As ever, I enjoyed reading and being instructed by Heinz Arndt. His conclusion is, of course, wrong. James Meade pointed out some years ago that while economics was to be called the dismal science, he understood that it is now called the gay science. (As James is the original innocent abroad no one was quite sure what he really meant.)

Since I call myself a Post-Keynesian, principally of the Kaleckian-Robinsonian variety, and as I regard what I try to do as political economy, I had best explain why the late Eric Russell used to tell of these economists who gave advice as they stepped off the plane because they already had in their minds a model of what the world – all worlds – were like and so knew immediately what policies should be followed. By contrast, if Kelecki was asked such questions as he got off a plane, he would say: "How can I answer, come back in six months' time when I have had a chance to absorb the historical, institutional, political, and sociological characteristics of your society. Then I may be able to answer your questions." Joan Robinson (I believe that in the end she was more influenced by Kalecki than by anyone else) would say: "I have come to learn, not to teach." They both were exemplifying a "horses-for-courses" approach, letting the specific characteristics of time and place talk to them before they attempted to model, explain and offer policy advice. They did not beg questions nor look at the world with a blinkered gaze – there must be an equilibrium out there (perhaps more than one), and it (or they) must be examined to see if it is (they are) locally or globally stable, the presumption being that usually it (they) would be. Rather, like Kaldor, they were on the lookout for stylised facts – empirical regularities – and simplified (so as to be manageable) versions of observed behavior by decision-makers which could be included in specific models in order to explain and so possibly lead to advice. This usually meant a return to the boundaries more generously drawn by our classical pioneers, as Maurice Dobb put it, rather than to take a very narrow view of what constituted the economic aspect of the problem.

Kalecki, in particular, allowed political and social phenomena to mingle freely with what the more orthodox would regard as economic factors proper.

A good example of what I have in mind is Eric Russell’s non-neoclassical notes for what became in James Meade’s version of their 1957 joint paper, a neoclassical model of how the distribution process worked in Australia, as between farmers, wage-earners and manufacturers, following an external shock, e.g. a rise in wool prices. Or if I may take an example from my own work, very different predictions are obtained about how business people might react to investment incentive schemes, according to whether the model is built up from a simple axiom concerning behavior, or from a simplified adaptation of the actual behavior of business people with regard to the aspect of the investment decision associated with the choice of technique. In this approach, the usual distinctions between fact and theory, ideology and analysis, existence and stability, and even trend and cycle disappear. Moreover, following Keynes, there is a host of relevant languages running from intuition and poetry through lawyer-like arguments to pure mathematics and formal logic, which are appropriate for different issues, and for different aspects of the same issues. We may have to be content with arguments that are merely plausible rather than clinching, as Frank Hahn now reminds us.

If we may judge from what Heinz Arndt wrote in his paper, I don’t suppose he would quarrel with much of what I have written. But if I am wrong it can’t be helped now (as Dennis Robertson said of his principles lectures published after his retirement) for it is what I believe and what I try to practise as a political economist.