Notes on Reading a Text: A Response to R. Leeson

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R. Leeson presents a caricature of my research on the development of the Phillips curve. He accuses me of fostering a conspiracy theory, making Phillips look unworthy of a chair at the L.S.E., and neglecting Phillips's contributions as a time series analyst. It is impossible for authors to control how readers read their texts. From what I know about readers's reactions to my work, Leeson's reading is idiosyncratic and selective.

Leeson shows no sign of having reflected upon the style of historiography that shaped my research. Leeson's criticisms ignore any consideration of the approach that I took, the questions that I posed, the organizing framework that I adopted, and the method that I used to investigate the history of the Phillips curve. The approach I adopted was that of an historian of science (Popper 1967, 1968). The questions I asked focused on the problem situation in which scientists find themselves. I asked, what were the problems to which Phillips and Lipsey's hypotheses of the inflation-unemployment relation were solutions? Against what general theoretical frameworks did Phillips and Lipsey raise their research problems? What knowledge was available to them? What training, scientific publications, and contemporary arguments informed their understanding of the problem they posed? What technical skills and tools did Phillips and Lipsey wield? To answer those questions, I cast the mold of my entire 1987 essay in the form of Lakatos' methodology of scientific research programs. The main historical method that I used was a sympathetic repetition of the original research. I took my task to be, in sum, the "historian's task ... to reconstruct the problem situation as it appeared to the agent, that the actions of the agent become *adequate* to the situation" (Popper 1968, 46). An effective criticism of my research would inquire how well I applied my chosen method of historiography and consider how appropriate the historiographic method was to the topic at hand.

I shall respond to Leeson's specific criticisms. My previous essays treated the topics that I raise. This paper summarizes how I treated those topics. Upon occasion, I explicitly cite some of the historical documentation that informed my original narratives. My conclusion emphasises that the crux of the argument between Leeson and myself is less about Phillips's historical importance than about Leeson's personal reading of my texts.

(1) *Did Phillips's choice of intervals matter?* The quotes of my texts that Leeson extracts about my discussion of Phillips's choice of interval may well suggest to some readers that I said or meant that Phillips's attempted to deceive his audience. What I wrote on the subject of the intervals does not call for such a negative interpretation. Phillips divided the unemployment-axis of the scattergraph of money wage inflation and unemployment 1861-1913 into six intervals. I asked, why did Phillips choose the particular intervals that he did? I answered that "a deceptive answer can be drawn from his [Phillips's] statement that 'each interval includes years in which unemployment was increasing and years in which it was decreasing'[17]" I did not say that Phillips's statement was deceptive, as Leeson suggests, I said that a reader who thinks that Phillips's statement answers my question is deceived. I continued that "[t]his statement ... did not explain the choice of the intervals themselves ... Thus the explanation of Phillips's intervals must lie elsewhere." My note 17 stated: "The author thanks

the anonymous referee for this point". Leeson ignores the fact that my essay, like most published research, had more authors than the by-line indicated.

Leeson correctly remarks that I interpreted that polemical issues motivated Phillips's choice of intervals. Leeson takes polemics and science to be mutually exclusive. The history of science and my experience as an economist have taught me that scientists typically do not approach real world data *carte blanche*. Rather, scientists seek to discover in real world data additional support of their favored, well reasoned theories. Scientists often learn most from the way that they constructed, or supported their conclusions, rather than from the conclusions themselves. Phillips hypothesised an inflation-unemployment relation based on his understanding of the behavior of dynamic systems, his knowledge of economic theory, and his insights gleaned from casual empiricism (Wulwick 1987, 836-38). Subsequently Phillips turned his attention to looking for support of his hypothesis in real world data. Phillips's six intervals, as I showed, had the special property that the associated six mean coordinates for inflation and unemployment formed a smooth hyperbolic curve which corresponded to Phillips's presupposed inflaton-unemployment relation (Wulwick 1987, 836-38; 1989, 171, 180).

Leeson says that I emphasized that Phillips constructed his curve by means of a crude statistical method. Phillips himself referred to the method by which he estimated the curve as a "very crude attempt to study the relation [between inflation and unemployment]" (Phillips 1962, 11). The averaging method was particularly crude in light of the fact, as I stressed in my essays, that Phillips was a leading time series analyst. My point was that Phillips cleverly applied a crude method of regression to solve a sophisticated problem.[1] Phillips's preferred specification of the Phillips curve required estimation by nonlinear least squares, which given the technology of the time would have taken him a long time to do. Phillips in the mode of an engineer estimated an approximation. As H. Phelps Brown, Phillips's L.S.E. colleague, explained, Phillips's "mathematical training had been that of an engineer, ... engineers are encouraged to use approximations to get practical results--in those days, to work with a pocket slide rule and the back of an envelope ... Phillips in fact did do something like this in arriving at his graph ..." (Letter from H. Phelps Brown to N. J. Wulwick, dated 8 September 1987). I expand further upon the contribution of Phillips's experiences as an electrical engineer to his approach to the analysis of the real world data in Wulwick 1996. So successful was he that, as I pointed out, modem nonlinear least squares yields estimates of Phillips's equation for 1861-1913 that come close to Phillips's estimates (Wulwick 1989, 182).

(2) *Was Phillips interested in economic policy-making?* Leeson says that I represented Phillips's work as driven by political interests. That Phillips like many macroeconomists was interested in economic policy-making does not mean that Phillips was not a disinterested scholar. Leeson claims that his entire correspondence with Phillips's colleagues fails to hint at Phillips's interest in policy-making. I received ample correspondence from Phillips's L.S.E. colleagues that attest to Phillips's interest in policy-making. For example, Phelps Brown, wrote that the L.S.E. economics faculty looked to Phillips "to apply the techniques of control engineering to macro-economic policy" (Letter from H. Phelps Brown to N. J. Wulwick, dated 24 January 1986; Wulwick 1989, 173; 1987, 841). To take another example, "[i]n his work at the L.S.E.", James Meade told me, "Phillips was ... interested in the whole gamut of problems connected with the modelling, econometrics, mathematical relationships, control problems and down-to-earth political economy issues presented by

dynamic macro-economic models" (Letter from J. Meade to N. J. Wulwick, dated 3 February 1986).

Additional historical evidence, as my essays emphasized, points to the fact that Phillips was interested in economic policy-making. The L.S.E. economics faculty were involved during the mid 1950s in the national debates over the causes of inflation. Drafts of Phillips's 1958 paper circulated through the L.S.E. economics department. R. S. Sayers, another of Phillips's L.S.E colleagues and a member of the Radcliffe Committee on the Working of the Monetary System (1957-59), referred to a draft of Phillips's 1958 paper at the Radcliffe Committee proceedings. So Phillips's paper was in the thick of things.

Leeson says that I distorted a quotation of Phillips which referred to A. J. Brown. Brown, a witness to the Radcliffe Committe, and Phillips discussed their contrasting empirical findings about the causes of inflation. Brown testified to the Committee that his data showed that the inflation-unemployment "relation looked unstable from cycle to cycle ...". But "Phillips did more statistical work than his predecessor", I explained. Then I wrote--appropriately using quotation marks to distinguish Phillips's statements from the interpretation I as historian lent to those statements--that Phillips "crudely fitted a curve to the scatter diagram relating money-wage inflation to unemployment for 1861-1913, which resulted in a strong, nonlinear inflation-unemployment relation. 'It was a rush job', Phillips later admitted to his biographer, C. A. Blyth. Phillips was about to go on sabbatical leave to the University of Melbourne. With the debate continuing, 'it was better for understanding to do it (the study) simply and not wait too long'. After all, he added modestly, 'A. J. Brown had almost got these results earlier'." (Wulwick 1989, 172-73). Having read my 1987 paper, Brown confirmed that "he [Phillips] thought I'd missed a clear relation [between inflation and unemployment] ... I (as you would gather from the Radcliffe Committee evidence you quote) have always been a bit more sceptical" (Letter from A. J. Brown to N. J. Wulwick, dated 21 June 1987).

Phillips's inaugural address (1962) contained down-to-earth suggestions for policy-makers to control inflation. Leeson says that my interpretation that Phillips's policy proposals were premised on what he at the time took to be a "unique and permanent relation" between inflation and unemployment was mistaken. Phillips's method of averaging cancelled out the effect of unemployment changes on estimated wage inflation and to that extent yielded an a-historical inflation-unemployment relation (Wulwick 1987, 840; 1989, 180). Phillips superimposed the curve he estimated by means of regression on the pre-World War I averaged data onto the scattergraphs of data for the interwar and the post World War II periods. On the basis of that curve, Phillips in his inaugural address precisely enumerated the inflation-unemployment trade-off for the purpose of policy-making in the early 1960s (Wulwick 1989, 184). Phillips in his address then offerred suggestions--as Leeson says--"for trying to shift the relation between employment and the rate of rise of wage rates in a way which would make it possible to maintain a higher level of employment with any given speed of inflation". However, Leeson ignores that Phillips reckoned that those suggestions were not "likely to meet with an enthusiastic response". I show in Wulwick 1996 that Phillips's view of a long run curve was reasonable: the modern nonlinear least squares estimates of Phillips's equation for 1948-57 come close to the Phillips curve for 1861-1913.

(3) *How did Phillips distinguish between demand-pull and cost-push inflation?* Leeson reported that Phillips recognized the phenomena of cost push and demand pull inflation. My essays show that that was the case. What Leeson fails to grasp is the unusual distinction that Phillips drew between the two types of inflation. Phillips interpreted that his

curve represented demand inflation while large deviations represented cost inflation, which he identified with rising import prices. In Phillips's own words,

> Assuming that the value of imports is one fifth of national income, it is only at times when the annual rate of change of import prices exceeds the rate at which wage rates would rise as a result of competitive bidding by employmers by more than five times the rate of increase of productivity that cost of living adjustments become an operative factor in increasing the rate of change of money wage rates. ... The purpose of the present study is to see whether statistical evidence supports the hypothesis that the rate of change of money wage rates in the United Kingdom can be explained by the level of unemployment and the rate of change of unemployment, except in or immediately after those years in which there was a very rapid rise in import prices ... Ignoring years in which import prices rise rapidly enough to initiate a wage-price spiral, which seem to occur very rarely except as a result of war, and assuming an increase in productivity of 2 per cent per year, it seems from the relation fitted to the data that if aggregate demand were kept at a value which would maintain a stable level of product prices, the associated level of unemployment would be a little under 2½ per cent ...

My research merely formalized Phillips's argument (Wulwick 1989, 182-83; Phillips 1958, 284, 298-99).[2]

(4) *Why did Phillips switch the money wage time series?* My essays made clear that Phillips carefully detailed the sources of the data that he used and openly discussed the fact that he switched the time series of the money wage index during the 1879-86 business cycle. I never accused or alluded that Phillips was involved in a "conspiracy", as Leeson called it. Phillips in the mode of a scientist openly switched the time series in order to lend greater support to his favored, well reasoned theory of an inflation-unemployment relation. As I showed in 1989, Phillips's 1958 article contained six graphs that showed the effects of unemployment and the changes in unemployment on money wage inflation during each of the six business cycles during 1861-1913. I quoted Phillips, who said that five of the six graphs showed "'a very clear relation between the rate of change of wage rates and the level and rate of change of unemployment, but the relation hardly appears at all in the cycle [for 1879-86] shown in Figure 4 [or Wulwick 1989, Figure 2a]". Phillips was using the time series for money wages in Phelps Brown-Hopkins (1950), which relied on Woods's money wage index. Blaming the inconsistency between his hypothesis and the Woods series on unidentified measurement error (the data, as I wrote, were poor), Phillips substituted Bowley's money wage index in place of Wood's index for 1881-85. Phillips's revised figure for the 1879-86 cycle (his Figure 4a, or Wulwick 1989, Figure 2b) showed--in Phillips's' words which I quoted--the "'typical relation between the rate of change of wage rates and the level and rate of change of unemployment"' (Wulwick 1989, 176-78).

Lipsey was interested in the effect of the data substitution on his estimates of the Phillips curve 1861-1913. I referred to Lipsey, who considered that "in the absence of any evidence favouring one series rather than the other [Bowley or Woods], we cannot eliminate one merely because it does not conform with our hypothesis" (Lipsey 1960, 5). Given his two sets of estimates, Lipsey saw "a noticeable shift in the relationship when [the] equation [with the Woods data--my equation 3] is replaced by [the] equation [with the Woods-Bowley data]" (Wulwick 1989, 179). Leeson correctly points out the data substitution barely changed the nonlinearity, or curvature, of Lipsey's equation. (What happens is that small change in the estimated markedly statistically insignificant coefficient on x^{-2} offsets the big change in the estimated coefficient on x^{-1}) Thus Lipsey's conclusion was incorrect and I should not have relied upon it.

Leeson's conclusion that "Phillips's data substitution did not change noticeably the nonlinearity of his [Phillips's] curve" is incorrect. My nonlinear least squares estimates of Phillips's equation using Phillips's data (including only the Woods wage index), which come close to Phillips's own estimates, are[3]

$$(1)\quad y = \underset{(0.1401)}{-0.8826} + \underset{(0.0)}{8.9387}x^{\underset{(0.0001)}{-1.3836}} \qquad R^2 = 0.64 \quad \text{D-W} = 0.78$$

My estimates based on the combined Woods-Bowley series are

$$(2)\quad y = \underset{(0.1026)}{-1.2136} + \underset{(0.0)}{9.1462}x^{\underset{(0.0002)}{-1.2262}} \qquad R^2 = 0.64 \quad \text{D-W} = 0.85$$

The two equations yield big differences in the predicted values of money wage inflation. Equation 2 in the unemployment range $2 < x < 4.5$ has a noticeably bigger curvature than equation 3. But those results and the whole issue of the effect of the data substitution on the estimates of Phillips's equation 1861-1913 had no bearing on Phillips's problem. Phillips focused solely on the effect of the data substitution on the inflation-unemployment scattergraph for the 1879-1886 trade cycle and gave only one set of estimates for the curve 1861-1913, which were based on the Woods series (Wulwick 1989, 177, 182).

(5) *Was it good practice to keep statistically insignificant coefficients in a regression equation?* Leeson defends Lipsey's specification of the Phillips curve which yielded statistically insignificant estimates of coefficients on some of the unemployment variables. The two statistical studies by A. Sleaman and J. Thomas on which Leeson based his defense were written more than ten years ago and remain unpublished. As it happens, I never received copies of those two studies.

I reestimated Lipsey's equation using Phillips's data for 1862-1913 and arrived at results very close to Lipsey's original estimates:

$$(3)\quad y = \underset{(0.0594)}{-1.1389} + \underset{(0.0810)}{5.4182}x^{-1} + \underset{(0.2228)}{3.8040}x^{-2} \qquad R^2 = 0.64 \quad \text{D-W} = 0.78$$

Like the two studies to which Leeson referred, I found that the F-test indicates that the probability of getting an F-statistic as large as one does if the true coefficients on both x^{-1} and x^{-2} are zero is small.[4] But that result does not mean that it was necessary or appropriate to retain x^{-1} and x^{-2} in the equation. An economist in 1960 could have ascertained that the precision of the estimated effect of unemployment on inflation increased upon excluding x^{-2} or x^{-1} from equation 3--

$$(4)\quad y = \underset{(0.0)}{-1.7328} + \underset{(0.0)}{8.9834}x^{-1} \qquad R^2 = 0.63 \quad \text{D-W} = 0.78$$

$$(5)\quad y = \underset{(0.47)}{-0.1631} + \underset{(0.0)}{9.0151}x^{-2} \qquad R^2 = 0.62 \quad \text{D-W} = 0.83.$$

--and that Lipsey's estimates (my equation 3) created a markedly underestimated money wage inflation 1861-1913.

Leeson also stated that two unpublished papers he has from the early 1980s show that "both the inter-war and the 1948-57 unemployment variables also appear to achieve joint statistical significance". I guess that the two papers referred to Lipsey's equation (13) (Lipsey 1960, 26). Lipsey (1960) reported the estimated coefficients of equation 13 and the standard errors of the coefficients. Therefore, it is possible for me to ascertain that each of the unemployment variables x^{-1}, x^{-2} in Lipsey's Phillips curve equation for 1923-39 and 1948-57 were statistically insignificant at the 5 percent level.[5] Lipsey did not give enough information for me to calculate the joint statistical significance of the two unemployment variables. No economist to my knowledge knows what time series Lipsey used to estimate the Phillips curve equation for the period 1923-57 (Wulwick 1996). Without Lipsey's data, I cannot test the hypothesis of the joint statistical significance of the unemployment variables implied by his estimated equation. In any event, British economists in around 1960 did not routinely report the results of hypothesis testing, such as the test of exclusion (Wulwick 1989, 175; Gilbert 1989; 1991, 293, 301).[6]

I gather that Leeson views me as criticising Lipsey personally, a task for which he seems me as too imperfect to perform. In my view, Leeson confounds historical agents with the human beings who bear identical names as the agents. The personal lives of those human beings involve continuity. In contrast, historical agents exist in discontinuous historical conjunctures dominated by certain research problems. The Lipsey in my texts was a major historical character whose econometric research helped me characterise the econometrics of the early post war era in Britain. Lipsey like many of his colleagues in the 1950s taught himself econometrics (Wulwick 1989, 174, 186). Many economists accepted the Phillips curve only after reading Lipsey's 1960 paper, which used OLS, the up-and-coming method of empirical investigation (*ibid.*, 186-87). A description of Lipsey's general approach to the treatment of data encapsulates the conventional practices of his time (Wulwick 1996).

(6) *What did Phillips's Ph.D. thesis and the 1958 essay say about stabilisation policy?* Leeson says that Phillips taught classes at the L.S.E. that stablisation policy was a bad policy to pursue. Leeson neglects to date those classes. I have shown that Phillip's confidence in the effectiveness of stabilisation policy altered between 1950 and the late 1960s (Wulwick 1995).

In the 1950s, Phillips developed a theory of stabilisation based on engineering control theory. The statement of mine that Leeson questions, that according to Phillips, "stabilisation policy required a target and Phillips assumed the target of a constant price-level rather than full employment" was based directly on Phillips's Ph.D. thesis, which I cited (Wulwick 1989, 171). According to Phillips, the economy possessed three types of feedback mechanisms to correct deviations in output, a proportional control (P) which depended on the size, an integral control (I) dependent on the cumulative error, and a derivative control (D) related to the rate of change of the error. Phillips, following the Samuelson-Hansen microeconomic stability equation, made price flexibility a proportional stabilizer. The nonlinear curve later known as the Phillips curve showed the rate of change of the price-level, or the money-wage level, as a varying proportion of the difference between the actual and the equilibrium level of output (*ibid.*). Phillips's confidence in 1958 in stabilisation policy was strong enough to have prompted Phelps Brown to admit that "[w]hat I missed and should have taken seriously, was the force of the [Phillips's 1958] conclusion, that wages and prices would be stabilised if unemployment were maintained at about 2.5%. (Letter from H. Phelps Brown to N. J. Wulwick, dated 24 January 1986; Phillips 1958, 299).

By the late 1950s, Phillips, the control engineer turned economist, began to approach economic control more circumspectly. Having reproduced the time-forms of the lags of the response of production to changes in demand on the electronic simulator at the National Physical Laboratory, Phillips (1957, 1958a) stressed that in many cases fine-tuning could increase cyclical instability. To avoid such errors, he warned policy makers to limit the values of the proportional, integral and derivative correction measures and reduce the delays in implementing these measures. For Phillips (1960, 1967), controlling the economy meant modifying the structure of the system with the aim of reducing the variance of target variables. Yet, this definition implied that one could not predict the effects of a control from an econometric model without first identifying the changes in the underlying structural model that would be caused by the control. Pessimistic about solving this identification problem, which has reappeared in the Lucas critique, Phillips in 1967 accepted a chair in economics at the Australian National University where he devoted his attention to Chinese economic studies.

Leeson entirely ignores the above narrative based on Wulwick (1995).[7]

(7) *Do scientists make mistakes?* Leeson claims that I corrected Lipsey for views that Lipsey never expressed and for an oversight that Lipsey himself recognized and corrected. Neither claim is correct. I emphasized that Lipsey saw serious problems with using the Phillips curve as a basis for stabilisation policy. Lipsey's OLS estimates for the post World War I period suggested that the inflation-unemployment relation was unstable and weak (Wulwick 1987, 841; 1989, 187). Despite Leeson's reading of my essay, there is on that point no difference between our interpretations of Lipsey's views about the Phillips curve.

What I interpreted as an oversight concerned Lipsey's attempt to derive a Phillips curve which represented a long run, general disequilibrium model, from the Walrasian principles conventional at the time (Wulwick 1987, 843-49). It appeared natural in my narrative that some economist would attempt such a derivation, which would be difficult to accomplish. Lipsey's aggregation hypothesis formed an important part of his derivation. He assumed that the inflation-unemployment pattern in the national data is the outcome of the aggregation of identical "immutable ... individual functions" relating inflation y and unemployment x at the level of the regions, $i=2,...,n$.[8] A regional economy is in equilibrium at x_i*, y_i, where, say, $y_i=0$. Because the regional Phillips curves are hyperbolic, an unequal distribution of unemployment makes the aggregate Phillips curve (which relates average inflation y to average unemployment x) lie above the regional functions: At $x*$, $y>0$ and there seems to be no equilibrium point. The lack of an equilibrium point is illusory, I argued. In the Walrasian model, the relation between the rate of change of wages and relative excess demand is symmetrical for positive and negative excess demand (*ibid.* Figure 4). The degree of inequality in the regional distribution of unemployment could not affect aggregate wage inflation. Unemployment alone (that is, without vacancies) evidently was not a proxy for excess demand. Conventional Walrasian microeconomics yielded negatively sloped Phillips functions that were linear *for every level of unemployment.* (Thus aggregating Phillips functions was insufficient to produce a long run disequilibrium situation in the model of the inflation and unemployment.) Lipsey, according to Leeson, in 1978 came to the same conclusion as me about the curvature of the Phillips curve. Hence, Leeson argues, my discussion was redundant and unfair to Lipsey. The discussion by Lipsey (1978) as summarized by Leeson concerns how to derive the slope of the curve only in the range of *high unemployment*. Leeson and I have been discussing two different issues. Thus his criticism is irrelevant.

Leeson views me as finding personal fault with R.G. Lipsey. The Great Man historiography that Leeson promotes does not allow for flaws. So my finding a contradiction in an argument adopted by Lipsey would imply that Lipsey has not been a Great Man, an assessment with personal overtones. In contrast, my historiography has addressed the problem of how economics has developed. From that perspective, Lipsey's attempted derivation of the Phillips curve from Walrasian underpinnings was successful, as I explained, for it performed the initial stages of the research project that M. Friedman pursued to fruition (Wulwick 1987, 849). In general, the process of advancing knowledge involves making mistakes and following dead ends, as the conclusion to my 1987 article emphasised (1987, 854; Janis 1994, 2).

(8) *What did "long run" and "short run" Phillips curves mean in 1960?* Leeson said that my use of the terms long run and short run Phillips curves was anachronistic. According to Leeson, those two terms had no meaning prior to the introduction by M. Friedman (1968) of the long run Phillips curve. In one sense, the distinction between long and short run curves was obvious to economists in 1960: Phillips estimated an inflation-unemployment relation for 1861-1957--that is, over the long run-- while Lipsey's estimates showed Phillips curves that were stable over just a few years--that is, in the short run (Wulwick 1987, 841). Leeson's objection suggests to me that he has not read important contributions to the Phillips curve literature. I discussed at length the research by Desai (1975, 1984), Gilbert (1976) and Brauchli (1972), who discussed the import of the distinction between the long run and the short run Phillips curves in light of their interpretations of the statistical decisions made by Phillips and Lipsey (Wulwick 1989, 184-86). According to Desai and Brauchli, Phillips averaged his data for 1861-1913 in order to construct a timeless relation between unemployment and inflation. In contrast, Lipsey used OLS on the complete data to estimate Phillips curves for different time periods during 1923-57. According to Gilbert, Phillips (1958) averaged his data to overcome the technical difficulties of estimating a curve that was nonlinear in parameters and thus the fact that Phillips averaged the data had no economic implications. The distinction between the long run and the short run Phillips curves assumed an entirely different sense after E. Phelps (1967) and M. Friedman (1968).

(9) *Did Lipsey have political interests?* My history of the Phillips curve confirmed my view that economics is a science in respect to which historians must make the best of the interplay between internal and political factors. I argued that differences over beliefs about the economic role of government propelled the proliferation of Phillips curve models with their contrasting implications for public intervention in the economy (Wulwick 1987, 843, n. 25, 854). In particular, I interpreted that Lipsey's derivation of the Phillips curve aimed for "a long run, general disequilibrium model based on orthodox classical microeconomics [which] implied the efficacy of fiscal intervention" (at the regional level). In that context, Leeson's reference (note 8) to the statement in a letter from Lipsey saying that he never joined a political party is irrelevant. The rest of Leeson's quotation of Lipsey's letter supports my interpretation. Lipsey said that he played a role in an attempt to discredit L. Robbins, a senior faculty member in the L.S. E. department in the latter 1950s, when Lipsey began work on the Phillips curve. According to De Marchi, "Robbins was perceived as invoking 'scientific' economics--pure deductive analysis, without measurement--directly in support of a noninterventionist policy stance ... Lipsey was itching to get into combat against Robbins ... His restlessness, shared by others, was based on political and technical dissatisfaction" (De Marchi 1988, 144-45 and n. 5).

(10) *Why was Phillips appointed to the rank of professor?* Leeson correctly quotes me as saying that "[i]t seems curious" that the L.S.E. promoted Phillips quickly to the position of Professor of Economic Science and Statistics. In the context of the Phillips curve story that I told in 1987, the promotion seemed curious. Indeed, my texts said that Phillips's election in 1958 by the L.S.E. economics department to the Tooke Chair of Economic Science had little

to do with the famous or infamous Phillips curve and that he gained the chair mainly as a consequence of his research on the stability properties of dynamic systems (Wulwick 1987, 841 and 841 n12; 1989, 173).

(11) *What did historians in the mid 1980s believe about the existence of Phillips's papers?* I am delighted that R. Leeson obtained Phillips's papers, which were in the possession of Phillips's widow. Leeson wrote that had I made inquiries, I would not have written as I did that "Phillips left no papers" (Wulwick 1989, 180). In fact, I asked C. A. Blyth, Phillips's leading biographer, whether Phillips left papers. Blyth responded with the answer "regretfully no" (Letter from C. A. Blyth to N. J. Wulwick, dated 6 October 1986). Leeson's misundertanding follows from my failure to cite the Blyth letter. Leeson wonders why historians have not asked Phillips's widow about the existence of papers. Blyth told me that "as far as I can find out his widow has no papers--I presume as a result of the two big departures late in their married life to Australia and then to New Zealand" Evidently, I should have directly asked Phillips's widow about whether his papers existed and, if so, whether I could have access to them.

Conclusion

Let us now turn to Leeson's list of five main specific topics about which he claims I err. (a) Leeson says that scientic research and polemical interests are at odds. My writings, motivated by the modern historiography of science, take scientists typically as motivated by polemical concerns of one sort or another. Phillips and Lipsey were typical in that regard. (b) Leeson claims that I ignored that Phillips recognized cost push and demand pull inflation. I clearly stated that Phillips recognized both phenomena. Leeson fails to grasp the unusual distinction drawn by Phillips between the two types of inflation. (c) Leeson suggests that I denied that Phillips's promotion to full professor made sense. My texts said that Phillips's research on the Phillips curve had little or nothing to do with Phillips's election to a chair and that the L.S.E. department promoted Phillips because of his achievements in a host of fields concerning the dynamical behavior of systems. (d) Leeson stated that "Phillips' data substitution for the years 1881-5 did not noticeably change the nonlinearity of his curve". Leeson's comment is irrelevant since, as I have shown, Phillips considered only the effect of the data substitution on the scattergraph for the 1879-1886 trade cycle. Moreover, it just so happens that the curvature of Phillips's curve 1861-1913 changes noticeably given the data substitution. Prompted by Leeson's criticisms, I further researched Lipsey's discussion of the data substitution, learned that Lipsey mistakenly thought that the data substitution noticeably shifted Lipsey's Phillips curve equation 1861-1913, and recognized that I should not have accepted and cited Lipsey's conclusion on that issue. (e) Leeson says that Lipsey in 1978 noticed that his 1960 article contained an oversight. If so, the section of my 1987 text which pointed out the oversight was redundant. However, what I interpreted as a contradiction in Lipsey's argument and the oversight that Leeson has in mind concern two different aspects of Lipsey's 1960 paper.

In general, Leeson's criticisms of my research rest on an idiosyncratic and selective reading of the texts. That is obvious in respect to my discussions of Phillips's contributions to economics. Leeson censures me as he remarks that Phillips was a "genius". It is true that my essays never have referred to any historical figure by such a celebratory term which typifies the genre of biography of Great Men that Leeson apparently prefers. My essays rather have portrayed Phillips's specific contributions to the economics of his time, which he approached from the perspective of an electrical engineer. As I have said, "'[o]bviously Phillips, as James Meade once remarked, should not go down to history just as 'the Phillips curve' chap'" [Letter from J. Meade to N. J. Wulwick, dated 3 February 1986]. The curve was only a major incident in the general progress of Phillips' studies on time-series modelling and classical economic control, which also is identified with his name" (Wulwick 1995).

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Notes

1. The citations in my 1989 paper indicated that my argument develops the argument of Gilbert (1975), with which Leeson evidently is unfamiliar.
2. Leeson ignores the earlier formalization of Phillips's argument by Desai (1984), which I cited.
3. 2-tailed statistical levels of significance appear in parentheses.
4. $F(44.97445, 2, 50) = 0.009879$.
5. Lipsey reported an estimated coefficient on U^1 of 0.43 and an estimated standard error of 2.1. Then the 2-tailed statistical level of significance is 0.84. Lipsey's estimated coefficient on U^4 was 11.18 and the estimated standard error was 6. Then the 2-tailed statistical level of significance is 0.076 (Lipsey 1960, 26).
6. Leeson, citing De Marchi, states that Lipsey was a falsifier, not a verifier. That is another reason why Leeson says he finds my evaluation of Lipsey's (1960) econometric exercise "strange". According to De Marchi, Lipsey was not a Popperian purist. Lipsey was interested in seeing the extent to which theories were testable. His main interest was simply to quantify theories (De Marchi 1988, 155).
7. I sent Leeson a copy of Wulwick (1995) (originally drafted in 1988) upon two occasions in the early 1990s. When it seemed to me that Leeson made use of material from Wulwick (1995) in his own essays, I wrote to Leeson, stating that "(it) is appropriate for you to reference the encyclopedia article" (Letter to Leeson from Wulwick, dated 6 June 1993). Leeson has never acknowledged receipt of Wulwick 1995, which shares the same outlook on Phillips's lifetime research as the pieces to which Leeson refers to as new history, as distinct from my "old" history.
8. I remarked in 1987 that it strikes economists today with amazement that in 1960 they saw the national Phillips curve as representing a long term, stable relation or--in Lipsey's terms-- Phillips "functions" as representing "immutable" theoretical relations. Leeson claims that my quotation distorted Lipsey's meaning. I do not see that citing Lipsey's (1960) two words--immutable and functions--had that effect (Wulwick 1987, 841; Lipsey 1960, 17, 19).

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