his opinion «the roughest attempt to measure a coefficient of elasticity would be better than none». Lehfeldt himself began the construction of these tables with the most obvious «substance»: wheat<sup>1</sup>.

Today Lehfeldt's plan seems naïve. There are a lot of problems than prevent us from using a single value for the elasticity of the demand for wheat (or any another product) like in the entropy of hydrogen. Firstly, demand elasticity is not constant in the long term. Arc elasticity –an easier estimation– is not the same as point elasticity; it is only an approximation. In general, the elasticity coefficients vary along the demand curve; for instance, in a straight-line demand function they extend from minus infinity, on the price axis, to zero on the quantity axis, covering a range of negative values. Generally speaking, a constant coefficient for demand elasticity requires not only a specific demand function –a rectangular hyperbola–, but constant elasticity coefficients for income and for substitute goods according to the «homogeneity condition», a simple implication of consumption theory. These difficulties (and others such as the «identification problem») explain why economists are reluctant to use the elasticity coefficient as a universal variable, and use it instead as an estimate with a wide margin for error<sup>2</sup>.

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<sup>1</sup> Lehfeldt (1914, p. 212). The final result (on page 217) is interesting: the «world» demand elasticity for wheat for the period 1888-1911 would be – 0.6.

<sup>2</sup> We could interpret the point elasticity, commonly known as the demand price elasticity or simply demand elasticity, as the percentage change in quantity demanded given a marginal change in the price of the commodity, other factors held constant. And arc elasticity as the percentage variation in quantity associated with a percentage change described when passing

Despite these problems, economists have done a significant amount of research on demand elasticity. The results are not entirely conclusive but they point at a range of possible values. Perhaps that is not the best of results but it is better than that of economic historians. Our specific studies on the wheat demand elasticity are few and contradictory. To my knowledge, there are only three works based on historical information (that is, before 1850): one by Giuseppe Parenti published in 1942, one by Robert W. Fogel in 1992 and one by Karl Gunnar Persson in 1996. This lack of research can only be explained by the widely used previous work of George King and Charles Davenant, from which King's Law was derived. This rule postulates that successive one-tenth reductions of wheat output would generate increases of the prices according to the following sequence: 1.3, 1.8, 2.6, 3.8 and 5.5. For example, a bad harvest that provided just 90% of a normal harvest would increase the normal or equilibrium price of wheat by 130%. A bad harvest that provided 80% would increase wheat price by 180%, etc. This succession of number pairs (90%/130%, 80%/180%, etc.) allows us to know several arc elasticities; but the data do not correspond to any simple function. For this reason Jevons, Bouniatian and others tried to find imperfect fittings. In other words, King's Law would not be a theoretical formulation, but an empirical rule; something like the summary of an indeterminate number of observations. In any case, literature assumes that King's Law describes a rigid demand curve<sup>3</sup>.

I suppose that the general acceptance of King's Law has been the main reason why researchers have eluded the search for more accurate elasticity coefficients, moving to topics that were less hairy. For example, William Hoskins (1964 and 1968) studied the presence of high and low price runs on wheat, and Wilhelm Abel (1980, pp. 9-13) looked at the effect of self-consumption on the profitability of three kinds of farms. These findings are valuable but they would be rendered futile if the values for the demand elasticity derived from King's Law were not correct. For instance, Abel thought that a farm family that sold 40% of their harvest would not nearly suffer a 20% reduction in yields. The reduction in income due to the decline in sales (50%) would be compensated with a better price (80%) and therefore the revenues would only drop by 10%. But if the arc elasticity were  $-0.5$  (and not  $-0.39$ , like in this example), the revenues would drop by 21.4%, which could be considered a serious loss<sup>4</sup>.

from one price to another. The criterion followed in this paper is that demand elasticity has a negative sign. This is an obvious consequence of these equations, but it is not always used in the economic literature. For this reason, and given that almost all the values in this paper are between 0 and  $-1$ , when referring to «a low (rigid) elasticity», I mean an elasticity close to 0 (i.e.  $-0.1$  or  $-0.2$ ); and when I refer to «a high (rigid) elasticity» I mean an elasticity close to  $-1$  (i.e.  $-0.8$  or  $-0.9$ ). For every good the sum of the three elasticities must be equal to zero. Philips (1974, pp. 34-38) and Tomek and Robinson (1990, pp. 36-38).

<sup>3</sup> Bath (1963, pp. 118-19); Persson (1999, pp. 48-50); Fogel (1992, pp. 248-49).

<sup>4</sup> Wrigley (1987, pp. 95-99).

But the implications of choosing one coefficient or another go beyond a numerical exercise; they affect our interpretation of the economy of the *Ancien Régime*. In the above cited paper Fogel estimated a very low demand elasticity, even lower than that proposed by King's Law. This would mean that large variations in wheat prices did not imply ample or even medium variations in consumption, but that year by year, approximately, the same quantity of wheat would have been available to feed people. However, that did not prevent the existence of high mortality rates. Obviously, chronic malnutrition, and not mortality crisis or famines, would have been the main cause of mortality in Pre-modern Europe. This problem would have not had any simple solution, and only «advances in agricultural and related technologies that permitted the per capita consumption of food to increase by about 50 per cent» would have solved it<sup>5</sup>. On the contrary, Persson (1999, pp. 23-64 and 131-155) estimates a less rigid demand curve. This implies that Europeans were more or less able to overcome variations in output. This was possible because grain markets corrected the worst deficits. In other words: overcoming the «Malthusian trap» did not require technical advances, but institutional reforms. The final goal would be to get past the crisis of subsistence, and not that of chronic malnutrition.

We can find examples of how different values of demand elasticity affect our knowledge of past economies. As an example we can recall Crafts' (1975, pp. 228-229) critique of Deane and Cole's estimate of English economic growth in the 18th century according to which the actual rate of growth of agricultural product prices (in reality, corn prices) would be:

$$\frac{\dot{p}_{ag}}{p_{ag}} = \frac{\frac{\dot{Q}_{ag}}{Q_{ag}} - \left[ n \left( \frac{\dot{Y}/Pop}{Y/Pop} \right) + \frac{\dot{Pop}}{Pop} \right]}{e}$$

where  $p_{ag}$  is the price of agricultural products,  $Q_{ag}$  is the domestic agricultural output,  $Y/Pop$  is the income per head,  $Pop$  is the population,  $n$  is the income elasticity of agricultural products, and  $e$  is their demand elasticity. Assuming that  $e = -0.6$  and  $n = 0.5$  (which is reasonable, according to the homogeneity condition), Crafts rejects Deane and Cole's estimate. So for the period from 1710 to 1740, the rate of growth of agricultural prices would have been 0.16, when actual rate was  $-0.75$ . But this refutation would not be possible if the demand elasticity –and, consequently, the income elasticity– were lower. Note the effect of  $n$  and  $e$  on the above equation.

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<sup>5</sup> Fogel (1992, p. 280)

In short, we should not simply state that the wheat demand elasticity is rigid. The implications of assuming different «low» values, ranging from  $-0.1$  to  $-0.9$ , are not irrelevant. In the following pages my aim is to find the most plausible estimate. However, I will elude many of the issues connected to this problem. I realize that accepting one value would imply accepting or refuting some critical hypothesis about the *Ancien Régime* economy, much like the controversies between Crafts and Deane & Cole or Fogel and Persson. But defending or rejecting all hypotheses would just add confusion to the line of reasoning of this paper.

After this introduction, this paper has been divided into seven additional sections. In section 2 I will look at the arguments that may or may not sustain King's Law. In section 3 I will revise the calculations of the wheat demand elasticity using historical data. In the next section I will estimate that elasticity by using Fogel's basic procedure. For this purpose, I will study the variability of yields in section 4. Next, I will look into the effects of long-term storage, trade and the seed factor in section 5 and the variability of prices in section 6. In section 7 I will estimate the wheat demand elasticity to conclude in section 8 with the main findings.

## 2. EVIDENCE OF KING'S LAW

Besides the above quoted succession of number pairs, we know little about King's Law. Certainly, it is anecdotic, but we are not even sure about the author. The rule was presented by Charles Davenant, who widely recognized his debt with George King, but neither one of them explained the kind of sources used. We do not know whether Davenant copied or summarized previous research by King or if he used his own data.

But there are more serious problems. Firstly, we do not know if the quantities series are based on gross or net yields. Wrigley noted that the interpretation of King's Law is very different in each case. We can illustrate this problem with an example. We will assume that every year the part of the production used as seed and fodder was fixed. Likewise, I will identify consumption with net production; that is, I will ignore the problems of trade and carry over. If King's Law were built on net production data, the reckoning of arc elasticities would be immediate: I would have got two series of consumption and price. However, if series are based on gross production data, I should remove seed and fodder from it. The next table reflects this problem. We have set out three cases. In the first, King's Law is based on net production. In the other two, it is based on gross. For each decrease of production I can deduce three possible arc elasticities. In the first case –sixth column– production is net and, consequently, is equal to consumption too. The

range of the five arc elasticities from 100 to 50 is  $-0.36/-0.50$ , with an average of  $-0.41$ . In the second case, and following the example proposed by Wrigley, I have supposed that the only fixed cost is the seed and equal to the 25% of a normal harvest. The values in column 7 would be higher and the range would be wider, from  $-0.52$  to  $-0.92$ , with an average of  $-0.64$ . In the third case, the fixed part includes seed and fodder, a 35% of a normal harvest. The range of elasticities would be  $-0.62/-1.37$ , with an average of  $-0.84$ .

**TABLE 1**

EXAMPLE OF ARC ELASTICITIES BASED ON DIFFERENT  
INTERPRETATIONS OF KING'S LAW

Production	Consumption = Net Production			Prices	Arc elasticity		
	(1)	(2)	(3)		(1)	(2)	(3)
100	100	75	65	100			
90	90	65	55	130	-0.40	-0.55	-0.64
80	80	55	45	180	-0.36	-0.52	-0.62
70	70	45	35	260	-0.37	-0.55	-0.69
60	60	35	25	380	-0.41	-0.67	-0.89
50	50	25	15	550	-0.50	-0.91	-1.37

Note: Column 1: Gross Production = Net Production. Column 2: Gross Production = Net Production + Seed. Column 3: Gross Production = Net Production + Seed + Fodder.

A similar exercise was performed by Wrigley (1987, pp. 105-108) to adjust the observations of harvests and prices of 1315 and 1316 collected by Farmer (1977) with King's Law<sup>6</sup>. In fact, these observations reject King's Law if we assume that production is net; but not if we suppose that they do not include seed. But this assumption poses bigger problems to the common acceptance on King's Law, because it implies higher with elasticities. But above all, it implies a demand function with low concavity. Apparently, this does not seem credible. In my opinion, the most reasonable hypothesis is to assume that Farmer's data do not prove King's Law<sup>7</sup>.

No matter with what type of data, it is obvious that King's Law shows more problems in the last pairs of numbers  $-70$ ,  $60$  or  $50\%$  of a normal harvest. This is not surprising. There are reasons to believe that at least this part of the rule was based on weak information. Davenant's book was published in 1699 and,

<sup>6</sup> Wrigley does not estimate elasticities.

<sup>7</sup> See also Bath (1963, pp. 119-209)

consequently, the data had to have been compiled in a period prior to that year; probably during the immediate years before. Registering a price series involves hard work and has only been performed systematically in recent times. But even in the case that all the information on prices and output had been easy to obtain, it does not seem probable that King and Davenant's research time span extended beyond a century. Nor does it seem probable that they used information from countries where they had not lived, or on which they had not written. These limitations reduce the sample to three or four countries: England, Holland, France and, perhaps, Belgium, although the need to use contemporary price and output data suggests that it most probably came from England. To any extent, in these countries, and during the 17th century, there is no price ratio for different agricultural years over 260%; or, at least, I have not found evidence of one. If we use monthly prices instead of annual, and compare the maximum of an agricultural year with the minimum of the previous one, it is possible to find those large differences; but only in one series –Paris– and one time –between March of 1649 and November of 1648 we find a ratio of 450%. In the rest of the series the relations between prices never surpassed 380%. For instance, in the series of prices of Toulouse –which, as we will see below, shows the largest volatility– there are four peaks in March of 1631, January of 1644, March of 1653 and May of 1694. The relations of these maximums with the minimums of 1629, 1642, 1651 and 1692 (in all cases, August) are 340%, 330%, 200% and 240%, respectively<sup>8</sup>.

It seems that all these facts are logical. As we will see below, wheat output in England, Holland and Belgium was the most stable in the world during the first decades of the 20th century. It is probable that 300 years earlier it was less stable but it would not fall under 60 or 70% of a normal harvest either. Surely, the variability of production in France –especially in the south– would be higher but not significantly so. According to Labrousse (1944, pp. 184-5), in the 18th century a bad harvest never would have fallen below two thirds of a normal one. It seems reasonable to suppose the same for the 17th century. Consequently, price increases over 260%, according to King's Law, would have been a very unusual phenomena, more related with political conflicts than with agricultural crisis.

In short, we do not know the kind of data used by King, Davenant or both. Moreover there are strong reasons to believe that the last number pairs of «failure of production/increase of price» –that is, 50%/550%, 60%/380%, and even 70%/260%– could never, or only exceptionally, have been observed. Of course, this does not imply that the two first pairs were incorrect. But it is undeniable that,

<sup>8</sup> In reality, it seems a very exceptional and strange case. Prices in January, February, March and April of 1649 were 12.6, 36, 60 and 17 *livres tournois per setier*, respectively. The next big difference appears between November of 1693 and September of 1692, with 310%. Baulant and Meuvret (1962, pp. 19 and 48). For Toulouse see Frêche (1967).

without any further evidence that proves it, we should be very cautious. Whatever they might be based upon, in the words of Abel (1980, pp. 9-10), statements from King's or Davenant's research «can no longer be verified.»

### 3. ESTIMATE OF WHEAT DEMAND ELASTICITY BASED ON HISTORICAL DATA

King's Law has received wide support from historians without being submitted to a formal contrast. Wrigley pointed out that «[e]conomic historians from Thorold Rogers to Slicher van Bath have also had resort to the ideas of Davenant (and/or King)» which should be read as a tribute. He also says that «his estimates were widely quoted and broadly confirmed by a number of later examinations of the same issue», an argument that others scholars, such as Abel, have also used. However the evidence these studies are based upon is doubtful because they use information from the last century. In most of these studies –but not in all!– the wheat demand elasticity is lower; but this is foreseeable. In general, any food demand elasticity should be low. For example, in the work of David Edgerton *et al.* (1996, pp. 107-121), for Scandinavian countries between 1960 and 1990, only cold drinks in Finland and alcoholic drinks in Norway had elastic demands. In a study presented by Kuo Huang (1985, pp. 1-22) for the United States between 1953 and 1983 only oranges (exactly – 1) and grapes had elastic demands. In a similar study by Stefano and Pieraccini (1965, pp. 193-208) for Italy between 1949 and 1965, they found elastic demand only for oranges. The reason is simple: in modern economies it is unlikely that a rise in the price of a good leads to a fall in its consumption because most of people can afford «a little extravagance». *Ad senso contrario*, it seems logical to suppose that, if the Europeans of the 19th and 16th centuries were poorer than us, then their demand elasticity would be higher.

An alternative would be to calculate the wheat elasticity using data from those contemporary countries that were the poorest. Of course, the work of Amartya Sen (1981, pp. 86-112) is a necessary reference; but it does not support the idea of a rigid curve of demand. For instance, Sen explains how during the Ethiopian famines of 1973 and 1974 food prices did not increase. In reality, he denies that grain markets have to correctly reflect the needs of people. For this reason among many others, f.i. the high degree of market integration even in the poorest countries nowadays, comparisons do not seem possible.

A much more promising approach is to try to contrast King's Law with historical information. By «historical» I mean to say data prior to 1850. It is obvious that conditions in most European economies in the middle of the 19th century were very different from those of the Middle Ages. The improved yields in harvests,



the construction of the railway, the imports of wheat from the United States, and the changes in diet, are some of the factors that could bias the estimate. Since we would not be able to compare the results we can only use three works.

The first one is a book about the Sienna wheat market written by Giuseppe Parenti in 1942. Its main merit is the use of a contemporary sample of wheat consumption (in fact yields) and prices for the period 1560-1667. Parenti's estimate is formally correct, but it is polluted by a number of small numerical errors. For this reason, his result, a coefficient of elasticity of  $-0.915$ , differs from mine:  $-0.794$ . In any case, both estimations are far from the elasticities of King's Law, whose validity he questioned. It is interesting to observe that in a number of the graphs and tables of his book output variations are only slightly bigger than those of prices<sup>9</sup>.

Before going on to commenting on Fogel (1992), I will discuss Persson's (1996 pp. 695-697, and 1999, pp. 52-54 and 64) work on this issue. In the mentioned papers Persson estimates barley demand elasticity in the south of Sweden between 1610 and 1662, and wheat demand elasticity in Bourges and Bourdeaux between 1825 and 1869. These cases are not very suitable to help contrast our hypothesis. Barley was not a grain widely used for human nutrition in Sweden and the region cannot be classified as a «typical» European country. On the other hand, the period studied for France is too recent. However, the Swedish estimate is of extraordinary interest for a different reason. Persson investigated the impact of inventories and trade on price formation. The final value of the elasticity,  $-0.64$ , includes these effects. And, once again, we find this coefficient to be higher than that of the elasticities implicit to King's Law.

It is not true that Fogel was the first to estimate demand elasticities through the relationship between standard deviations. Lehfelddt used this procedure in his 1914 paper, but his work seems to have been forgotten. Explaining this methodology is beyond the scope of this paper, but perhaps it is worth explaining the intuitive idea that supports it. To estimate the wheat demand elasticity we need long contemporary series of prices and quantities. Parenti could find this type of information; but this is usually not possible, because consumption, output, yields or any other similar series are scarce and incomplete which makes matters worse. Moreover, the results would not be entirely reliable because a local case must not be representative for a region or nation. For this reason, the correct way to proceed would be to estimate several local elasticities in the same country. But the lack of documentary sources impedes this.

These problems, one would suppose, drove Fogel to find a different method. The supporting idea could be found in (for instance) the works of Bath or Abel. In the long run, the price of bread suffers more severe fluctuations than other commodities. The reason is simple: bread is absolutely essential. This means that

<sup>9</sup> Parenti (1942, pp. 173-216).

bread displays a rigid demand curve. In other words: a low demand elasticity implies big fluctuations in prices. But fluctuations are «big» with respect to what?

The intuitive answer is «in relation to consumption»; that is, production (later we will explore the validity of this identification). Consequently, if we have two representative samples of prices and quantities for the same geographical area, even if we have obtained them from different places, we could estimate the demand elasticity through the comparison of their oscillations. In other words, if the oscillations of prices are larger than those of production, demand elasticity should be deemed rigid. And it will be more rigid when the oscillations in production are bigger than those of prices.

Fogel (1992, 248-255) –and before him Lehfeldt– draws up this idea. The relationship between the standard deviation of quantities and that of prices would be an approximation to that elasticity:

$$\varepsilon = \frac{\delta_Q}{\delta_p} r_{Qp}$$

Where  $d_Q$  is the standard deviation of the quantities of wheat consumed,  $dp$  is the standard deviation of wheat prices, and  $r_{Qp}$  is the correlation coefficient of the two. Obviously, if the price and quantity series are not contemporary, and have been obtained in different places, it does not make sense to use the correlation coefficient. For this reason Fogel, as well as others, simply estimate the demand elasticity through the relationship between standard deviations of quantities and prices.

It is important to note that standard deviation could be exaggerated if the original series had a trend. For this reason it is necessary to make a previous transformation of the data. Fogel takes deviations proportional to the trend. This is a possibility, but there are others. For example, we can take moving averages or logarithms. The last procedure is rougher, but it has an advantage: transformation by logarithm is the most usual method to elude the «heteroskedasticity» of the series, that is, the presence of irregular variations in some parts of them. In general, wheat price series are heteroskedastic in their high values.

But whatever method we use, it is not reasonable to expect large differences between estimates. Therefore, it is surprising that Fogel's estimate of  $-0.18$ , is much lower than that of Parenti ( $-0.79$ ), Persson ( $-0.64$ ) and even King/Davenant (approx.  $-0.4$ ). In this section I will explain that the discrepancies lie partly in a numerical error, partly in the peculiarities of the English economy at the end of the 19th century, and partly in the use of incomparable figures.

Fogel does not calculate the standard deviation of wheat prices. He took it from an earlier work by Ronald Lee (1981, p. 374) dealing with demographic questions. His series covers three centuries, 1544 to 1834, has four origins and is annual until 1649, and monthly from then on. With the method described by Fogel

one needs to refine the trend component of this series. Lee uses different transformations in the annual and monthly data –in the latter, the seasonal trend needs to be eliminated– and does not clearly state what he does with his mixed series. This omission may be unimportant. I have reconstructed the annual series of prices described by Lee, eliminating their trend, just as he does –mobile averages of 11 years– and I have calculated the standard deviation. The result is identical to his: 0.22. And it is also the same if we employ the Eton series obtained by Abbott Usher (1931, pp. 112-113). In short, I presume that Lee used a series of annual prices. However, if this was not the case there would still be little difference.

The numerator of the formula, that is, the standard deviations of consumption, is calculated by Fogel from the data of yields in Great Britain between 1884 and 1913. The underlying idea is that the intensity of the oscillations in yield would have been the same in all periods and, because of the existence of high transport costs, this would reflect that of consumption in the Middle and Premodern Ages. In a footnote he argues that wheat yields in the United States in the decades 1960 and 1970 have a similar variability to those of 1871-80. In my opinion, this fact alone does not support Fogel's hypothesis, and in particular it does not support it in Great Britain. In 1884 English cereal farming had already suffered more than ten years of fierce competition from North American grains. Since 1874 the area cultivated in wheat had fallen by more than 25% and would be reduced to much less than half this figure towards the beginning of the century. Probably the land that continued to be sown with wheat was the most apt, at the same time presumably being those lands with the most stable agricultural yields. For this reason, it would not be farfetched to think that the yields of 1883-1913 were more stable than those of the decades 1960 or 1970.

Foremost there is a numerical error. Given that calculating mobile averages of 11 years in series that only cover three decades implies doing without a third of the data, Fogel calculates «proportional» or «percentage deviations from trend». The standard deviation of this series would be 0.04 and, therefore, the demand elasticity would be  $-0.18$  ( $0.04/0.22$ ). I have repeated all these calculations with the same series of yields, and the result is double, 0.082. Even using mobile averages of five or eleven years a similar result is obtained. Therefore, following his procedure faithfully, the demand elasticity for wheat in Great Britain would be  $-0.37$  ( $0.082/0.22$ )<sup>10</sup>.

But comparison with other countries and periods confirms that this figure is still too low. Given the availability of sources, we can repeat the same exercise with a number of medium sized and large European countries: Germany, Spain, France, the Netherlands and Italy, as well as, of course with Great Britain. Just as Fogel did, I will use data for yields in the period 1884-1913. However, the existence of good statistics in France allows me to extend the calculation to two further

<sup>10</sup> Mitchell and Deane (1962, pp. 90-91).

periods, marked by certain changes in the way areas were measured: 1815-1851 and 1852-1883; on the contrary, the deficiencies existing in Spanish and Italian statistics do not allow me to make calculations before 1890. All these figures are official, and are taken from Mitchell (1998). A non-official and little known source is the estimation made by Sir John Bennet Lawes (1904/05, p. 38, and also others years). I include this, despite the fact that it refers to Great Britain and Northern Ireland together, because it covers the period 1852-1883.

In order to avoid complications, I will use three different transformations of the data. In the first, I will use Fogel's procedure, that is, percentage deviation from the trend. In the second, I will use Lee's procedure, a moving average, but only of nine years. In the third, I will use logarithms.

**TABLE 2**

VARIABILITY OF YIELDS IN EUROPE. 1815-1913

Country and period	Trend	Moving average 9	Logarithms
G.B.-Ireland 1852-1883	0,17	0,17	0,18
G.B. 1884-1913	0,08	0,08	0,09
France 1815-1851	0,10	0,10	0,14
France 1852-1883	0,13	0,16	0,16
France 1884-1913	0,10	0,09	0,12
Germany 1884-1913	0,06	0,06	0,13
Netherlands 1884-1913	0,09	0,08	0,13
Italy 1890-1913	0,10	—	0,14
Spain 1890-1913	0,15	0,15	0,17

Sources: Mitchell (1998) and Lawes (1904-05, p. 138)

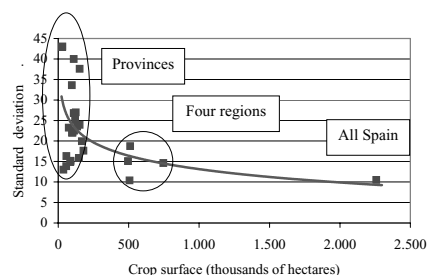
As can be seen, the differences between the first two methods are negligible except for France 1852-83; although the differences with the other method are not big either. Probably, these differences are a consequence of the transformation by logarithms, due to the incomplete elimination of trend. It is significant that the second lowest standard deviation is for Great Britain in 1884-1913. Standard deviation in Great Britain-Ireland for the period 1852-1883 is almost double that for 1884-1913; with these data demand elasticity would be  $-0.77$ ! Wheat production in the Netherlands has a comparable (although higher) variability in relation to Great Britain; indeed Dutch agriculture suffered similar problems to those faced in Britain. It seems clear from this that the sample used by Fogel is not representative of a normal situation in Europe in the same period.

But it could be more interesting to analyze the low variability of production in Germany. It seems to contradict with the rest of the results given the strong protection of its agriculture. In fact, the determining element is not commercial policy, but the sheer size of the country. In this period the total surface area of Germany was twice that of Great Britain. This implied a low variability because the deficits of some regions could be compensated with the excesses of others. Note that it is not relevant whether this would really happen; it is only a statistical not necessarily a real effect. Extrapolating this would be the same thing as to say that world wheat production variability was close to zero.

Obviously, the world is not a suitable unit to calculate wheat demand elasticity. But probably a national sample is just as inadequate. As we will see later, a national sample would reflect a real case had we, amongst other things, perfect national market integration. The important point here is to observe that the choice of large samples generates low variabilities. The next graph shows the relation between variability in production (measured through standard deviation of moving average series) and surface area in Spain between 1905 and 1926. This country is a good field for study because production suffered sharp changes, and there were two kinds of climatic alternations: east-west and north-south. Data have three levels of disaggregation: national (obviously, one figure; not all the country but only the sum of cereal regions), regional (four) and provincial (22; I have excluded provinces with low production and two regions with mixed features). The lowest variability is for one of the regions, Ebro; but it is practically the same as national variability. Provincial variability was usually higher than regional variability; so, only seven provinces have a higher variability than the most irregular region, Guadalquivir<sup>11</sup>.

### GRAPH 1

#### SPANISH REGIONAL CEREAL SURFACES AND VARIABILITY IN PRODUCTION



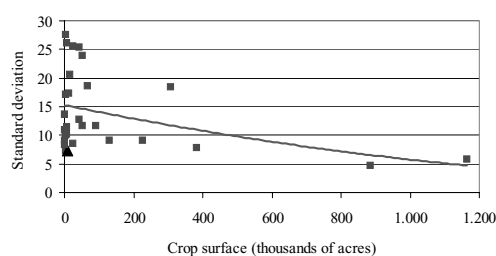
Source: GEHR (1991)

<sup>11</sup> GEHR (1991). These regions are La Mancha –Albacete, Alicante, Cuenca, Murcia and Valencia–, Ebro –Huesca, Lérida, Navarra, Teruel and Saragossa–, Duero –Ávila, León, Palencia, Salamanca, Segovia and Valladolid– and Guadalquivir –Cádiz, Córdoba, Huelva, Jaén and Seville.

Graph 2 shows a different level of aggregation. It has been constructed with production data and crop surface area –standard deviation of moving average series– from 25 countries and three conglomerates –the World, the World except Russia and four major exporters– between 1890 and 1932 –with a deliberate lag in 1914- 1919–. Russian crops show high volatility. This, however, is not a normal case. On the contrary, we find low variabilities –standard deviation lower than 10– in the three conglomerates and the US. Variabilities in medium and small countries show more dispersion. We find standard deviations that are lower than 15 in India, Chile, New Zealand, Japan and all the Western European countries except for Portugal. Standard deviations greater than 20 are found in countries with extensive crop systems such as Argentina or Australia, and in small non-European countries as Algeria or Tunisia. The highest variability was found for Uruguay, a country that meets the two conditions. But the more significant fact is that one of the lower variabilities, and the lowest (together with Belgium) among individual countries, is for the British Isles (big cone)<sup>12</sup>.

## GRAPH 2

### WORLD AND NATIONAL CROP SURFACES AND VARIABILITY IN YIELDS



Source: Bennett (1933, pp. 264-274)

We can derive two conclusions from Graph 2. Firstly, during these years, covering the period used by Fogel's estimate, Great Britain displayed an unusual stability in its cereal production. Second, production variability was dependent on country size. Variability of yields in a country was greater than that of the conglomerate it belonged to; but variability of yields in a region was greater than that of the country it belonged to; and variability of each locality was greater than that of its region. It follows that variability of each farm was greater than that of

<sup>12</sup> Bennett (1933, pp. 264-274). These countries are Russia, USA, Canada, Argentina, Chile, Uruguay, Australia, New Zealand, India, Japan, Algeria, Tunis, France, Germany, Italy, Belgium, British Isles, Netherlands, Switzerland, Denmark, Sweden, Bulgaria, Romania, Portugal and Spain.

its locality. Fogel chooses a national sample; it would be the correct election had he chosen a national price sample to match it with. But really he chose a local sample. The figures are not comparable.

In short, the corrected estimate of  $-0.37$  (not  $-0.18$ ) would be recalculated using a local sample of yields for another period. Most likely, the demand elasticity would be higher and close to the results by Parenti and Persson.

#### 4. THE VARIABILITY OF WHEAT YIELDS IN EUROPE

Indeed, there is a more evident criticism to Fogel's work: why did he use 19th century data if he could have used contemporary ones? There are several sources for the study of yields in Europe. The most important source, despite it being limited from the 16th century on to the Catholic regions of the continent, is the information of tithes. The collection of data has been especially fruitful in France, for which we have the works of René Baehrel (1961, pp. 615-616), Emmanuel Le Roy Ladurie (1966, pp. 842-848), and, above all, Goy and Le Roy Ladurie (1972), which bring together the contributions of several authors. This source must be used with a certain amount of caution. An initial problem lies in the existence of gaps. When these gaps are of only one, two or three years, I have ignored them, but I make interpolations to estimate the seven year moving average –i.e., I use these data in the denominator. When the gaps are greater, I have divided the series into samples, and worked with each of them. Some series have «suspicious» repetitions of figures, when it is highly improbable, not to say impossible, that the same harvest was brought in during two consecutive years –of course, I have not included figures referring to leasing of tithes.

On the other hand, since these are taxation data, we can expect there to be a greater amount of fraud for years of very bad harvests than for others. For this reason I have ignored those years for which the receipt was very low. It is difficult to determine if this decision was correct. If fraud did not exist, the suppression of these data probably implies an estimate of the demand curve that is less rigid than the real one. We can expect the elasticity values on the left hand of the curve (close to price axis) to be higher than the others. But if fraud did exist, the inclusion of that data would imply the opposite: the demand curve would be more rigid than the real one. These considerations have moulded my criteria to accept or reject doubtful data. In short, I have rejected the lowest figures. For instance, in the series for Agdi, Languedoc, I have eliminated the figures for the years 1697 and 1709, according to which the harvest would have been 38% and 21% of their centred moving average of seven years. On the other hand, I have maintained figures for 1639 and 1699, which imply harvests of 64 and 56% of their moving averages. As we saw above, Labrousse did not think that in the 18th century a

bad harvest would fall below two thirds of a normal one, so two extremely bad harvests like those could have been possible in the 17th century, especially in this region of France. But a harvest below 40% is not credible.

TABLE 3

VARIABILITY OF TITHES IN FRANCE AND GENEVA. 14<sup>TH</sup> – 18<sup>TH</sup> CENTURIES

City	Period	Trend	7 year mov. av.	Logarithms
Alsace	1484-1632	0.14	0.13	0.16
	1646-1790	0.18	0.10	0.37
	Average Alsace	0.16	0.11	0.26
Arles	1599-1788	0.27	0.25	0.31
Beaume	1597-1788	0.15	0.11	0.16
Cambresis	1446-1477	0.06	0.03	0.07
	1502-1525	0.09	0.04	0.09
	1540-1578	0.15	0.11	0.18
	1601-1635	0.06	0.06	0.07
	1700-1728	0.14	0.08	0.16
	Average Cambresis	0.09	0.06	0.11
Geneva	1550-1589	0.18	0.16	0.20
	1595-1780	0.18	0.17	0.20
	Average Geneva	0.18	0.16	0.20
Languedoc	Agde 1593-1723	0.18	0.16	0.20
Breziers	1577-1753	0.18	0.17	0.20
	Average Languedoc	0.18	0.16	0.20
Toulouse	1764-1787	0.26	0.25	0.21
Average France and Geneva		0.18	0.15	0.21

Sources: Le Roy Ladurie (1966, pp. 842-848); Goy and Le Roy Ladurie (1972); and Baehrel (1961, pp. 615-616)

In addition to this, we have information on yields themselves, of different origin and methodology, which, on the whole, seem more reliable than that from tithes. For the case of England we have two series. Firstly, the estimations made by David Farmer (1977, pp. 557-558) for several estates in the Winchester area in the Early Middle Ages. These are not data for units of surface area cultivated but rather for units of seed, once the following year's seed has been deducted; it is therefore an expression of the surplus. Secondly, the estimations of two businessmen from Liverpool on the wheat yields in two «circuits» which ran, from the city, through



the south-centre and east of England. The figures have been taken from Healy and Jones (1962, p. 578), who state that, although there may have been a general overestimation, «the yearly changes are probably quite accurately determined». The sample is long, from 1815 to 1859. Nevertheless, I will not use it in calculating the demand elasticity for two reasons: it aims to reflect the production of a whole nation, and it refers to a recent period.

For France we have the yields obtained by Joseph Goy (1972, pp. 250-253) in Arles. This series covers a very long period –1621-1735– with a single important gap between 1649 and 1660. The greatest problem for its application to France is that Arles can scarcely represent the oscillations in production of the Atlantic coast, which is where the greatest part of the wheat was harvested. Likewise, we have two series for Brussels obtained by Marie-Jeanne Tits-Dieuaide (1975, pp. 291 and 317), one of yields for seeds, like the previous, and another of area yields. Its main advantage is that the climatic characteristics of Belgium more closely resemble those of Atlantic France.

For Italy we have three series. Firstly, there is an estimation of seed yields in the city of Parma between 1509 and 1539. We also have a series of wheat production in the territory of Parma between 1784 and 1805. Finally, I have included the estimations of production in the valley of Asciano performed by civil servants from the nearby Sienna between 1560 and 1667<sup>13</sup>.

**TABLE 4**  
VARIABILITY OF YIELDS IN EUROPE. 14TH – 18TH CENTURIES

City	Yield	Period	Trend	Mobile av.	Logarithms
<b>Arles</b>	Seed	1621-1648	0.21	0.16	0.21
	Seed	1661-1735	0.26	0.24	0.29
<b>Brussels</b>	Seed	1450-1493	0.25	0.21	0.26
	Surface	1450-1496	0.23	0.19	0.28
<b>Parma</b>	Seed	1509-1539	0.24	0.27	0.28
	Production	1784-1805	0.18	0.19	0.17
<b>Sienna</b>	Production	1560-1667	0.29	0.21	0.25
<b>Winchester</b>	Seed	1349-1453	0.16	0.14	0.16
<b>Average</b>			<b>0.23</b>	<b>0.20</b>	<b>0.23</b>
<b>Circuit</b>		<b>1815-1859</b>	<b>0.15</b>	<b>0.12</b>	<b>0.24</b>

Sources: Goy (1972, pp. 250-53); Tits-Dieuaide (1975, pp. 291 and 317); Romani (1975, p. 137); Spaggiari (1966, p. 189); Parenti (1942, pp. 175-216); Farmer (1977, pp. 557-558); and Healy and Jones (1962, p. 578).

<sup>13</sup> Romani (1975, p. 137), Spaggiari (1966, p. 189) and Parenti (1942, pp. 175-216).

The list of tithes samples is longer than that of yields. However, its average is lower, except with the method of absolute deviations, for which the two coincide: This suggests that I may have overdone the omission of outliers in the tithe data. In short, the series of yields and tithes suggest that the deviations in production in the period under study were much more marked than those we find for national production at the end of 19th century (Table 2). It is highly likely that these differences can be put down, not to the period in question, but to the more reduced area of the series of tithes and yields. Given that the price series we will use are local, their use is not only improper, but also technically much more correct than that which uses national series.

## 5. THE EFFECT OF LONG -TERM STORAGE, TRADE AND THE SEED FACTOR

As we have pointed out, using yield variations is only justified by our lack of data on variations in consumption. In this section we contrast to what extent this identification is feasible. Put another way, to what extent trade, storage and the seed factor can distort the results.

Nowadays, studies on market integration give us clues as to the effects of trade on the stability of consumption in Europe. A high degree of integration would allow markets with a deficit to be supplied by those with a surplus –or a less pressing deficit–, which would avoid the appearance of shortages. Over the last few years several national studies have looked at the question of market integration, but their results often do not coincide. Furthermore, there is a European work –Persson (1999, pp. 91-130). The preliminary conclusion of this research is that before the 19th century there was little market integration, although in some national markets such as Great Britain or Belgium we find «precocious infants». Three factors seem to have played an essential role in this integration: the existence of a liberal legislation, good sea or river communications and agricultural surpluses. These same factors help to explain why they are not similar to other European countries. Indeed, we know that: first, States normally intervened in cereals markets. What is more, there is an apparently growing trend to interventionism from the end of the Middle Ages until the second half of the 17th century, which only begins to drop significantly with the spread of physiocracy. Second, few countries had a geography that allowed low transport costs. Even in England the cost of land transport was high. Third, until relatively recent times agricultural yields were too low to permit large commercial surpluses, especially in the south of Europe<sup>14</sup>.

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<sup>14</sup> Clark (1998); Buyst, Dercon and van Campenhout (1998); Persson (1999, pp. 65-90 and 131-155); Jackman (1962, pp. 139-140, 347-349 and 716-723).

Nevertheless, Persson estimated that the effects of trade on the barley market in the south of Sweden between 1610 and 1662 would have meant a reduction in the standard deviations of consumption from 0.25 –the value obtained from yields– to 0.16. Given the singularity of the case, it is obvious that we cannot generalize. However, could similar effects have reduced the consumption variability with respect to that of production? In my opinion, this effect is not very important given that with high transportation costs, the regions connected by the trade could not be distant. The surplus of regions with excess production that was marketed to regions with deficit could never be large because the variation in production would be similar. For this reason the proximity to the sea, i.e. to international markets, was a more decisive factor for smoothing out price oscillations. This access was only possible for some cities, which explains why the international grain market was smaller in comparison to the European total consumption; according to Braudel (1979, pp. 101-103), it would be less than 1 or 2 per cent of the total<sup>15</sup>.

By long-term storage –or carry over– we mean storage done systematically, be it by public or private agents, in order to satisfy demand during the agricultural year which begins at the next harvest. That is, it does not include storage aimed at satisfying the demand for cereal in the eleven months following the harvest, and which causes prices to change according to the season. There is a great deal of controversy about its importance or even existence; both questions are far from being resolved. However, I believe there are two strongly related arguments to reject the relevance of storage as a regulator in wheat markets<sup>16</sup>.

Firstly, we have hardly any evidence, except for some specific cases, that this activity was important. There is little evidence as to the existence of long-term private stores. Then again, speculating with grain was not well-considered when not actually prohibited, which could justify the scarcity of records. But the same could be said of prostitution or smuggling which, however, existed and can be accounted for. It is very significant that Parisian bakers made no use of the grace that the Crown attempted to bestow upon them to create their own granaries<sup>17</sup>.

In fact, neither does it appear that, except in some countries, there had been large public grain storage systems. Outside Mediterranean Europe I have only found two cases, both in the 18th century: The Prussian stores system and the system of Swedish grain banks. The activity of the former was linked to the needs of the Army, such that only the second case could really be considered. As we have seen, Persson rules out its effects. In Mediterranean Europe we find more

<sup>15</sup> Persson (1999, pp. 52-54). It is relevant that prices in Bordeaux were less variable than in Bourges, despite the fact that variability in output has an opposite behaviour.

<sup>16</sup> Hoskins (1964 and 1968); McCloskey and Nash (1974); Wrigley (1987); Fogel (1992); Nielsen (1997); and Persson (1999, pp. 47-64).

<sup>17</sup> McCloskey and Nash (1974, pp. 173-177); Kaplan (1996, p. 562).

examples. Firstly, there is the strange –not strictly speaking «Mediterranean»– case of Geneva, where a theocratic regime kept an important public granary for many years, together with several market control mechanisms. In Italy there were several public grain stores in the cities of the centre and south of the country –Rome, Naples, etc.– and many «*monti frumentori*» in the Peninsular area of the Kingdom of the two Sicilies; and especially, in the region of Molise. But it was in Spain – except its northern regions– where the most widespread public cereal storage system was developed: the *pósitos*. The story of all these granaries seems to have been the same: they were unprofitable institutions of doubtful public use, which died almost unnoticed with the arrival of Napoleon's troops, if not before. Perhaps the most revealing fact is that the worst agricultural crises at the turn of the 18th century occurred in Spain, the country that went to the greatest lengths to construct and maintain these stores<sup>18</sup>.

The lack of profitability of granaries is, in itself, the second reason to raise doubt about its incidence. Probably this is the reason why we do not know of long-term speculative activities in private granaries. This also explains the geographical distribution of public granaries. Although granaries were unprofitable, in regions with a dry climate they could prosper because the costs of grain deterioration were smaller. This is the case of southern and central Spain and southeastern Italy. In contrast, in «humid» Europe, where deterioration of grain was more significant, political support was crucial. For this reason, public granaries only existed under the protection of authoritarian States, like those of Prussia or Geneva. In short, if carry over activities were fragile and needed protection, we can assume that its incidence had to be exclusively local<sup>19</sup>.

However, there is another approach to the «storage effect». Precisely because the harvest was unpredictable and demand more or less constant, the generation of surpluses in years of good harvests would be unavoidable. Despite losses, peasants, millers, bakers and grain agents would store cereal for two reasons: excess of corn and granaries had few alternative uses –only as animal food–, and there was nothing to ensure that the next harvest would not be a disaster. This would explain the apparent contradiction existing between the testimonies that prove the deficiencies of storage systems, and the reduced utility of the methods proposed to correct it<sup>20</sup>.

In section 2 we mentioned the difference between using net or gross yields. The essence of those lines –or Wrigley's work– is that the use of gross production

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<sup>18</sup> Persson (1999, p. 83); Piuze (1964, pp. 54-76); Castro, (1987, pp. 153-175)

<sup>19</sup> Persson (1999, p. 83); Piuze (1964, pp. 54-76). About the small incidence of carry over, Persson (1999, pp. 55-62 and 67-72). In the case of Sienna, Parenti (1942, p. 70) says: «*L'Abbondanza di Siena non fu mai in grado di poter regolare in senso positivo el livello o la dinamica del mercato del grano.*»

<sup>20</sup> Kaplan (1984, pp. 53-63).

data implies an underestimation of the oscillations of consumption; that is, an underestimate of demand elasticity. If I am not mistaken, all the sources used to estimate consumption are based on gross yields. Tithes are a fixed part of the harvest; so they are a measure of gross production. The same can be said of the yields based on seed or surface. Finally, data from Sienna and Parma –in the 18th century– are gross production. Consequently, we can assure that there is a bias in those estimates.

The problem is that it is not easy to estimate how much of a bias exists. Basically, it depends on the seed factor, which could vary a lot for each country and period considered. For instance, the seed factors found by Le Roy Ladurie in Languedoc or Joseph Goy in Arles, are low: 1:4 or 1:5. So, with the latter seed factor, consumption variability is underestimated by 29.7%. On the contrary, in Winchester or Brussels, we find much higher yields, even in medieval times. All in all the seed effect is never negligible. For example, if we subtract a seed factor of 1:10 from a production series in Great Britain, the underestimate of consumption variability is 11.4%.

In short, the bias introduced in the calculation of the variability of consumption due to long-term storage and trade has an opposite sign to that of seed factor. The scarce commercial integration and the difficulties of storage suggest that these factors may not have led to great changes in the variability of consumption, which in any case, would have been compensated for by the upward bias of the seed factor. With the available data it is hard to make a more precise assessment of the facts.

## 6. THE VARIABILITY OF PRICES

Let us now go on to look at prices. The sample size limitations depend on maintaining the conditions required of the data: in particular, excluding very marked trends and being able to assure the functioning of markets. The first of these conditions was seriously altered by the «Price Revolution» of the 16th century; in particular, the second half of the century suffered strong trends. The second condition had a critical moment with the elimination of controls on internal trade, which took place between the decade of the 60s in the 18th century and the 20s in the 19th century, except in Great Britain, where it was earlier. Thus, with this exception, an optimum period for this estimate would be the period between 1620 and 1760; but it depends on each city.

Another problem arises from the time unit used. Initially, it would be logical to employ annual series, more specifically, referred to agricultural years, in order to make deviations in quantities and prices comparable. However, perhaps it would be advisable to do a test on the results derived from using monthly samples. Table 5 reports the results obtained using two samples, from Toulouse between 1600 and 1788 and from Siennac between 1580 and 1765. In both cases, the three methods –trends, moving averages, and logarithms– described are applied to monthly and annual samples.

TABLE 5

## VARIABILITY OF ANNUAL AND MONTHLY SAMPLES

City	Period	Years	Frequency	Trend	Moving Av.	Logarithms
Toulouse	1600-1788	188	Monthly	0.33	0.31	0.47
			Annual	0.31	0.28	0.46
Sienna	1580-1765	185	Monthly	0.35	0.25	0.32
			Annual	0.29	0.23	0.31

Sources: Frêche (1967, p. 50-74); Parenti (1942, p. 38-41)

As was to be expected, the variability of the monthly sample is greater than that of the annual, but not by very much. In the case of Toulouse, it is almost insignificant with the method using price logarithms, and very small with the other two. The case of Sienna provides similar results, except with the first method; that is, that of the estimation and posterior elimination of the trend. I do not think that this differential behaviour is caused by problems such as seasonality, but by the intrinsic deficiencies in the first method. This may be because a linear trend is not always the best adjustment, or may even be unacceptable to discover the long-term evolution of prices. This problem did not arise with the tithe or yield series because they lacked a definite trend. Given the difficulties of applying a specific function to each price series, and that the results of the adjustment are not necessarily better than those of the mobile average, it seems wise to do without this method.

We are now in a position to calculate the price deviations. For the reasons explained, I will only use prices per agricultural year, which puts quite a restriction on the number of places. Likewise, I have attempted to avoid samples from the 16th century, and from the end of the 18th century. Specifically, the series and periods chosen are: Brussels (1599-1794), Cologne (1600-1786), Exeter (1316-1549 and 1620-1792), Paris (1520-1697), Ruremonde (1599-1793), Sienna (1580-1765), Toulouse (1600-1788), and Vienna (1692-1799). The most significant fact is that the deviations are in a fairly narrow band of 0.17/0.28 (moving average) not far from that found by Lee in England<sup>21</sup>.

<sup>21</sup> Except Vienna –rye–, all the series refer to wheat. An interesting question is whether the variability of rye or barley in the same country was higher than that of wheat. Rye bread was more common in East-Central and Northern Europe. But in countries like France or England it was the common food for the poorest classes; and the same could be said about barley. For this reason, a higher variability in these cereals would indicate, from the point of view of poor people, a more rigid curve of demand. Certainly, this is true. There are several ways to explain it; in particular, its lower price increased the cost of transport, and made the markets less integrated. But, in any case, the difference is not large enough to modify the results significantly. See Labrousse (1933, pp. 188-99). The values of the series for the Iberian Peninsula, Evora for 1737-1796 and Pamplona for 1589-1800, are not very different either.

**TABLE 6**VARIABILITY OF PRICES IN EUROPE. 14<sup>TH</sup> - 18<sup>TH</sup> CENTURIES

City	Period	Moving average	Logarithms
<b>Brussels</b>	1599-1695	0.19	0.26
<b>Cologne</b>	1600-1786	0.17	0.25
<b>Exeter</b>	1316-1549	0.25	0.30
<b>Exeter</b>	1620-1779	0.19	0.24
<b>Paris</b>	1580-1697	0.25	0.36
<b>Ruremonde</b>	1598-1793	0.18	0.25
<b>Sienna</b>	1580-1765	0.23	0.31
<b>Toulouse</b>	1600-1788	0.28	0.46
<b>Vienna</b>	1692-1799	0,21	0,27
<b>Average</b>		<b>0.22</b>	<b>0.30</b>
<b>Evora</b>	1737-1796	0.21	0.30
<b>Pamplona</b>	1589-1800	0.23	0.31

Sources: Verlinden (1959, pp. 487-493); Ebeling and Irzigler (1977, pp. 666-671); Beveridge (1929); Baulant and Meuvret, (1960, p. 243 and 1962, p. 135); Ruwet (1966, pp. 91-97); Frêche (1967, pp. 50-74); Parenti (1942, pp. 38-41); Pribram (1938, pp. 390-392) Godinho (1955, pp. 135-136); and Arizcum (1989, pp. 82-84).

## 7. CALCULATION OF THE DEMAND ELASTICITY

To find the demand elasticity we divide each of the standard deviations of consumption (in reality, yields or tithes) by each of their respective –that is, geographically close– deviations of local prices. However, there are two reasons for using the averages of these data. Firstly, the data referring to consumption do not often correspond to places close to others in which there is a price series. Secondly, what we have seen in the tables above is that the most marked differences are not due to the use of different periods and the transformation with the first or second procedure, but to the sample size, kind of data (tithes or yields) and the use of logarithms. These motives explain the structure of Table 7.

In short, to estimate the demand elasticity we could divide these statistics by the average of those obtained with the prices –0.22 for mobile averages and proportional deviation from trend and 0.30 for logarithms–. The result is shown in Table 8.

Two of the four estimates give the same result: – 0.78. The other two are above and below this figure. The average of the four is – 0.79. Of course, we have to bear in

**TABLE 7**

VARIABILITY OF TITHES AND YIELDS IN EUROPE. SUMMARY

Sample	Moving average/Trend	Logarithms
<b>Tithes France and Geneva</b>	0.17	0.21
<b>Yields England, Belgium, France and Italy</b>	0.20	0.23
<b>National yields 1884-1913</b>	0.10	0.13

Sources: See the text

**TABLE 8**

ESTIMATES OF THE ELASTICITY OF DEMAND

Sample	Moving average/Trend	Logarithms
<b>Tithes</b>	-0.78	-0.68
<b>Yields</b>	-0.90	-0.78
<b>National yields 1884- 1913</b>	-0.45	-0.43

Sources: See the text

mind the possible existence of an upward bias due to the role of trade and «casual» storage. However we should not forget the opposite effect of the seed factor. It is important to note that, for the reasons explained in section 5, trade and carry over, could not have a significant effect in the oscillations in consumption. On the contrary, the seed factor was never negligible.

Much more questionable is the comparison of national yields in the 19th and 20th centuries with local prices in the 14th to 18th centuries. My view is the following: let us assume that the period 1884-1913 was «normal», that economic integration was complete, and that the effects of storage and the seed factor cancelled each other out, then the relation between the two would be the value of demand elasticity. As we know that these suppositions were not true in the Pre-modern Age, we can affirm that these values are lower. In short, it seems clear that not only was demand elasticity for wheat not very low, as Fogel proposes, or low, as Davenant affirms, but that it would actually have been close to  $-0.79$ . The value of  $-0.64$  found by Persson in Sweden is reasonable: the average of  $-0.44$  and  $-0.79$  is  $-0.62$ .

## 8. CONCLUSION

The objective of this paper was to estimate the wheat demand elasticity in Pre-modern Europe. In my opinion, this topic is relevant because it has important



implications for our vision of Pre-modern economy. Moreover, there are good reasons to believe that the common approach to this problem –Pre-modern elasticity being governed by King's Law– is erroneous.

It is, of course, easy to question the accuracy of the procedure but I do not think that anyone who has dealt with this topic has attained complete perfection. The method proposed by Fogel provides biased estimates but we really do not know what magnitude of bias is being introduced. Excluding long-term storage and trade provides estimates which are too low; although it is reasonable to think that these effects –and especially that of carry over– are not too important. On the contrary, the use of gross yields provides too high estimates. This is not a negligible effect, but it could be very different for each country and period. It is not clear whether this seed factor cancels out the effects of trade and carries over. Note that, from the Middle Ages to the 19th century, the effect of the seed factor on the harvest was decreasing while the trade factor was increasing.

In this paper I have presented new calculations for wheat demand elasticity, which provide me with an estimation of  $-0.79$ , but there are reasons to believe that the actual value should be somewhat lower, like that of  $-0.64$  proposed by Persson. Both of these figures are far from the low values derived by King's Law, or those even lower values estimated by Fogel. Actually, the biases derived from trade, carry over and seed factor prevent us from reaching a more accurate estimate. For this reason, it would surely be more prudent to state that the wheat demand elasticity in Pre-modern Ages was around  $-0.7$ , or between  $-0.6$  to  $-0.8$ .

This exercise could prove unnecessary. There is an easy way to demonstrate this: if you have understood the «intuitive» idea that supports Fogel's procedure, you would only have to observe the plot of prices and quantities. Oscillations in prices are only slightly bigger than those in quantities. Consequently, demand elasticity has to necessarily be closer to  $-1$  than to  $0$ .

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