propria produzione. Continuava a non svilupparsi –fra l'altro– un'aristocrazia civile che sulla base di ricchezze e autonomi orizzonti sovraregionali potesse aspirare ad attenuare la potenza della nobiltà fondiaria.

La conclusione del volume –forse in parte al di là delle stesse intenzioni dell'autore– restituisce quindi retrospettivamente senso e legittimità alla ricerca che si è dedicata ad individuare nel medioevo strutture che avrebbero alla lunga influito sulla successiva storia isolana. Certo l'attenzione a quei temi si è alimentata a costi che ora non sono più sostenibili. Il prezzo pagato, frutto di malcelati anacronismi, di mal controllate contaminazioni ideologiche e di una profonda deformazione di prospettiva, è stato infatti costituito dalla ripetuta tentazione di procedere a una sistemazione globalizzante della intera vicenda sociale tardo-medioevale in chiave di blocco e di immobilismo plurisecolari. Ma sono costi che non dovremo più sostenere. A Epstein va tutto il merito di aver fran-tumato a nostro beneficio quel groviglio storiografico: il suo libro rappresenta una spinta pienamente in grado di mantenere la storia del medioevo siciliano lontano dalle secche in cui si era insabbiata.

A REPLY

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1. Petralia's comments are generous, perceptive and on the whole accurate. Several of the specific issues he raises can only be resolved by further work in the archives; others indicate more general points of methodology, some of which I discuss below. We seem instead to disagree on the significance of those matters for a broader understanding of Sicilian and Italian society. My reservations arise from the fact that Petralia focuses on the strictly Sicilian and Italian themes in the book (the role of foreign trade and the contrast between North and South). However, he overlooks the use of the Sicilian example to outline a model of the late medieval «crisis» that is applicable, *mutatis mutandis*, to the whole of Europe. Yet this establishes what is possibly the book's principal claim: that Sicilian, and by extension Italian, economic history are merely a variant of a more general, European pattern of development –thus implying that the frame-

work for comparative history can no longer be just Italy, but must embrace Europe as a whole.

Petralia is of course in part aware of this underlying argument, and indeed implies (although he does not use the term explicitly) that *An island* has caused a «paradigm shift» within the historiography on Italian development, which renders Bresc's and my approach essentially incommensurable («*due mondi incompatibili*»): it is not so much the data we use that are different, as the way that we give them meaning. But there are two ways to react to such a shift. One is to say, like Petralia: let us drop the old theoretical framework which is no longer useful, and return to the old but still important questions with a fresh and unbiased mind. This requires an act of faith in the old historiographical framework, a conscious decision to view Sicilian history strictly as a variant of Italian history. Alternatively, one can pursue the metaphor of the «paradigm shift» further, to argue that such a change also inevitably causes the hierarchy and content of «relevant» questions to change: problems that were central to the previous paradigm become irrelevant and new problems emerge in their place. Here I shall try to sketch out some reasons why the «paradigm shift» we should be contemplating is from a history of «Sicily-as-part-of-Italy» to a history of «Sicily-as-part-of-Europe».

2. Petralia's suggestion that an excessive desire to overturn previous assumptions leads me to underestimate the relative impact of foreign trade[1] does not take into account two essential premises of my argument. The first is that surviving records of economic transactions in this period, principally notarial contracts, produce a distorted picture of the economy because they over-represent the number and volume of commercial transactions between foreigners and

[1] Book titles can sometimes unintentionally mislead. *An island for itself* does not define an economically and politically autarchic world «*indipendente da quelli... circostanti*»; the title refers to a society whose history has not been imposed or determined from the outside, whose identity is not defined through its relations with «foreigners». (The title is the result of a verbal and conceptual pastiche, between a famous verse by the seventeenth century poet John Donne –«*no man is an iland, intire of itselfe*»– and the Hegelo-Marxian distinction between objective «class in itself» and subjective «class for itself»). The book neither ignores nor dismisses the role of foreign trade, although readers trained in a previous historiographic tradition may interpret the emphasis on domestic factors in these terms. On the contrary, I note at several points that late medieval Sicily had an extraordinarily *open* economy (see S.R. Epstein, *An island for itself. Economic development and social change in late medieval Sicily*, Cambridge 1992, pp. 282, 312-313, 407), comparable only to late medieval Holland and far more open than, say, early modern Poland, often seen to epitomize capitalist «dependency through trade» before the industrial revolution.

autochthons compared to domestic ones: whereas foreigners required a written record of transactions to protect their property rights in court, Sicilians could more frequently rely on oral agreements that left no written trace. This rule applies both to sales of commodities, studied in particular by HENRI BRESC, and to the transactions in the agricultural credit market discussed by Petralia. The second premise is therefore that one can make sense of the micro-level data emerging from notarial records only in the context of an adequately reconstructed macro-economic framework.

Both considerations clearly apply to the grain trade. We can only estimate the overall impact of foreign merchants on the grain market if we know the average proportion of output they traded. Average exports from medieval and early modern Sicily never exceeded 17 percent of domestic output net of seed (the ratio before 1500 being 5-15 percent),[2] and foreign merchants seem to have largely avoided the domestic grain trade; it follows that they will not have controlled more than 5-15 percent of the credit market. In fact, although conditions probably varied locally, it is unlikely that foreign credit sustained that large a proportion of the grain produced for overseas markets.[3]

Petralia also suggests that I underestimate the role of foreign traders in developing the silk and sugar industries, which from *c.* 1430-50 began to turn increasingly from the domestic to overseas markets. Since these industries were entirely demand-led (they «produced on commission»), he argues, foreign merchants who conveyed information on foreign markets to Sicilian producers in effect «caused» these industries to develop. By contrast, I underplay the role of foreigners, and emphasize Sicilian entrepreneurship. In fact one can accommodate our two perspectives (which are essentially «demand» and «supply»-led models of growth) by distinguishing between «long-run» and «short-run» causes of industrial expansion. In the short run, demand factors were clearly most important. But in the long run, supply factors must surely have been essential. Both industries could only expand into foreign markets because they had access to a pool of native skilled labour which had built up over centuries, mainly to supply the domestic market. In addition, the very long investment lead-ins (se-

[2] *Ibidem*, pp. 274-275.

[3] Foreign merchants would have been unable to predict changes in overseas demand very far ahead, so they would not have sunk very much capital in loans. They also faced higher information and enforcement costs than native Sicilians in the local credit market; if the latter was competitive (see below), their margins on the loans would have been lower. On logical grounds –which must of course be tested against the evidence– foreign credit should not have been a major player on local agricultural credit markets.

veral years for sugar cane, up to a decade for the mulberry trees which fed the silkworms) were particularly disavantageous for foreign-based merchants, who because they lacked detailed technical knowledge and could not supervise their investment closely, faced higher risks of failure. For both reasons, direct investment by overseas merchants in the silk and sugar industries was most unlikely.[4]

3. There is evidently need for more research on these topics, as Petralia underlines. Similarly I agree with him that we require more rigorous analysis of agricultural credit markets, in place of the tired sophistries and preconceptions of the older literature. In particular, Petralia suggests that we need to verify if agricultural credit markets were competitive, as I claim, or «usurious» (monopolistic) as he implies they might have been. This criticism seems to be based on a misunderstanding of my arguments. A monopoly on credit (the source of «usury») will exist only where (a) borrowers have access to only one lender, or (b) lenders collude to set a fixed rate of interest, which will therefore include a monopoly profit; the latter can only occur where lenders are few and the borrowers' circumstances and requirements do not change. Where neither of these conditions apply, competition between lenders will drive the rate of interest down to an average rate of return on capital. I suggest that this is what occurred in late medieval Sicily, where producers had access to large numbers of lenders in big cities, and where the size of the agricultural market made collusion between lenders unlikely. Insofar as a competitive credit market should produce similar rates of interest across the region, this outcome can be verified empirically and Petralia correctly points out that I do not do so.[5] On the other hand, I do not think that medieval theories of usury are much use in understanding the modern concept of monopoly profits on credit. Surely the fact, noted by Petralia, that the law enshrining the *contratto alla meta* was drafted in the context of a papal campaign against «usury» –defined as interest above a mere 10 percent!– is proof of theological muddle rather than of economic reality.[6]

[4] Of course, neither the second nor the third point applies to merchants who settled in Sicily, like the Pisans whom Petralia has in mind.

[5] Rates of interest significantly higher than the average can be explained either as monopoly profit or as reflecting a higher risk. The choice of explanation depends on the degree of competition in the credit market, and can be determined empirically.

[6] The fact that the law on *contratti all meta* also refers to several other credit arrangements used by foreign merchants suggests that they were protecting themselves against ecclesiastical sanction across the entire range of contracts they employed; this in itself gives no indication of the extent to which they controlled the sicilian credit markets.

4. An assessment of the relative balance of «foreign» versus «domestic» trade thus requires a combination of theory and macro-economic indicators to contextualize detailed empirical analysis at the «micro» level. But by casting the debate in terms of a stark contrast between the domestic and the international spheres, Petralia is in danger of resurrecting the dualistic paradigm which he otherwise considers to be dead and buried.[7]

To understand the role of commerce for Sicilian growth we must distinguish between positive and normative arguments about trade. In strictly *positive* (economic) terms, trade is beneficial because it stimulates both specialization (through the division of labour) and innovation (through competition on price). A priori it makes no difference whether trade is domestic or foreign; assuming that domestic output is competitve on international markets, the distinction between the two spheres of trade is simply a function of relative transaction costs. In the early middle ages, long distance trade cost more than short distance, so only high value added commodities entered into it; when the costs of transport over long distances declined (as they did significantly during the later Middle Ages), the barriers between the two spheres of trade disappeared and cheaper goods increasingly found their way onto international markets.[8]

This argument implies that it is also a priori irrelevant whether merchants are of domestic or foreign origin. By contrast, the paradigm of dualism and dependency holds to a strictly *normative* view of trade, in which merchants exploit their control over foreign trade to exert political and economical power over the host country, while at the same time playing a revolutionary role in economic development and «modernization». Therefore the fact that a country has a strong native class of merchants, or alternatively must rely mainly on foreign traders as Sicily did, is used as a measure of its relative economic «modernity» or «backwardness».[9]

[7] Let me note in passing that Petralia's plea for further research on the economy of the twelfth and thirteenth centuries, with which I essentially agree, is based on a similar misunderstanding. My approach draws on a simple behavioral assumption that peasants would respond rationally (i.e. by ordering preferences) to the institutional constraints that they faced in their economic activities. Therefore, as long as that institutional context is understood and accounted for, it can be assumed that a twelfth century serf in a weakly «commercialized» economy was just as capable of rational calculus (limited by the information at his disposal) as a fifteenth century large-scale tenant; only the risks and opportunities they contemplated were different.

[8] EPSTEIN, *An island* cit., pp. 268-270.

[9] One may note that before mercantilism was backed by cannon, the commercial boot was on the other foot: foreign merchants depended for tax privileges on the authorities of the countries they traded with, who had no compunction in exploiting their position of power (*ibidem*, pp. 284-285).

Petralia suggests that my explanation of Sicilian decline in the seventeenth century accepts the previous normative orthodoxy, that the roots of the country's failure to «modernize» are to be found in the twelfth and thirteenth centuries, when Sicily was unable to establish a native commercial «bourgeoisie» and a strong export-led textile industry. My argument is actually rather different, and goes something like this. Further economic development after the mid-seventeenth century crisis required larger internal markets. What characterized successful countries like England was their ability to lower the jurisdictional costs (set by tariffs, tolls, etc.) of domestic trade. Instead Sicily's «natural» domestic market, the kingdom of Naples, reacted to contraction in the opposite way, by raising internal barriers to trade which restricted opportunities for further growth through specialization.

On the other hand, the question whether a stronger mercantile and manufacturing base would have helped Sicily reach a more positive outcome to the «crisis» is a moot point. Although I do not pursue the question further in the book's conclusion, it is worth noting that neither strong merchant communities nor powerful urban manufactures shielded northern Italy from the devasting effects of the seventeenth century depression. In the « bourgeois» and «industrial» North, markets contracted, old urban industries collapsed, industrial production shifted from the towns to the countryside, and traditional manufactured exports were replaced by the kind of raw materials (silk, oil, wool) once considered typical of the «backward» South. In other words, the allegedly more «modern» northern Italian institutions –epitomized by the vibrant societies of the city-states hinted at by Petralia– responded to structural crisis little better than the «backward» southern monarchies.

5. The paradox is such, however, only so long as we remain within the magic circle of the old historiographical paradigm. The paradigm was founded upon a syllogism: «feudal» institutions were not conducive to economic growth; therefore, the economy of the «feudal» South could not develop. But proof that the South could achieve rates of growth equal or superior to those in many regions in the North causes the old historiographical castle to collapse. My book reinterprets old and new archival evidence to suggest that the traditional contrast between «bourgeois» cities and «feudal» states is of little use in explaining the course of development in Italy, or indeed elsewhere in the West. And it suggests an alternative interpretation which draws on a more complex understanding of how social institutions affected economic performance in the past.

Thus, to object that the aristocratic reaction in Sicily after 1500 shows the country's lack of «modernity» is to confuse the institutional trappings of authority with the machinery of economic accumulation. Of course the Sicilian state was «feudal», in the customarily loose and imprecise sense of the term: how much of Europe before the French Revolution was not? But alternatively, was a strong «bourgeoisie» a prerequisite for unending economic growth? Did modern Italy's merchant classes, or Holland's bourgeois state, propel these countries teleologically to industrialized bliss? Why did that «transition» first occur in England, an archetypal «feudal» state and an underpopulated country whose urban population before 1650 was half the size of Italy's? To measure a society's economic «modernity» from the strength of its urban social fabric risks making the same mistake Petralia identifies in Bresc: namely, to slide insensibly from a (partially) measurable and economic definition of «progress», to a more vaguely cultural definition of «modernity» and «backwardness» whose inherent pitfalls are self-evident. This is a slippery slope indeed, and I do not embark upon it in the book.

6. In conclusion, if one accepts that the dualistic paradigm in Italian history has collapsed, then many of the historiographical questions it produced become largely irrelevant to questions of economic history. The new perspective I propose raises questions of a different kind. The main one concerns the interplay of political and social institutions –the norms, laws, organizations and rules of the game– and economic performance: did political structures make any difference to the preindustrial economies, and if so, in what way? The answer, I suggest, lies in the changing balance of power between sovereign, lords, towns and rural communities over a given region, which defined the capacity of each to capture the profits from trade. The attractiveness of this hypothesis is twolfod: it promises to unite the «new» political history with the «new» economic history into a new «political economy of growth»; and it lays the foundations of a genuinely comparative European history, which adds to the institutional historian's interest in a typology of political and jurisdictional arrangements, the economist's concern with dynamism and change.[10] In this sense, perhaps, Sicily could become a paradigm for Europe as a whole.

[10] See S.R. EPSTEIN, «Town and country in late medieval Italy: economic and institutional aspects», *Economic History Review*, 2nd ser. 46 (1993), pp. 453-477; IDEM, «Regional fairs, institutional innovation and economic growth in late medieval Europe», *Economic History Review*, 2nd ser. 47 (1994), pp. 459-482; IDEM, «Freedom and growth. The European miracle?», in E.V. BARKER ed. *LSE on freedom*, London, 1995, pp. 165-181.