Two Comments on Manning. "The Enslavement of Africans: A Demographic Model," CJAS/RCEA, 15 (1981), and a Response.

Nous avons invité un démographe, John Caldwell, et un historien, J.E. Inikori, à commenter l'article de P. Manning qui est paru dans le dernier numéro de la Revue. Nous sommes contents de publier ces deux commentaires en vue de stimuler le débat sur cette question fondamentale. Une réponse de Manning suit.

We have invited John Caldwell, a demographer, and J.E. Inikori, an historian, to comment on the article by P. Manning in the last issue of the Journal. We are pleased to publish these two comments as a contribution to the debate on this important subject. Manning's rejoinder follows.

John C. CALDWELL*

This paper is certainly the best attempt to date to estimate the impact of slavery upon African populations. A decade ago I made a more limited attempt when writing a chapter for an as yet unpublished volume of the UNESCO history of Africa. Manning has now convinced me that, like Fage,[1] I may have overestimated the demographic recuperative power of African society. Yet there remain problems which we should identify.

The first is that neither Fage's nor anyone else's retrospective population estimates based on likely rates of population growth are worth anything. Population projections of future growth tend to be extremely unreliable even when we are in a position to posit likely mortality trends with a fair degree of accuracy. In contrast, one can develop an argument quite easily to support a series of plausible birth and death rates that will yield a population for the whole continent at the beginning of the eighteenth century of either 100 million (the figure in the United Nations' *Determinants and Consequences of Population Trends*[2] based on Willcox's[3] naive belief that the seventeenth century Italian Riccioli[4] knew much about Africa's demography) or 40 million. If we are to get closer to the truth, it will not be through such exercises but through estimates of land occupation and of crops grown. By these methods, based on the agricultural evidence of authorities such as

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1. John D. Fage, "Slavery and the Slave Trade in the Context of West African History," *Journal of African History* 10 (1969): 393-404; John D. Fage, "The Effect of the Export Slave Trade on African Populations," in *The Population Factor in African Studies*, eds. R.P. Moss and R.J.A. Rathbone (London: University of London Press, 1975), pp. 15-23; John D. Fage, "Slaves and Society in Western Africa, c. 1445-1700," *Journal of African History* 21 (1980): 289-310.

2. United Nations, Department of Economic and Social Affairs, *The Determinants and Consequences of Population Trends: New Summary of Findings on Interaction of Demographic, Economic and Social Factors*, Population Studies, No. 50, Vol. 1 (New York, 1973).

3. Walter F. Willcox, "Increase in the Population of the Earth and of the Continents since 1650," *International Migrations, Vol II, Interpretations* (New York: National Bureau of Economic Research, 1931), pp. 33-82, 640-644.

4. B. Riccioli, *Geographie et Hydrographiae Reformatae* (Bologna, 1661; Venice, 1672), "Appendix: De verisimili hominum numero," pp. 630-634.

Marvin Miracle[5] and on the testimony of European sailors on the use of the West African forest and the foods eaten there, one can argue more plausibly for the latter than for the former figure.

The next worry is that the argument depends so much on the fraction of females killed or caught and on what happened to them after capture. Manning's footnotes are not convincing, and other evidence suggests that he may have overstated the number of females removed from the region.

Perhaps the weakest and most unnecessarily stressed part of his argument is the claim that a woman who enters a polygynous marriage will have decisively lower fertility than she would have exhibited if she had been married monogamously. There is no agreed evidence for this. We have found no fertility differences by type of marriage in either Ghana or Nigeria, and for the latter country this has been confirmed by the work of Ohadike, Orubuloye and Okore.[6] Furthermore, we would expect to find some differences because the infecundity or subfecundity of one wife is a reason for taking another, and because some polygynous marriages are levirate ones where the widow of a relative may be taken by a man as a form of social welfare not always extending to sexual and reproductive services. Admittedly, Page, in a chapter in *Population Growth and Socioeconomic Change in West Africa*[7], has listed a greater number of surveys showing lower fertility in polygynous than in monogamous marriages than surveys showing the opposite, but the differences do not appear to exceed those one would anticipate from the above argument. The data presented (in f.n.25) from the Economic Commission for Africa are worthless because they were not standardized for age, and, indeed, the differentials in current fertility are almost certainly explained by the older average age of the polygynously married women (perhaps half of whom were once younger mono-gamously married women). Muhsam's classic paper[8] is irrelevant, both because it is based on the Middle East where polygyny is relatively rare compared with sub-Saharan/Africa, and because his data may be largely explained by the subfecundity of the first wife. In the Changing African Family Project and the Nigerian Family Study, we have not even been able to establish any difference in the duration of post-natal female sexual abstinence between the two types of marriage.

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5. Marvin P. Miracle, "Murdock's Classification of African Food Economies," in *Reconstructing African Cultural History*, eds. Creighton Gabel and Norman R. Bennett, (Boston: Boston University Press, 1967), pp. 201-225.

6. P.O. Ohadike, "Patterns and Variations in Fertility and Family Formation: A Study of Urban Africans in Lagos, Nigeria," (Ph.D. thesis, Australian National University, 1967), p. 266; I.O. Orubuloye, "Fertility, Sexual Abstinence and Contraception among the Yoruba of Western Nigeria: A Study of Selected Rural Communities in Ekiti and Ibadan Divisions," (Ph.D. thesis, The Australian National University, 1977), p. 119; A.O. Okore, "The Value of Children among Ibo Households in Nigeria: A Study of Arochukwu Division and Urban Umuahia in Imo State," (Ph.D. thesis, Australian National University, 1977), p. 234.

7. Hilary Page, "Fertility Levels" in *Population Growth and Socioeconomic Change in West Africa*, ed. John C. Caldwell (New York: Columbia University Press, 1975), pp. 29-57.

8. H.V. Muhsam, "Fertility of Polygamous Marriages," *Population Studies* 10 (1956): 3-16.

Similarly, it is untrue that there has been compellingly convincing documentation of the tendency for families to replace lost children other than by the automatic biological mechanism whereby the death of an infant may terminate lactation and subsequently amenorrhoea or by the social mechanism whereby, after a proper interval, post-natal abstinence is terminated earlier than it would be if the child had survived. In neither case is intentional replacement involved.

However, these points are trivial compared with the question as to whether other forces were acting upon population growth and, if so, whether they were independent of slaving.

The evidence appears to be that the New World crops, maize and manioc, and, in more limited areas, beans, spread throughout the century and ever since. Their use, plus that of rice, and a greater willingness to grow yams in the wet forest, led to an increasing invasion of the forest. This was part of the change in balance over the millennia between hunting and gathering and agriculture, for neolithic revolutions do not happen all at once (and certainly did not do so in Africa).

The evidence, at least from the increasing penetration of the forests, seems to be that African population probably grew considerably during the eighteenth century, almost certainly faster than in any previous century. Yet Manning has shown that it would have grown even faster, unless, of course, slaving itself indirectly abetted population growth or unless the losses from slaving were automatically made up. The latter would be the case in the Malthusian situation, whereby population size through starvation and disease reinforced by malnutrition determined the death rate. It has not yet been convincingly demonstrated that this was not wholly or partly the case. The former would occur if the European presence, or the greater economic activity arising from slaving, led to the more rapid adoption of new crops or the use of tools better adapted for slashing forest, or if it increased urban or elite populations demanding more food from expanded agricultural activities, or if the importance of the coast in commerce accelerated the invasion by farmers of land close to it. Perhaps firearms allowed the more economically aggressive to move into land previously occupied by hunters and gatherers or more conservative farmers. On the other hand, the gains from slaving may have raised the level of warfare and concomitant deaths. In an unsettled community with greater external contacts, sterilizing disease may have spread as it apparently did in the late nineteenth and early twentieth centuries in the Congo Free State and the French Congo (touched upon in my UNESCO chapter, and now being much more convincingly established by David Voas at Cambridge).

The debate will be settled finally only by evidence of population densities, and my reading of the scattered evidence that already exists is that they will establish population growth. The investigators who do demonstrate such population growth will have to take into account Manning's proof that it occurred despite the potential for population erosion arising directly from slaving.

A final point. The extent of polygamy is determined not only by the female-male

balance, but also by the degree of insistence on widow remarriage, the average age difference between spouses, the rate of population growth, and the mortality schedule. There are pronounced regional cultural differences in the degree to which widow remarriage is demanded. Polygyny is flexible and in Ghana, as Aryee and Gaisie have shown, is at present increasing within each separate female educational group while remaining constant in the whole population and maintaining differentials by education.[9]

J.E. INIKORI*

In works written since 1975,[1] I have tried to confront, directly or indirectly, the main arguments of John D. Fage on the slave trade and African societies.[2] It was reassuring to read Patrick Manning's paper based on an entirely different method but with conclusions basically identical to mine.[3] While Manning directs his paper as a counter to Fage's

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9. A.F. Aryee and S.K. Gaisie, "Fertility Implications of Contemporary Patterns of Nuptiality in Ghana," *Nuptiality and Fertility*, ed. L.T. Ruzicka (Liège: Ordina, 1981), pp. 287-304.

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1. My main works dealing with this subject are as follows: "The Slave Trade and the Atlantic Economies, 1451 - 1870," in *The African Slave Trade from the fifteenth to the nineteenth century* (Paris: UNESCO, 1979), pp. 56-87; "Slave Trade: A Retardative Factor in West African Economic Development," in *The Economic History of the Central Savanna of West Africa*, Mahdi Adamu, ed. (in press); *Forced Migration: The Impact of the Export Slave Trade on African Societies* (London: Hutchinson, in press), Editor's Introduction; "Under-population in Nineteenth-Century West Africa: The Role of the Export Slave Trade," in *African Historical Demography II*, (Edinburgh: Centre of African Studies, University of Edimburgh, 1981), pp. 283-313.
2. J.D. Fage, "Slavery and the Slave Trade in the Context of West African History," *Journal of African History*, 10 (1969): 393-404; "The effect of the export slave trade on African populations," in *The Population Factor in African Studies*, R.P. Moss and R.J.A.R. Rathbone, eds. (London: University of London Press, 1975), pp. 15-23; *A History of Africa* (London: Hutchinson, 1978), part 3; "Slaves and Society in Western Africa, 1445 - 1700," *Journal of African History*, 21 (1980): 289-310.
3. Patrick Manning, "The Enslavement of Africans: A Demographic Model," *CJAS/RCEA*, 15 (1981). My main argument is that Fage's calculations and the quantitative conclusion based on them are affected by three elements: Curtin's low figures that form their base; failure to take into account the effects of the trans-Saharan slave trade and the large additional population losses directly and indirectly connected with the export slave trade; improper consideration of the number of descendants the people exported would have produced in Africa had they not been exported. My conclusion is that contrary to Fage's view, the population of West Africa declined absolutely from 1650 to 1850. See my "Introduction" to *Forced Migration* for the details.

argument in general, a substantial part of the paper relates directly to the specific analysis of John Thornton which seeks to provide empirical support for Fage's conclusion on the demographic impact of the export slave trade on sub-Saharan Africa.[4] Since I am in complete agreement with Manning's main conclusions, my comments are intended to show whether Manning's model provides an effective counter-argument to the analysis it is intended to refute. Where necessary a more effective counter argument is suggested.

I

Fage's central argument is contained in his view on the dynamics of internal slavery and slave trade in Africa. Fage holds that the growth and development of internal slavery and slave trade in Africa was a product of autonomous historical processes within Africa. In Fage's view, the export slave trade was an out-growth of internal slavery and slave trade in Africa. In other words, the export slave trade was incidental to African internal slavery and slave trade.

> The prime motive for warfare and raiding in Africa, then, was not to secure slaves for sale and export, but to secure adequate quantities of this resource and to diminish the amounts available to rivals.[5]

This argument has a far-reaching implication for Fage's calculations and conclusions relating to the demographic effects of the export slave trade. The all important but unstated implication of this view is that population losses — directly through deaths during wars and raids, and indirectly through increased mortality arising from political, social and economic disruptions — cannot be attributed to the export slave trade. This is logical if it can be proved that slave raiding and wars related to the gathering of captives were not stimulated by export demand but by internal needs.[6] This explains why Fage does not include in his calculations any serious consideration of population losses arising from slaving operations.

Manning argues, and tries to demonstrate with his model, that contrary to Fage's view, "the level of external demand and the changes in external demand were the main factors conditioning the course of slavery on the African continent."[7] But does the model provide an unambiguous and effective counter to Fage's argument?

Manning's model demonstrates quantitatively the process through which the number of slaves held in different parts of sub-Saharan Africa grew over time.[8] While the model

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4. John Thornton, "The Slave Trade in Eighteenth Century Angola: Effects on Demographic Structures," *CJAS/RCEA*, 14 (1980): 417-427.

5. Fage, *A History of Africa*, p. 269.

6. See my "Introduction" to *Forced Migration*, where it is argued that slave raiding and wars related to the gathering of captives were stimulated by export demand for captives.

7. Manning, "A Demographic Model."

8. Manning, "A Demographic Model," Tables 1 and 2.

does not explicitly discuss natural increase among the slave population, it shows that additions other than natural increase came from the same pool of captives from which exports were taken. The model also demonstrates where in sub-Saharan Africa one would expect the slaves to be: slave receiving and exporting territories (the "Raiders") as opposed to the victim areas (the "Raided").

Many of the hypotheses built into the model can be verified empirically. In particular, the historical evidence shows clearly that those areas involved in the export trade were also the areas with large-scale accumulation of slaves for internal use. But while Manning's model shows correctly that captives retained for internal use formed part of the general pool from which export captives were taken, the model cannot demonstrate what constituted the main stimulus for the gathering of captives: export demand or internal need. Using the model it could be argued that internal demand provided the main impetus just as it could equally be argued that export demand was the main stimulus. Having failed to build this issue into the predictive capacity of the model, Manning assumed which of the two possibilities operated historically. To assume away what constitutes the central question in Fage's analysis may be seen as a major weakness in the model.

To prove whether internal need or external demand provided the motive force for the large-scale gathering of captives in sub-Saharan Africa from the fifteenth to the nineteenth century one has to examine the use of internally retained captives. To date, the best analysis of this question is by Claude Meillassoux who has argued that there was a close relationship between the use of internally retained captives and the structures which developed in response to slaving operations.[9] We also need to examine the extent to which the export slave trade, as an agent of under-population, retarded the development of other forms of labour and so encouraged the gathering of captives for internal use.[10] The latter consideration helps to explain why in sub-Saharan Africa slave labour failed to give way to wage labour, as was the case in other major regions of the world where internal slavery and slave trade developed at an early stage of history.

Without consideration of the foregoing points one is likely to argue with an awkward logic. This seems to be the case with Manning:

As the external demand for slaves was gradually shut off, slave prices fell substantially and, in response, the use of slave labour in African production expanded rapidly (p. 501).

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9. Claude Meillassoux, "The Role of Slavery in the Economic and Social History of Sahelo-Sudanic Africa," in *Forced Migration*, Inikori, ed., Chapter 2. See also my "Introduction." The idea that export demand for captives was the main stimulus for the geographical spread and quantitative expansion of slavery in Africa was first developed by Walter Rodney in his paper, "African Slavery and Other Forms of Social Oppression on the Upper Guinea Coast in the context of the Atlantic Slave trade," *Journal of African History*, 7 (1966): 431-443.

10. For the details of this argument see my "Introduction" to *Forced Migration*.

The economics of this argument, in the context of Manning's model, is awkward. If the supply of slaves was strictly an economic activity, as Manning's model presupposes, then the quantity of slaves brought to the market must bear a significant relationship to prices: higher prices give rise to greater quantities and lower prices to smaller quantities. If historical facts reveal a rapid expansion of slave use in African production in the nineteenth century after the elimination of external demand, this calls for a more complex explanation. In fact, without the structural and population argument, Manning's explanation destroys the logical basis for his claim that external demand was the main stimulus for the gathering of captives, and that captives retained for internal use were a mere left-over. If the elimination of external demand led to a substantial decline in slave prices and yet slave gathering increased, then slave gathering could not have depended on external demand previously, except if it can be shown that three centuries of gathering to meet external demand gave rise to structures which encouraged the "production" of captives while at the same time substantially increasing internal demand.[11]

II

Since Thornton's conclusion based on analysis of Angola's census data for 1777 and 1778 constitutes so far the strongest support for Fage's demographic argument, Manning's critique of Thornton is the peak of his refutation of Fage's analysis. Thornton's central argument is that the export slave trade led to a substantial increase in crude birth rates of African societies because of "unbalanced sex ratios." According to Thornton, this increase in crude birth rates more than off-set the depopulation effects of forced migration, so that the population of areas from which most of the export slaves were taken is greater today than it would have been without the export slave trade.[12]

With a few additional inferences from his model, the points in Manning's counter argument may be summarized as follows. The central argument is that the surplus of females over males arising from the selective removal of population by the Atlantic slave trade was a phenomenon restricted to only a few areas of sub-Saharan Africa. It was not present in the victim areas from which captives were taken, because the process of capture did not discriminate between the sexes at the point of capture. The sex imbalance occurred only in the captive receiving and exporting territories in the coastal region where the surplus females were in fact captives retained as slaves after the export

11. Lovejoy and Klein actually argue that the existence of large external demand stimulated the development of large-scale slave "producers" in Africa, state governments, and this involvement of states, with their acquisition of characteristics adapted to the gathering of captives, lowered the cost of slave "production" and the prices at which they were offered to the consumers. Martin Klein and Paul E. Lovejoy, "Slavery in West Africa," in *The Uncommon Market: Essays in the Economic History of the Atlantic Slave Trade*, Henry A. Gemery and Jan S. Hogendorn, eds. (New York: Academic Press, 1979), p. 201.

12. Thornton, "The Slave Trade in Eighteenth Century Angola," p. 427. As Thornton puts it, "modern population densities might bear no relation at all to the intensity of the slave trade. Indeed, we can suggest that coastal areas of West Africa are so densely populated today partly because of the slave trade, and not, as some have suggested, depsite it."

numbers had been sold. Being slaves, the age-specific fertility of these surplus females was drastically reduced for reasons related to well-known facts. First, these female slaves ended up, at best, in polygynous marriages which are generally known to reduce fertility significantly. Second, since the number of male slaves in these societies was strictly limited, a large proportion of these female slaves may have remained unmarried for much of their child-bearing period, reducing their fertility further.[13] Third, the strenuous and degrading conditions of slavery which seriously reduced the fertility of slaves in the New World and North Africa may also have operated among the female slaves in sub-Saharan Africa with similar effects. The overall effect was to drastically reduce fertility of the surplus females in the coastal slave receiving and exporting areas. Hence, while the crude birth rates may have increased somewhat because of the relatively larger number of young women, the increase was minimal. At the same time, however, victim areas in the coastal region from which the captives were taken, having lost a relatively larger proportion of females within the child-bearing ages, suffered a decline in crude birth rates. This latter decline would wipe out the minimal gains recorded in the captive receiving and exporting areas of the coastal region. If the initial population ratio, "Raiders" to "Raided," was 50:50, the crude birth rate of the "Raiders" and "Raided" (in the coastal region) combined would be slightly reduced. But if the "Raided" had much larger population initially (say, 25:75, "Raiders" to "Raided"), then the decline in the combined crude birth rate of the two sectors of the coastal region would be greater.[14] In the northern savanna and parts of East Africa which supplied slaves to the Muslim World, crude birth rates declined both in victim areas and in captive receiving and exporting areas: first, because more females than males were exported; and second, because the females exported were predominantly from the most fertile age-groups (15-30). Relating this analysis to the estimated number of slaves exported, Manning concludes that, contrary to Fage and Thornton, the export slave trade not only restricted net population growth in sub-Saharan Africa, but also, led to depopulation.

A crucial point about the critique of Thornton is that Manning is not aware of the serious discrepancy between the sex ratios which his model predicts for the slave receiving and exporting areas of the coastal region and those shown by Thornton's census data for Angola. If the prediction of Manning's model is correct, then sex imbalance arising from the selective export of slaves in the coastal region should be found among the slave and not the free population. However, the census data presented by Thornton in Table 2 of his paper show the exact opposite in the age group 7-15, and only a slightly

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13. This point, though made about slaves in the Middle-East, is not explicitly made by Manning about the slaves in the coastal region of sub-Saharan Africa.

14. This point is an inference from Manning's model. It was not explicitly made. The logic of the point is that the decline in the crude birth rate among the "Raided" on the coast is much greater than the gains in birth rate made by the "Raiders." This can be easily demonstrated. While the "Raided" suffered the full loss in number of births proportionate to the number of young women removed, the "Raiders" only recorded a fraction of what the "Raided" lost in births: first, because of the reduction in fertility among the female captives retained by the coastal "Raiders" for reasons stated earlier; and second, because some of the young women lost by the "Raided" were exported by the "Raiders."

greater sex imbalance for the slave population in the age group 15-60.[15] It may, therefore, be inferred that either the prediction of Manning's model is wrong or there is something wrong with Thornton's census data. From all indications Manning's analysis in this respect is quite close to historical reality. Historically, it is realistic to say that sex discrimination did not occur at the point of capture. Hence, there is no reason to expect a serious sex imbalance among the uncaptured free populations in the victim areas. Similarly, the export slave trade could not have caused sex imbalance among the free populations in the captive receiving and exporting areas on the coast. But, since the Atlantic trade took most of the male and left most of the female captives, this selective process ought to produce a sex imbalance among coastal slave populations as a result of the regular absorption of captives, who were predominantly female. The indication, therefore, is that the discrepancy is due to inaccuracies in the Angolan census data of 1777 and 1778. In fact, the indication is further strengthened by what Thornton says about the Angolan census of 1778. He points out that the population of free males in the 1778 census "is drastically out of line with the remaining figures," and so he considers it "well advised to dismiss it as having been mistakenly constructed, perhaps due to mistranscriptions.[16] This raises considerable doubt about the accuracy of births and birth

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15. Thornton, "The Slave Trade in Eighteenth Century Angola," p. 422. While the low ratio of male to female slaves in the age group 15-60 (43,6/100 and 40,6/100) can be explained in terms of the Atlantic slave trade, the similarly low ratio among the free population within the same age group (57,8/100 and 55,6/100) cannot be explained in the same way. Nor is there a logical way of using the Atlantic slave trade to explain the large imbalance in the free population and the near balance in the slave population, in the age group 7-15. Thornton's figure may be compared with those for Luanda in 1773 provided by Herbert S. Klein:

	Free population of Luanda		Total slave population by owner *(Male and Female)*
	Men	Women	
Whites	214	37	787
Free Mulattoes	106	32	187
Free Negroes	143	4	9
	463	73	983

Herbert S. Klein, "The Portuguese Slave Trade from Angola in the Eighteenth Century," *Journal of Economic History*, 32 (1972): 909. Klein does not give a sex breakdown of the slave population. But among the free blacks (excluding the mulattoes) the ratio of males to females is 35,75:1 or 3 575 men to 100 women. It is difficult to reconcile Klein's figures for Luanda with those of Thornton for Angola as a whole. Luanda was the most important slave trading port in Angola in the eighteenth century.

16. Thornton, "The Slave Trade in Eighteenth Century Angola," p. 421.

rates calculated by Thornton from these data. Besides, birth rates calculated for a single year cannot carry the weight of Thornton's argument.[17] On the whole, Manning's argument is well conceived and persuasive.[18]

As to additional population losses arising from slaving operations, the 10 percent assumed by Manning is extremely low when compared with the available evidence. For example, out of 10 442 slaves brought to Mozambique for sale to exporters in 1819, 1 200 died before they could be purchased; of the 9 242 purchased by the Brazilians in that year 1 804 died at Mozambique before the slave ships left. The 3 004 export slaves who died at Mozambique made up 40 per cent of the 7 438 actually exported.[19] For Angola, Joseph Miller says that a local merchant, Raimundo Jalama, knowledgeable about the conditions prevailing in the 1780s, "used as working estimates death rates of 50% during the slaves' march to the coast and 40% (of the remainder) in Luanda."[20] When population losses due to wars directly and indirectly caused by slaving operations are added, it can be seen why Manning's assumed rate is extremely low.[21]

On the issue of Malthusian checks operating to curtail population growth in sub-Saharan Africa in the absence of the export slave trade, Manning's analysis is unenlightening. He confuses demographic crises arising from natural causes with mortality arising from population pressure. Thus he cites the droughts of the 1740s and 1750s in Sahelian West Africa as though caused by population pressure. For a discussion of Malthusian checks in relation to the demographic impact of the export slave trade to be meaningful, we must differentiate between demographic processes arising from the pressure of population on the means of subsistence (agricultural land) and demographic

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17. Without proving that the single year population growth rate calculated for 1777-78 (even if correct) is representative of the long-run growth rate in Angola during the four hundred years of the slave trade in that country, no meaningful historical statement can be based on it, because single years of unusually high or unusually low growth rates can be encountered in societies at a stage of development similar to that of Angola in the 1770s.

18. In relation to this sex imbalance argument, it is important to note that the evidence shows a regional variation in the sex distribution of the slaves exported from different regions of Africa by way of the Atlantic trade. The evidence indicates that the largest proportions of females were exported from Benin and the Bight of Bonny in the eighteenth century. See my "Introduction" to *Forced Migration*, Table 2.

19. Edward A. Alpers, *Ivory and Slaves in East Central Africa: Changing Patterns of International Trade to the Later Nineteenth Century* (London: Heinemann, 1975), pp. 211-215, 225. Alpers states that of 20 000 slaves annually brought to Mozambique for export in the 1820s and 1830s only 15 000 actually left Mozambique yearly for Brazil, the others dying in Mozambique. The 5 000 who died annually in this period represent about 33 percent of those actually exported.

20. Joseph C. Miller, "Some Aspects of the Commercial Organization of Slaving at Luanda, Angola, 1760-1830," *The Uncommon Market*, in Gemery and Hogendorn, eds., p. 103.

21. For a detailed discussion of these additional population losses, see Inikori, "Underpopulation in nineteenth-century West Africa," and my "Introduction" to *Forced Migration*.

calamities due to natural causes.[22] If by Malthusian checks we mean demographic processes strictly related to the pressure of population on agricultural land, then there is no way those checks could have operated in sub-Saharan Africa in the eighteenth century in the absence of the export slave trade. Sub-Saharan Africa as a whole remained a land surplus region throughout the nineteenth and early twentieth century, even with the rapid growth of population and agricultural export production after the elimination of the export slave trade and its attendant social and political disruptions. The population of sub-Saharan Africa would have had to have been several times greater than it actually was under the impact of the export slave trade before the Malthusian checks could have operated.

The export slave trade did not prevent demographic crises arising from natural factors: harvest failures due to climatic change and periodic plagues. In general these factors are independent of population size and density. The indication is that the reduction of African population by the export slave trade was a man-made addition to the periodic population losses imposed by these natural factors on all pre-industrial societies. In fact, as the evidence shows, the social disruption attendant on slaving operations (wars, political instability and general disorganization of societies' economies) both aggravated and stimulated the operation of these natural factors.[23]

III

To conclude, it should be said that Manning's paper is a timely and well-conceived contribution to the debate on the demographic impact of the export slave trade on African societies, weaknesses notwithstanding. By implication, Manning's and Thornton's analyses show acceptance of the fact that a satisfactory measurement of total population loss suffered by sub-Saharan Africa as a result of the export slave trade must include:

a) the number of descendants that would have been produced in Africa by those exported had they been left in Africa;

b) population loss due to reduced fertility of the women left in Africa, as a result of the structural transformation of African population;

c) population losses arising from the wars and raids connected with slaving, and from mortality among the export captives between the time of capture and the final departure.

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22. D.B. Grigg has pointed out that "the major criticism of Malthus' system is that he never makes clear the relationship between population growth, the positive checks and the means of subsistence." *Population Growth and Agrarian Change: An Historical Perspective* (Cambridge: Cambridge University Press, 1980), p. 13. In his discussion of demographic processes in Western Europe in the Middle Ages, Grigg differentiates between demographic crises due to natural causes, such as climatic change and plague, and those due to population pressure.

23. For a more detailed discussion of these issues see Inikori, "Under-population in nineteenth-century West Africa," and my "Introduction" to *Forced Migration*.

This is a welcome development. However, it is suggested that future contributions to the debate should adopt a more comprehensive framework. By this I mean that a discussion of the demographic impact of the export slave trade on Africa would be more satisfactorily done if carried out within a broader context of an effort to explain under-population in Africa in the late nineteenth century.[24]

Patrick MANNING*

The comments of John C. Caldwell and J.E. Inikori on the demographic model I have proposed are thought-provoking.

The main question separating Caldwell and me is the size of the pre-slave-export African population, especially in West Africa. Caldwell assumes (based on agricultural and archaeological evidence) a low African population in the seventeenth century. He deduces, therefore, that it must have grown rapidly in the eighteenth century — despite slave exports — in order to reach the level known for the late nineteenth cetury. His reasoning implies African net population growth rates ranging from 0.5 percent to nearly 1.0 percent per year, which would rate rather high on a world scale, especially for the eighteenth century.

I believe, on the other hand (based on a different reading of the same evidence, but also on linguistic and political evidence) that African population was relatively dense in the seventeenth century. This assumption leads more easily to the conclusion that African population growth may have been reversed by slave exports. We may reasonably hope for new evidence on agricultural change and population density to resolve this issue. For example, did New World crops (e.g., maize) present a decisive advantage in productivity over African crops (e.g., yams) and, if so, was this impact felt in advance of or concurrently with the impact of slave exports?

On a second issue, I must concede that Caldwell and others are correct in arguing that polygyny had little significant effect on fertility. I continue to believe that polygyny itself was much reinforced by slavery, and that slavery led to the creation of very large harems in the eighteenth and nineteenth century, within which fertility was almost certainly low because of reduced sexual contacts. Nevertheless, I must acknowledge that these harems, however large, were of little overall demographic significance.

But as this factor fades from my argument, another remains in its place: the subjection

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24. See Inikori, "Under-population in nineteenth-century West Africa," where this has been attempted.

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into slavery may have had a significantly negative impact on women's fertility. This was demonstrably the case among many New World slaves, and Claude Meillassoux has collected a series of analogous examples for Africa.[1]

Joseph Inikori, meanwhile, argues that my presentation assumes, rather than proves, that export demand generated the institutions of slavery in Africa. That is precisely true: I have presented a largely deductive model, working outward from an assumed export demand, and have argued that the results of this modeling show a remarkable similarity to the empirical descriptions of African slavery. The project of *proving* that African slavery was generated by export demand is a matter of an entirely different order, and will be a tall order.

Inikori is also correct in noting an incompleteness in the explanation I offer as to why the supply of African slaves should have continued at a high level even when prices fell sharply in the nineteenth century. In a separate study, I am attempting to address this problem, by working from the assumptions (1) that each African group faced a socially-conditioned decision about whether to participate in slave trade, and (2) that there existed a systematic distinction between the low prices of slaves at the point of capture and the higher prices once they had been moved some distance away.

Finally, though I am no great admirer of Malthus, I must spring to his defense against Inikori's distinction between a "Malthusian" check caused by excess population and a non-Malthusian impact of disease and drought on population. One strong point of Malthus's analysis is precisely his linking of these two issues: he argues that rapid growth of population, even in a region of land surplus, can lead to crisis and epidemics, and can exacerbate the effect of (though not cause) drought.[2] All these may legitimately be labelled Malthusian checks, and in any case they must, as Inikori indicates, be studied along with enslavement to give a fuller picture of the patterns of change in African population: Joseph C. Miller and Jill R. Dias are two additional scholars working on these issues.[3]

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1. Claude Meillassoux, "Female Slavery," in *Female Slavery in Africa*, eds. Martin A. Klein and Claire Robertson (forthcoming).

2. Thomas Malthus, *Essay on the Principles of Population*, 4th ed. (London, 1807), pp. 139, 168 and 169, 174-175.

3. Jill R. Dias, "Famine and Disease in the History of Angola, c. 1830-1930," *Journal of African History* 22 (1981): 349-78; Joseph C. Miller, work in progress.