A Response to Professor Reed

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After citing and mis-citing some of my published work, Professor Reed declares that his purpose is "to show that none of the conclusions" that he ascribes to me "are upheld by the known evidence or by economic theory." He concludes by noting that a price inference approach is no substitute "for direct observation on economic variables." There can, I think, be no serious disagreement with this latter remark and, as I am totally in accord with Professor Reed on the matter, I feel somewhat churlish in observing that more than half of his footnotes refer to my work and that none refer to direct observation of original source material. What then is Professor Reed's purpose? His claim to demonstrate that neither historical evidence nor economic theory justify my conclusions at first seems promising. The reader looks forward to a juxtaposing of evidence that will refute or invalidate some assertion that Miskimin and others have made or to the moment when the full power of economic analysis falls upon those authors and reveals them mired in theoretical inconsistency. This does not occur. Instead, the reader learns that medieval data, even late medieval data, are ambiguous and that economic theory will generate varying conclusions depending upon the initial assumptions and premises. In fairness, I do not believe that anyone who has done serious work in medieval economic history would dispute Professor Reed's estimate of the nature of the available evidence nor would any reasonable economist deny that the conclusions drawn from economic theory depend upon and fluctuate with the assumptions.

We will return to questions of evidence and of the role of economic theory in history, but first let us consider those of Professor Reed's arguments which appear to be substantive. He finds price data abundant and unambiguous — a concession that indicates a generous turn of mind and perhaps a certain unfa-

miliarity with the sources. He then notices my argument that the terms of trade shifted in favour of manufactures, causing a balance of payments deficit in rural trade with urban centres. He does not acknowledge my consideration of the complementary pressures arising from increased taxation, military expenditures, and migration to urban areas.[1] Similarly, when he offers a précis of my argument for an outflow of bullion from the northwest to the south and east, he presents it solely as a terms-of-trade issue without observing that, in accounting for the movement of precious metal from northern Europe, I suggest three motivating forces — shifts in consumption patterns, papal remittances, and military and diplomatic expenditures.[2]

Stripped of much of its historical context and reduced to a minimal theoretical expression, my argument is almost ready for Professor Reed's economic analysis, but one further distortion was apparently felt necessary to assure a certain victory. In his theoretical section, Professor Reed reduces my discussion of the probable causes of the rural-urban balance of payments deficit exclusively to a relative shift in demand favouring manufactures. This is certainly part of my argument, but it does not follow, as Professor Reed asserts, nor do I so claim, that total revenue in manufacturing rose. Further, given the extent of the late medieval demographic catastrophe, Professor Reed is simply wrong in his statement that "these demand shifts imply that manufacturing production increased both absolutely and relative to agricultural production." Since the statement is neither correct nor extant in any of my published works, it seems injudicious to place it in evidence as a contradiction of my findings.

Having first ascribed to me an argument predicated solely on demand shifts and then apparently satisfied himself with his refutation of that, Professor Reed sets out to "offer a more consistent explanation of the observed movements in relative product prices" by focusing on the supply side of the equation. His contention that population decline in urban areas was greater than that in rural areas is unsupported by any statistical evidence and utterly neglects the critical problem of migration to the towns.[3] With this exception, I am happy to accept Professor Reed's "alternative" explanation or rather, to welcome its return -- a prodigal idea which, though besmirched and cast in unfamiliar language, remains my own. In discussing the relative positions of agricultural and manufacturing output within the context of overall decline[4] in *both* sectors, I observed that:

> The supply of manufactures was rigidly tied to the number of workers and to their technological prowess. Hence, in contrast to the situation in agriculture,

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[1] H. A. Miskimin, *The Economy of Early Renaissance Europe, 1300-1460*, Cambridge, Cambridge University Press, 1975, p. 90 and elsewhere.

[2] Miskimin, *Economy*, p. 148 and elsewhere.

[3] Miskimin, *Economy*, pp. 29-33.

[4] Miskimin, *Economy*, pp. 81-86.

> overproduction was not an immediate problem in industry. The presence of more capital per worker did not foster significant gains in output; the difficulty of training, or of finding, skilled artisans tended, if anything, to reduce productivity, At the same time the increased desire for luxury diverted those with the greatest skills from the more common tasks. With stable or reduced productivity and fewer workers, there was little chance of excess production. Thus for a time, the towns enjoyed a favored economic position, insulated against the problems evident in agriculture.
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> *The Economy of Early Renaissance Europe, 1300-1460*, p. 86.

So be it. We appear to agree on the role and importance of the relative supply of manufactures and agricultural products. I would even be prepared to accept Professor Reed's assertion that balance-of-payments inferences are technically indeterminate, if one considers only arithmetic and not history. It is, however, necessary to point out that the price levels of manufactured goods are notoriously difficult to determine, that they are especially elusive for the unique and luxurious goods that were most sought after, and that his argument (PmQm/PaQa) totally ignores the economic impact of imported goods. One may also note that in choosing fifteenth-century prices for his illustration, Professor Reed overlooks my statement that:

> That part of urban prosperity which had been based on the divergence between the prices of manufactured and of agricultural products could last only so long as it took to drain the countryside of funds. In England, there is evidence that this had happened by the beginning of the fifteenth century; the price gap then closed, as the costs of manufactured goods fell in relation to agricultural prices.
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> *The Economy of Early Renaissance Europe, 1300-1460*, p. 90.

Late fourteenth-century disparities between manufactured and agricultural goods' prices appear to have been much greater than they were in the fifteenth century. So far my comments have been specific and technical, but let us be gracious and allow all of Professor Reed's premises. Even if we yield hypothetically an all counts, we learn finally that " the more likely is the conclusion of a balance of payments movement in favour of urban areas." He has arrived at the position expressed in my work — a triumph that is immediately repeated. I believe that Professor Postan is quite capable of defending himself, but if Professor Reed had given more care to my discussion of the butter question, he might have noticed my attempt to contrast the variant influences on the relative supplies of grain and butter.[5] Slow herd growth, its interconnectedness with the demand for meat, the epizootics of 1348, 1363, and 1369, and the impact of human mortality through heriot payments in limiting herd size are all noted

[5] MISKIMIN, *Economy*, pp. 40-41.

as factors on the supply side affecting the prices of butter and meat and their relative strength in comparison with grain prices.

When Professor Reed addresses empirical problems, we are even more disappointed by the total absence of new evidence. His discussion is limited solely to possible variant interpretations of *some* of the material mentioned in my work; while my interpretations are questioned, no substitute explanations are offered or cogently argued. After apparently accepting the reality of a late medieval monetary shortage, Professor Reed, again citing my published work, notes that the dearth may have been caused by the collapse of domestic mining. My original position was that diminished mining ouput was indeed a partial cause of monetary scarcity. He presses on, picking up an idea that Carlo Cipolla put forth in embryonic form some years ago, and suggests that monetary shortage may have been induced by higher per capita incomes. Cipolla appears to have felt that falling prices represented an increase in the demand for money expressed through a rise in the commodity price of bullion. He hypothesized that such a situation might arise if population were stable, while (real) incomes were increasing.[6] In terms of the Fisher quantity equation, this condition would appear as follows:

$$\overline{M}\,\overline{\overline{V}} = \overset{\uparrow}{P}\overset{\downarrow}{T}$$

If the money supply and its velocity of circulation remained constant, an increase in transactions or output would result in a decrease in prices — a phenomenon that could also be referred to as an increase in the commodity price of bullion or as an increase in the demand for money. I hasten to admit — I am sure Professor Reed will join me — that it is impossible to measure with certainty any of the four terms of the equation during the period under consideration and therefore, that absolutes will not emerge from such theoretical exercises.

Let us, however, remember history. Population did fall sharply throughout the late fourteenth and early fifteenth centuries, and if we assume constant M and V terms, the first effect of rapid population decline would have been a surplus of money as output fell with population. If population decreased by one third, a fifty percent increase in average productivity among the surviving citizens would have been required to maintain output (the T term) at a level high enough to forestall an increase in the price level, i.e. a fall in the commodity price of bullion or decrease in the demand for money. Despite some fragile indications of a limited increment in average worker productivity in the agricultural sector in response to an elevation in the average quality of arable land, there is nothing to suggest that the gain approached fifty percent. There is even

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[6] C. M. Cipolla, R. S. Lopez, and H. A. Miskimin, "Economic Depression of the Renaissance?" *The Economic History Review*, 2nd. ser., XIV (1964), p. 524.

less reason to believe that there was any general productivity gain in the manufactured goods sector. Historically, since rapid depopulation occurred in the absence of massive productivity gains, the first consequence of plague was a surplus of cash and a rise in the price of goods — particularly luxury manufactures, but also certain foods such as wine and imported spices and fish.[7] In Cipolla's terms this was a decrease in the demand for money or a fall in the commodity price of bullion; it is in direct opposition to the monetary scarcity that Professor Reed and I agree existed somewhat later. In the absence of historically untenable levels of productivity increases to absorb this surplus cash, one of three things must have occurred with the passage of time to generate monetary scarcity and declining prices. Hoarding might have sharply reduced the velocity of circulation of bullion, but hoarding is consistent neither with the social history of the period nor with the public clamour over the absence of specie. Why would people across western Europe bury their money and then bemoan its scarcity? Wear and attrition might have diminished the money supply, but although this may have contributed to the dearth, it seems highly unlikely that the money supply of western Europe simply wore out in the fifty years following the first visitation of the Black Death. The third possible mechanism that might have driven Europe from monetary surplus to shortage is, of course, an international balance of payments crisis. Since contemporary expert opinion, legislation, public outcry, merchant complaints, and parliamentary petitions all implicate an adverse balance of payments as the cause of monetary scarcity and since it is the most likely theoretical explanation, it seems the most acceptable means of accounting for the observed shortage of circulating specie. Furthermore, such complaint is directly related to variations in mint output; heightened public outcry corresponds to periods of low coinage.[8] This correlation would appear mutually to confirm my views that public complaint was addressing a real issue and that, in the absence of incremental coinage, commercial, papal, and military and diplomatic remittances were draining the money supply away from England and northern Europe. Since the mining industry was in disarray and since extensive hoarding is both unlikely and undocumented, incremental mint outputs were likely to occur, as contemporary legislation and comment indicated, only as a result of a favourable balance of payments. The available statistics establish the fact that mint outputs remained very low throughout the late fourteenth and early fifteenth centuries. The logical presumption is that the balance of payments was unfavourable to northwestern Europe.

Professor Reed's discussion of "Deflation and Unemployment" leaves me somewhat puzzled since he does not identify any scholar who subscribes to

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[7] MISKIMIN, *Economy*, p. 87.

[8] MISKIMIN, *Economy*, pp. 140–42 and by the same author, "Monetary Movements and Market Structure — Forces for Contraction in Fourteenth- and Fifteenth-Century England," *The Journal of Economic History*, XXIV (1964), 470–90.

the views that he attacks. His footnote 24 incorrectly claims that I have argued "that the inflation of the sixteenth century returned England to a state of full employment." Had he read more carefully, he would, perhaps, have noticed that in the passage cited, I stated only that there was "less than full employment of *productive resources*" prior to 1500.[9] At issue was the abundance of waste and abandoned land left vacant or underemployed as a result of the continuing demographic decline during the century after the first appearance of the Black Death. Few historians would dispute the existence of such unemployed land. My argument noted that the availability of such waste land was a factor in holding back inflation when the money supply began to grow at the end of fifteenth century. As population and the money supply recovered, land was brought back under cultivation and because land was available and population growing, the initial impact of additional bullion and money was not inflationary; instead, it acted as a stimulus to real economic growth.[10] Inflation was not the cause of full employment of productive resources, but rather a consequence of the continuing expansion of the money supply (and its velocity) beyond the moment when full employment of productive resources had been attained.

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To go further in answering Professor Reed would simply be to repeat what I have already written elsewhere. It seems wiser to place my trust in the evaluation and judgment of my colleagues who will surely turn to the written evidence before forming their opinions. However, Professor Reed's approach to history is not unique, so a word on that subject is perhaps in order, since his is the counsel of despair. There is, as he points out, no doubt that the historical evidence that remains to illuminate the later middle ages is deeply flawed. The record will never be so clear as to dispel all reasonable doubt. Equally, the fertile brains of the economists are rich in alternative models which will allow varying conclusions depending on the assumptions, parameters, and theoretical selections of the historical investigator who deploys them. Must we, therefore, surrender to the historical pyrrhonists and concede that all is indeterminate and historical knowledge impossible? I would hope not. Certainty may well be impossible; it is not even claimed by the most advanced scientific disciplines since they deal in statements of greatest likelihood and highest probability. Historians, especially medieval historians, would be injudicious to expect more. After we have assembled as much evidence as possible, it is our duty to study that evidence and to attempt to make sense of it. If we are fortunate enough

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[9] H. A. Miskimin, "Agenda for Early Modern Economic History," *The Journal of Economic History*, XXXI (1971), p. 182.

[10] This argument is more fully developed in H. A. Miskimin, *The Economy of Later Renaissance Europe, 1460–1600*, Cambridge, Cambridge University Press, 1977.

to find a body of economic theory that aids in the enterprise, so much the better. The measure of our success must be, as in science, whether we have provided the least cluttered means of accounting honestly for the most evidence. Refutation must occur when new evidence cannot be incorporated within the existing structure, and such refutation should be welcomed as progress. To argue, as Professor Reed does, that the evidence is flawed and that, under certain assumed conditions, portions of a whole may be indeterminate, is to argue that there is no valid choice between Ptolemaic and Copernican descriptions of the universe.