

The Demise of the High Inflation Trade-Off Interpretation

A Reply to Chapple

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Abstract

Two pieces of historical evidence have forced a re-evaluation of the textbook story about Phillips' contribution to stabilisation policy. The first is the realisation that there existed, prior to Lucas (1976), a 'Phillips Critique'; the second is that Friedman used what is now called "Phillips' Adaptive Inflationary Expectations Formula" to undermine the high inflation Phillips Curve. In a subsequent paper, I shall argue that Phillips constructed his econometric policy evaluation critique in three stages. The first stage (1950-62) involved work on the curve that bears his name; the second (1967-8) and third (1972) were of more general applicability. The first stage took zero or trivial inflation plus exogenous import prices as part of the 'structure' of his model; a regime shift to a non-trivial inflation outcome changed the 'structure' of his model. In particular, an inflation-devaluation spiral (caused by quite small inflation differentials) would, via rising (and now endogenous) import prices, set off a "wage-price spiral" thus displacing Phillips curve observations off to the right in a stagflationary direction. In the present paper, I shall demonstrate that Chapple has unwittingly surrendered his position about the stable trade-off in Phillips' work: when inflation generates the expectation of a continuation of inflation this, in Phillips' model, increases inflation still further, thus destroying the notion of a "stable" trade-off. But the instability caused by inflation in Phillips' model is far removed from the cosy equilibrium forces of the conventional natural-rate model. Thus Chapple's defence of the textbook story tell us more about the reliance on textbooks as the transmitter of 'authoritative analysis' than it does about Phillips' subtle and insightful stabilisation proposals.

Chapple's Defence of the Textbook Story¹

My reply to Simon Chapple is divided into two parts. The first part demonstrates that the evidence does not support Chapple's assertions. The second part argues that there were three zones in Phillips' RI-C-CU curve: the zone of "runaway inflation" (RI), the "compromise" zone (C) plus the zone of "catastrophic unemployment" (CU). Deeply embedded in Phillips' work was the commitment to zero or very low inflation; his trade-off related exclusively to the low inflation "compromise" zone. Minor inflation differentials in the trade-off zone could be accommodated by minor exchange rate adjustments – but nowhere did Phillips display any tolerance for outcomes in the "runaway inflation" zone. Indeed, the 'Phillips Critique of Econometric Policy Evaluation' can be used to predict the stagflation that balkanised macroeconomics in the 1970s. These arguments will be presented in a subsequent paper.

It is in everyone's interests if Chapple's work is also divided into two parts (or 'roles'). Thus the comments that follow relate to Chapple's role as textbook true believer, so to speak,

and are not intended to reflect on any scholarly work that he may be engaged in. In this way Chapple's attempts to defend the literal truth of the textbooks can be informative about the type of 'evidence' that is thought to be sufficient to maintain an 'equilibrium' with respect to conventional perceptions, after that 'equilibrium' has been rendered unsustainable by fresh historical evidence. Otherwise, the exchange may seem to the reader to be sour and repulsive: a negative sum game.

Chapple's latest attempt to pin on Phillips the authorship of the inflationary maelstrom that balkanised macroeconomics is a continuation of an exchange which began, on his part, as a reply to my 1995 *New Zealand Economic Papers* essay on 'Bill Phillips: His Machine and His Curve'. My current reply is thus a reply to a reply (Chapple 1998) to a reply (Leeson 1997a) to a reply (Chapple 1996a). Chapple (1996b, 9) initially cited the *HER* Leeson-Wulwick exchange in which Nancy Wulwick advanced the trade-off interpretation (in contrast to the interpretation which stresses Phillips' opposition to inflation) which Chapple now champions.² But this reference disappeared from Chapple's work, and has not returned.

Chapple has volunteered to play the role of barometer and defender of received textbook opinion. But there is an extensive literature on the Phillips curve: Santomero and Seater's (1978) bibliography exceeds eleven pages; an attempt to explore the immediate pre-history of the Phillips curve utilised a bibliography in excess of twelve pages (Leeson 1997b). In his ten-page NZEP critique, Chapple apparently read only eight relevant pages from the thousand or so that I have written on the Phillips curve: an input-output ratio of impressive proportions. These eight pages did not include the *Economica* essay on 'The Trade-off Interpretation of Phillips' Dynamic Stabilisation Exercise' where my argument is presented.³ His assertions missed their target because he was unaware that I had clearly defined the high inflation trade-off to exclude the low inflation trade-off that Phillips discussed. But despite being alerted to this definition Chapple repeats this misrepresentation in his second reply.

In his second reply, Chapple refers (in addition to the NZEP references) to two *Encyclopedia* entries: (three pages of biography, plus four pages on the various Phillips curves) plus the *Economica* essay and a political background essay in *HOPE*. Chapple has therefore now apparently read about ten percent of what I have written on the Phillips curve. Since Chapple's propensity to read now (by a small margin) outstrips my current propensity to add to the Phillips curve literature, he may, sometime mid-way through the next century, be in the correct starting position. But the outcome will still be uncertain since Chapple insists on attributing to me a position that is the opposite of that which I have taken in the work he claims to have read.

The tone of Chapple's essays is 'Authoritative Analysis'. But Chapple's defence of the textbook story has led him to redefine "runaway inflation" as stable and therefore non-runaway; to redefine the "classic quantity theory" so as to allow any rate of inflation to result from a constant rate of growth of the money supply; and to redefine a state of permanent excess demand driving prices permanently upwards in response to this excess demand as a state of "equilibrium" (section 3). Chapple informs us that the turmoil that destroyed the Bretton Woods system was "simply not an issue" (section 5); and the major constraint on post-war British policy-makers was simply "apple pie". Chapple also criticises Phillips for not having been influenced by Marxian analysis (section 4).

Chapple (1998, 73, 84, 85, n4; 1996b, 9) asserts that Phillips was "simply plain wrong in his positive belief in an exploitable inflation-unemployment trade-off ... the simple Phillips curve". He also asserts that the "revisionist" challenge to the simple textbook story is a ship floundering on "one great rock". But Chapple's ship is made of straw, entirely of his own construction. He claims to offer "substantial quotations" from my work; but not substantial enough to refer to the material where I explicitly argue a position the opposite of that which

Chapple attributes to me. Chapple also offers "direct quotations from Phillips's published writings ... This minimises ambiguity..."; but these direct quotations do not include the material in Phillips' work that contradict Chapple's assertions. This adversarial tactic *maximises* ambiguity, as does Chapple's habit of offering references that bear no relationship to the assertion that he is making.⁴ In his first reply, every time Chapple quoted from Phillips he either misquoted, truncated the quote without acknowledgment (in at least one instance, deleting important information) or got the page reference wrong, or a combination of all three. The same combinations of errors reappear in his second reply.

Chapple (1998, 84) states that it "is worth mentioning" that the attempt to distance Phillips (a strong anti-inflation advocate) from inflationary outcomes merely "picks up on a resonating theme in the history of macroeconomic thought". It is also worth mentioning that Chapple's textbook story resonates with a very dubious interpretation of the history of knowledge: Error Defeated in Noble Battle by Truth. In the macroeconomic creation myth, wage cutting (Pigou) was defeated by aggregate demand management (Keynes); in the monetarist counter-revolutionary creation myth, neglect of inflationary expectations and confusion between real and nominal wages (Phillips) was defeated by rigorous microeconomic foundations (Friedman); in the New Classical counter-revolutionary creation myth, the unwarranted extrapolation of zero-inflationary structure into inflationary scenarios (Phillips *et al.*) was defeated by the awareness that structure is not policy invariant (Lucas). So compelling are these pedagogical fairy tales that it is unlikely that they will ever disappear from the textbooks; but the Phillips curve is very informative about what passes for historical knowledge among mainstream economists.

Sections 2-5 of the present essay relate at least in part to Phillips' work. Sections 2 and 3 demonstrate that Chapple has unwittingly surrendered his trade-off interpretation and that his equilibrium interpretation is a misrepresentation of Phillips' work. The connection between Lawrence Klein and Phillips is examined in section 3, and Samuelson and Solow's seminal essay is placed in an appropriate international setting in section 4. Sections 6-8 are more concerned with correcting Chapple's errors than in providing fresh information about Phillips and may therefore be skipped by those with an interest in Phillips and no taste for 'village' disagreements. Chapple has constructed his argument about my discussion of stabilisation policy in an open economy by quoting an entire paragraph - minus the context-providing opening sentence. Chapple then provides his own context, which is different from the context of the deleted opening sentence (section 6). Chapple's argument about Alogoskoufis and Smith has been constructed by attributing to those authors (by making an unacknowledged insertion into a quote) some words that they did not write (section 7). Chapple's assertions about my claims for Phillips' contribution to the adaptive inflationary expectations literature are the opposite of the claims that I have actually made (section 8). Brief concluding remarks are provided in section 9.

2. The Demise of the High Inflation Trade-Off Interpretation

Chapple has yet to acknowledge the demise of the textbook story, but a full-scale retreat is evident in his second reply. His defence of the textbook story tells us less about the evidence than it does about the reluctance of some economists to abandon preconceived notions. For example, there is overwhelming evidence linking Phillips to what Cagan (1998) calls "Phillips' Adaptive Inflationary Expectations Formula". But in his first reply Chapple (1996a, 219, 226) could not find the textual evidence nor could he believe the memories of those involved; the evidence was characterised as "tenuous ... incorrect ... minimal". Despite being referred to page 311 of the theoretical Phillips curve (Leeson 1994a, 612, n13), Chapple

was apparently unable to find Phillips' clear discussion of the destabilising influence of inflationary expectations. Indeed, Chapple (1998, 81) continues to assert that "It is difficult to find discussions on instability in Phillips's work." But Chapple contradicts both himself and the high inflation trade-off interpretation with his discussion of Phillips' analysis of instability in his second reply.

Having now located the evidence linking Phillips to inflationary expectations, Chapple (1998, 75) asserts that "Unfortunately for Leeson's argument, Phillips (1954, 311) argues that inflationary expectations may impact in a destabilising fashion on *aggregate demand* and hence the position of the economy along a *given Phillips curve* [emphases in original]." Unfortunately for Chapple's argument, elementary logic now obliges him to abandon the high inflation trade-off interpretation.

Phillips (1954, 298-9) stated that flexible prices are integral-type forces and he demonstrated the alarming consequences of integral-type policies generating a "dynamically unstable [system] ... In such a case the oscillations would increase in amplitude until limited by non-linearities in the system and would then persist within those limits so long as the policy was continued ... There may, however, be a tendency for monetary authorities, when attempting to correct an 'error' in production, continuously to strengthen their correcting action the longer the error persists, in which case they would be applying an integral correction policy ... It will be seen that even with a low value of the integral correction factor, cyclical fluctuations of considerable magnitude are caused by this type of policy, and also that the approach to the desired value of production is very slow. Moreover, any attempt to speed up the process by adopting a stronger policy is likely to do more harm than good by increasing the violence of the cyclical fluctuations...".

The final and most crucial sub-sections of Phillips' stabilisation model are 'Inherent Regulations of the System' and 'Stabilisation of the System' which begin with: "Some examples will be given below to illustrate the stability of this system under different conditions of price flexibility *and with different expectations concerning future price changes*" [emphasis added]. The theoretical Phillips curve is then tested against a variety of scenarios: inflationary expectations being a crucial factor in determining whether the system has satisfactory outcomes or not: "Demand is also likely to be influenced by the rate at which prices are changing, or have been changing in the recent past, as distinct from the amount by which they have changed, this influence on demand being greater, the greater the rate of change of prices ... The direction of this change in demand will depend on expectations about future price changes. If changing prices induce expectations of further changes in the same direction, as will probably be the case after fairly rapid and prolonged movements, demand will change in the same direction as the changing prices ... there will be a positive feed-back tending to intensify the error, the response of demand to changing prices thus acting as a perverse or destabilising mechanism of the proportional type" (1954, 311, 313).

Economic reasoning proceeds sequentially; but let us make the false assumption that Phillips' reasoning ends there (which it does not) and that Chapple is therefore correct to assert that Phillips' saw inflationary expectations as destabilising aggregate demand alone (with no feedback via inflation differentials to the exchange rate and import prices). This in itself destroys the possibility of a stable trade-off because the expectation of further inflation "tend[s] to introduce fluctuations": "The strength of the integral regulating mechanisms increases with the increasing degree of price flexibility, while the total strength of the proportional regulating mechanisms decreases as demand responds perversely to the more rapid rate of change of prices, and both these effects tend to introduce fluctuations when price flexibility is increased beyond a certain point. When price expectations operate in this way, therefore, the system ... becomes unstable ..." (1954, 313).

Thus when non-trivial inflation enters the system and the expectation is formed that this inflation will continue "the system ... becomes unstable". It is tautologically true that stabilisation theorists such as Phillips begin from the *premise* that instability is undesirable: stabilisation policy is, after all, designed as an antidote to instability. If inflation at some non-trivial rate led (via inflationary expectations) to inflation at a higher rate, and this inflation was expected to continue, leading (via inflationary expectation) to a still higher rate etc., then this denies the possibility of a stable inflation-unemployment trade-off in the "runaway inflation" zone.⁵

3. The Phillips-Lipsey Disequilibrium Model

Another "resonating theme" in Chapple's work (resonating from Nancy Wulwick's work) is the representation of Richard Lipsey in an unfavourable light. The second variable (the rate of change of unemployment) in Phillips' (1958, 283) model influenced wage changes through the expectation that the business cycle will continue moving 'upwards' (or 'downwards').⁶ Lipsey, who worked closely with his LSE colleague, labelled this "an expectation effect ... the reaction of *expectations* [emphasis in text], and hence of competitive bidding, to changes in u " (Lipsey 1960, 20). But Chapple (1998, 82) asserts that Phillips (1958, 283-4), Lipsey (1960, 20) and Leeson (1997d, 165) are wrong: "Phillips does not devote any paragraphs to this issue. Phillips (1958, pp. 283-4) ... contains no mention of expectations and no discussion of future labour market conditions". Lipsey's interpretation of Phillips' work is branded as "inaccurate" by Chapple (1998, 80) and Lipsey (1978) is also chastised for not citing Phillips' unpublished Melbourne Paper, which was virtually unknown until after Lipsey's essay was finished.⁷

According to Chapple, for four decades, Lipsey has been labouring under the misapprehension that Phillips' (and presumably Lipsey's) models were disequilibrium models. Chapple (1998, 85, n7) claims that Phillips' statement about wage and price rising at ten per cent per year is "evidence - if more evidence is indeed needed - against [Lipsey's] disequilibrium interpretation."

In his Inaugural Lecture Phillips (1962, 3-5) illustrated "some general points about equilibrium, stability and fluctuations ... Suppose the system is not in equilibrium; for example, suppose there is excess demand. Then the price will rise. This will increase production and reduce consumption and so reduce the excess demand. Since the price continues to rise so long as there is any excess demand and any rise in price reduces the excess demand the process will continue until the excess demand is eliminated. In brief; the existence of any discrepancy between production and consumption causes a movement in price which tends to correct the discrepancy."⁸ And the opening sentences of Phillips' 1958 model describe a disequilibrium model where prices and wages are expected to rise in response to future excess demand in the labour market: "When the demand for a commodity or service is high relatively to the supply of it we expect the price to rise, the rate of rise being greater the greater the excess demand. Conversely when the demand is low relatively to the supply we expect the price to fall, the rate of fall being greater the greater the deficiency of demand" (1958, 283).

Indeed, if these disequilibrium situations along Phillips' curve were actually equilibrium outcomes along Chapple's (1998, 79) "long run equilibrium curve" then there would be no need for aggregate demand manipulation. But Phillips' (1959, 4; 1958, 299) discussion related to the "demand for labour at which policy should be aimed" and the consequences "if the demand for labour were held" (via aggregate demand manipulation) at various levels. Phillips' stabilisation policy was focused on outcomes in the "compromise"

zone: from aggregate product market equilibrium to trivial, and therefore sustainable, low inflation product market disequilibrium (with aggregate demand, informed by the insights of the PID analysis, operating to restrain departures from these minor disequilibrium positions).

Lipsey (correspondence 24 August 1998) recalled that "During the year that I worked closely with Phillips on my 1960 Phillips curve article, I flirted for a month or so with what would now be called 'new classicism' in trying to explain all the points on the Phillips curve as intersections of labour demand and supply curves. Phillips would have none of this. But he did endorse my interpretation of his curve, which appeared in Part II of my 1960 essay. If you want to see so clear a statement of the disequilibrium nature of the curve as can admit of no other interpretation, look at page 14 of my *Economica* article. What was in doubt, and what I subsequently got wrong, were the consequences of maintaining a perpetual disequilibrium, but what none of us doubted was that we were talking about a disequilibrium phenomena."

There is further evidence relating to both labour market disequilibrium and inflationary expectations. Phillips initially experimented with capacity utilisation data to investigate the empirical properties of the theoretical Phillips curve. But severe data problems caused him to look for other data (Blyth 1975, 306; Lipsey 1978, 50). Henry Phelps Brown suggested labour market data; Phillips was then forced to reconstruct his model in labour market terms. Around this time, Phillips introduced Lipsey to Bent Hansen's *A Study in the Theory of Inflation* as an appropriate source of inspiration: "Bill first put me on to this source and I came to accept this view of the Phillips curve as being a Hansen-type reaction curve for the labour market" (correspondence 19 February 1993). One likely reason that Hansen's model appealed to Phillips was the "explicit inclusion of disequilibrium in the labour market in the analysis" (Hansen 1951, 249). Certainly, Lipsey's (1960, 13) model introduced the "dynamic hypothesis that the rate at which w changes is related to the excess demand, and specifically, the greater is the proportionate disequilibrium, the more rapidly will wages be changing ... This is Phillips [1958, 283] hypothesis. It is also used extensively, for example, by Bent Hansen [1951]".

Hansen (1951, 139) discussed the "quite drastic changes in productivity" that occur during inflation (a theme that can also be found in Phillips' work). He also conducted an extensive analysis of expectations. In "Final Remarks", Hansen (1951, 246-8) concluded that "... price expectations do disturb the analysis in so far as they can render the price-reaction equations unusable ... it is clear that in practical forecasting, price expectations and their changes are a difficulty of the first order, and that a policy which aims to maintain monetary equilibrium is forced to accord a great deal of weight to holding expectations in check".⁹ The most promising method of holding inflationary expectations in check is, of course, to advocate policies that involve either zero (or trivial) inflation, which is exactly what Phillips advocated.¹⁰

4. Klein and Phillips

Chapple (1998, 83) informs us that "it seems to me that there are flaws in [Phillips'] analysis". One of these "flaws" is that "Neither Phillips's published works nor his PhD thesis shows any evidence of having read Klein." Chapple (1998, 85, n4; 1996a, 221, n3) appears to be favourably impressed with one of Klein's (1947) early essays. Klein (1947, 115, 118) concluded that that the Keynesian "system is rigged to get an unemployment equilibrium as much as the classical system is rigged to get a full employment equilibrium. Neither approach is entirely acceptable ... [the] problem of the relation between wages, profits, and employment is of great current interest but cannot properly be analysed within the customary framework of Keynesian and classical economics ... the Marxian theory is based

fundamentally on the interrelationships between wages and profits. The Marxist theories of reproduction are well suited for the study of this problem".

Klein (1947, 115-6, 111) demonstrated what he called "a basic contradiction in the working of the capitalist system when the traditional supply curve of labor is used". According to Klein, "the essence of capitalism" is that "The workers have nothing to say about the amount of employment that will be forthcoming at any point in time". Klein argued that Joan Robinson's *Essay on Marxian Economics* "holds the key to an answer": "[in] the modern labour market ... the individual worker has no opportunity to decide anything except whether it is better to work or to starve" (Robinson cited by Klein 1947, 116). Thus in Klein's model the labour supply function has no meaning: "an exogenous variable ... determined by demographic factors". Klein found that "there is no relation that is more stable than the demand for labor"; he therefore sticks with the 'classical' formulation, with employment being demand-determined according to the "marginal-productivity theory".

In the 'classical' model a cut in wages is required when unemployment occurs "in the state of disequilibrium". But in Klein's (1947, 113) model, wages and employment (not unemployment) are only slightly negatively related: "There is an initial position of unemployment. Wages fall, but employment and income increase little or not at all". Wage cuts may generate the expectation of further wage cuts: "the very conditions that make the system unstable and make it likely that wage cuts will push the system away from rather than towards its full-employment equilibrium". Thus with a fixed labour supply, labour demand shifts inwards, following the cut in wages, increasing unemployment. With a 'classical' labour demand function it is the process of wage cutting that drives dynamically unstable labour market outcomes.

According to Chapple, it was Klein (1947) who "first developed theoretically (and indeed with a form of Phillips curve estimated empirically) ... the static IS/LM model with a simple Phillips curve and a given nominal money supply". Moreover Chapple believes that the Phillips curve is an equilibrium phenomenon. But contrary to Chapple's assertion, Klein does not present "a form of Phillips curve estimated empirically". Klein (1947, 116-7) wrote down the classical labour demand function combined with an exogenous labour supply assumption (a positively sloped 'Phillips curve') and asserted (without any empirical evidence) that "The parameters of this equation suggest that small wage cuts are not associated with large increases in employment". Thus the slope of this 'Phillips curve' is positive but rather steep.¹¹

In Klein's (1947, 116) model the wage rate is determined by "collective bargaining". Thus if collective bargaining increases the wage rate (along a given labour demand curve) this would increase unemployment: again, a positively sloped Phillips curve. Thus under capitalism, wage increases increase unemployment and wage cuts would not reduce unemployment by much and may actually increase it.

Within a year of Klein's essay, the cold war began: "In the McCarthy era I left Michigan for the peace and academic freedom of Oxford" (Klein 1988, 28, 39). His eastwards geographical migration may have corresponded with a rightward political migration (he became coordinator of Jimmy Carter's economic task force). Klein (1998) later reflected that "Bill Phillips was a remarkable person ... Our last encounter was in 1971 ... It was evident that the stroke had laid low the person that I remember from England of the mid-1950s, full of exuberance, fresh ideas, and optimism about our subject. We had good opportunities then to discuss wages, inflation, unemployment, and the many promising leads for econometric research, when he visited Oxford for a seminar at the Institute of Statistics".

Indeed, a casual glance at Phillips' published works reveals not only that Chapple's assertion about Phillips not having read Klein is false but that Klein changed Phillips'

understanding of his relationship. Phillips (1962, 11; 1961, 365) stated that "In my own very crude attempt to study this relation [1958] I assumed that changes in the cost of living only affect wage changes in years when prices are rising rapidly, usually as a result of rapidly rising import prices. Others [footnote included] have assumed that changes in the cost of living have a proportionate effect on wage rates in every year. For the post-war years this is probably nearer the truth than the assumption I used." In the footnote, Phillips stated that these "others" included Klein and Ball (1959), Dicks-Mireaux and Dow (1959) and Lipsey (1960).¹²

5. Samuelson and Solow

In his second reply, Chapple (1998, 28) makes only one reference to his first reply. There exists a considerable literature about Samuelson and Solow's (1960) seminal paper; a literature Chapple was encouraged to read. But in his second reply, Chapple again apparently failed to consult the literature, concluding that "The great irony about casting Samuelson and Solow as villains of the Phillips curve is that their discussion of the curve, including as it does explicit consideration of both adaptive expectations shifting the curve and issues of path dependence, is significantly more sophisticated than that of Phillips himself (see Chapple 1996)".

It is not clear how the reader would benefit by this instruction to "see Chapple 1996" since all that can be found there is what has already been described as an "unoriginal couple of sentences" (1997a, 61). Moreover, Chapple has completely misrepresented my work on Samuelson and Solow. I have already highlighted the role that Samuelson and Solow (1960, 193, 189) accorded to "expectations ... shift[ing] the curve downwards in the long run" (cited by Leeson 1998a, 93); "Samuelson and Solow (1960, p.189) noted that there had been an upward shift in the relationship in the 1940s and 50s ... Samuelson and Solow (1960, p.193) also suggested that a policy regime switch might alter the shape of the Phillips curve" (Leeson 1998a, 93-94; 1996, 250). I emphasised that "*it must be stated* [emphasis added] that they [Samuelson and Solow] hedge their analysis with cautions about the data, the reliability of the relationship, and a specific warning that 'what we do in a policy way during this next few years might cause it to shift in a definite way' (1960, 193)" (1997c, 144; 1995b, 26). Chapple (1998, n4, n9, n10) provides three references to pages 144-5 of this essay, but the imperative that "it must be stated" made no impression on him.

There are no scholarly "villains" in 'The Origins of the Keynesian Discomfiture' (Leeson 1998b; 1995c). In an essay that Chapple (according to his bibliography) appears to have read I concluded that "The arguments expressed in this essay should not be interpreted as in any way impugning the motives of either Hansen, Samuelson, Solow, Tobin, Klein or Modigliani (who were concerned with the noble aspiration of reducing unemployment and poverty)" (1997c, 147; 1995b, 29). I stated that Samuelson and Solow's advocacy of four or five per cent inflation applied to "the years immediately ahead"; "These economists did not abdicate their responsibilities with respect to containing inflationary forces in the post-1965 period" (1997b, 458; 1994b, 19). Indeed, the 'Phillips curve' was clearly lurking around the debate over post-war inflation at least since 1948 and I continue to disparage 'The Distasteful Hunt for 'Guilty' Economists' (1995d).

Chapple (1998, 20, n10) also defends the lack of an international dimension in Samuelson and Solow's analysis. He states (with no supporting evidence) that "As a country highly independent on world trade and the supplier of the world's reserve currency during the 1960s, balance of payments problems were simply not an issue in the US."

But in the months before Samuelson and Solow's AEA presentation, the Congressional Joint Economic Committee considered 'The International Monetary Position and Policy of the United States'. On 28 October 1959, Robert Triffin (1960, 3-11) cautioned that "we can no longer afford to ignore the impact of our internal policies upon our external position". Triffin was concerned about "the alarming deterioration in our balance of payments and the doubts this may raise about whether or not we are pricing ourselves out of the world market". He concluded that "We must, first of all, strengthen or recover our competitiveness in world trade by arresting creeping inflation ... Our huge gold losses of last year ... have been slowed down this year by an extremely sharp rise of interest rates in this country, prompted by our domestic concern about creeping inflation." Part of the solution involved the International Monetary Fund exercising "a considerable influence upon members to restrain internal inflationary abuses". After these hearings, Triffin (1960, 159) recalled that "a wave of panic swept over Washington officialdom".

President Kennedy instructed his Council of Economic Advisers (Walter Heller, Kermit Gordon and James Tobin) plus the Secretary of the Treasury (Douglas Dillon) and the Director of the Bureau of the Budget (David Bell) to formally address these issues. The Council then invited the Brookings Institution to prepare a report. The report began with the imperative that "it is clear [that] ... the large and continuing balance of payments deficit is in itself an urgent problem for the United States to which an appropriate solution must be found - a solution that does not conflict with other basic domestic and international objectives" (Salant, Despres, Krause, Rivlin and Tarshis 1963, 2).

The Brookings Institution later sponsored a conference on this issue based on the inside information contained in the National Archives and Record Service and four presidential libraries. The conference was attended by what Chairman Kermit Gordon called "the most star-studded cast of movers and shakers in economic policy-making ever to be assembled around the same table" (cited by Goodwin 1975a, 385). The editor, Craufurd Goodwin (1975b, 1, 3) opened the volume thus: "Every president from Truman on has faced inflation as a major problem ... During the next three presidents' administrations the effects of inflation on international competitiveness and the balance of payments joined its other costs at centre stage ... secular inflation was seen to be perilous not only to the domestic economy but for the entire international economic order as well." Excluding 1957, the US had been permanently in deficit on the balance of payments from 1948-1964: "the CEA was well aware that ... [t]he *real* (goods and services) side of the US international situation was not healthy" [emphasis in text]. By 1967 "the international financial picture was a multifaceted disaster ... Devaluation by the United States was anticipated" (Cochrane 1975, 215-6, 281-2). The US balance of trade "showed a marked decline in the late 1960s as imports rose sharply and exports, partly because of mounting domestic inflation, failed to match this growth ... a *Joint Economic Committee* report was issued, calling for an incomes policy and recommending a devaluation of the dollar" (De Marchi 1975, 346-7).

Fear of this inflation-devaluation spiral drove the advice of the Rostow balance of payments stabilisers, who were competing with Paul Samuelson for President Kennedy's ear. Walt Rostow informed the President that "an inflationary settlement [in the automotive industry] might set in motion a gold drain which could upset our present tenuous equilibrium", and within weeks of coming to office Kennedy "made clear that his central concern in economic matters lay in bringing the balance of payments under control ... the White House had early and unmistakably assigned top priority to the international aspects of economic strategies ..." (Barber 1975, 147, 141-2; Weber 1975, 356). This determination was publicly transmitted (6 February 1961) in his 'Message to Congress on the Balance of Payments and Gold' (Triffin 1960, 179-80).

6. Stabilisation Policy in an *Open Economy*

When Chapple quotes (or more typically misquotes) Phillips, he repeatedly deletes those sections which contradict his argument. Nevertheless, he (1998, 24) asserts that he is competent to divine the points that "Phillips wishes to make". Some of his divinations imply that the decision-makers of some journals are bereft of elementary powers of logic. Thus according to Chapple (1996a, 226), *the Economic Journal* published material which claimed that in 1954 Henry Phelps Brown confirmed that Phillips was the 'author' of the analysis contained in Friedman's 1967 AEA Presidential Address. A moment's reflection would suggest that his time-travel interpretation is implausible (outside the realm of science fiction).

Chapple (1998, 9) also asserts that *Economica* published an essay that found an inflation-devaluation story in Phillips' essay on 'Stabilisation Policy in a Closed Economy'. Chapple quotes *almost* an entire paragraph from section I of my *Economica* essay. He asserts that this paragraph is "an interpretation of Phillips's 1954 theoretical curve." He also asserts that "Much of this commentary on the original curve is flatly contradicted by what Phillips wrote".

But Chapple's commentary is flatly contradicted by what I wrote. Chapple (1998, 9) informs us that "There is not even a suggestion in Phillips (1954) of an inflation-devaluation spiral. By definition there cannot be ... the title of the article is tellingly 'Stabilisation Policy in a Closed Economy.'" But tellingly, Chapple has failed to report that the paragraph from which he partially cites was not "an interpretation of Phillips's 1954 theoretical curve" but an extrapolation from the theoretical curve to the empirical curve.

My *Economica* essay is densely referenced (one hundred and eighty-eight references in thirteen pages of text). If I were presenting a "commentary" on Phillips' 1954 essay I would have referenced Phillips' 1954 essay. After the Introduction there are only three paragraphs with no references; one of which is the paragraph from which Chapple partially quotes. In keeping with his promise of "substantial quotations" Chapple reproduces *almost* the complete paragraph (he quotes one hundred and eight words). But there are two vital pieces of information missing from the words that Chapple quotes. The first is the opening sentence of the cited paragraph which provides the context of the rest of the paragraph: "The axes are worth noting" (1997d, 158). Clearly it is I, not Phillips (1954) who is doing the noting.

The second piece of relevant information that Chapple fails to report is the final sentence of the previous paragraph which links what has gone before with what is to come: "His empirical curve was designed to flesh out these theoretical perspectives" (1997d, 158). The Introduction to that essay states that Section I "outlines Phillips's stabilisation model"; the references relate to almost everything Phillips wrote between 1953 and 1968. Section I ('Phillips's Dynamic Stabilisation Exercise') refers to Phillips' writings between 1953 and 1958, stating that Phillips' model had "already been outlined" prior to his 1958 empirical essay. The first sentence of that section states that "It has been suggested that Phillips's empirical investigation lacked a firm theoretical foundation" (1997d, 155-7); the paper "examines Phillips's famous curve" by relating the empirical curve to its theoretical predecessor. We know little about the process by which Phillips abandoned his search for an empirical counterpart to the "level of production", and replaced it (after experimenting unsuccessfully with capacity utilisation) with percentage unemployment. But we do know that Phillips began by examining "the simplest case of a closed economy with government ignored ... with the usual assumption of constant prices and interest rates" and that the theoretical Phillips curve was "illustrated here with particular reference to aggregate

production in a closed economy, but they are of quite general applicability" to "any stabilisation policy" (Phillips 1954, 305).

The theoretical Phillips curve used in Phillips' empirical work had been developed sequentially by relaxing the assumptions of no government, no trade, constant prices and interest rates. Phillips later extrapolated from his stabilisation model into the open economies of the United Kingdom, the United States and Germany. I was clearly not offering a commentary on the closed model but extrapolating from that model into the situation that Phillips later turned his attention to and which he had "outlined" in 1954.¹³

7. Alogoskoufis and Smith

In his final words of his first reply, Chapple (1996a, 228) asserts that his position is taken by "some of the best minds in the profession". In recognition of his contribution to the Phillips curve literature, Chapple claims immortality in the form of "Chapple's Law" (1996b, 3). However, many authors have found illuminating qualities in Phillips' work; qualities that are missing from the standard story. I suggested that Chapple should familiarise himself with this literature so as to disabuse himself of the illusion that the non-standard interpretation of Phillips' work is the product of a single pen, and that he is merely ridding the profession of the work of a 'turbulent' economist. Chapple was given a non-too-subtle hint that some of these interpretations can be found in recent issues of high profile journals. I could have cited from the *Economic Record*: Phillips' "insightful model and its empirical estimate shed much light ... on the role of *expected* [emphasis in text] price inflation" (Perry 1980, 90); but instead I chose a more recent essay.

My first reply contains only one extraneous reference to a journal: "Alogoskoufis and Smith (1991, 1256), for example, writing in the *American Economic Review*, concluded that 'Contrary to conventional wisdom, A.W. Phillips does *not* [emphasis in text] seem to have been confusing nominal with real wage changes, in putting forward his hypothesis ... an expectations-augmented version of Phillips's wage equation ... is outlined in the first three paragraphs of his 1958 paper'" (1997a, 57-8, n8). This is an accurate account of what Alogoskoufis and Smith found, but Chapple (1998, 23) asserts that "when one contextualises the quotation from Alogoskoufis and Smith, they actually state the opposite of what Leeson believes. Leeson fails to point out that Alogoskoufis and Smith unambiguously write that Phillips (1958) 'clearly did not take inflationary expectations into account' (Alogoskoufis and Smith 1991, p. 1256). Rather, what Alogoskoufis and Smith (1991, p. 1256) actually do is to *add* a hypothesis about inflationary expectations to the original Phillips curve to provide a model for econometric estimation" [emphasis in text].

Alogoskoufis and Smith (1991, 1256) did not "unambiguously write that Phillips (1958) 'clearly did not take inflationary [sic] expectations into account'". They explained the reasons why Phillips felt justified in deleting price inflation from his estimating equation ("he supplemented his wage equation with a mark up-price equation") and suggested that Phillips' estimated model was different from his "postulated structural wage-adjustment equation". Their article was entitled 'The Phillips Curve, The Persistence of Inflation and the Lucas Critique ...' and the equation that Chapple refers to contains only one expectations term: "E is the mathematical expectations operator, and I is the information set of wage-setters" (1991, 1256). Immediately before presenting this equation they state that "although Phillips's analysis does not appear to have suffered from 'money illusion,' he clearly did not take expectations into account, and his econometrics are clearly susceptible to the Lucas critique" (1991, 1256).

There is more to seeing than meets the eyeball: Chapple (1998, 23) took this statement, deleted the reference to both the Lucas Critique and to "expectations" and inserted his own phrase "inflationary expectations" (all within Alogoskoufis and Smith's quotation marks). Chapple also fails to point out is that what he calls this "add[ed]" hypothesis was Phillips': "we shall for the most part stick to an expectations-augmented version of Phillips's wage equation as outlined in the first three paragraphs of his 1958 paper [emphasis added]. From the discussion of the three factors that he highlighted, we can write a log-linear, expectations-augmented version of his wage equation ..." (1991, 1256).¹⁴

8. The Phillips Curve and the Invalidity of the Natural Rate Model

Chapple (in his role as defender of the textbook faith) assumes, without justification, that my argument must consist of the following: Phillips was an opponent of inflation and was aware of inflationary expectations, therefore he must be the author of the natural-rate model that replaced the original Phillips curve in conventional thinking. This is precisely the same sequence of 'thought' that infers that because Phillips drew the curve and discussed the trade-off in the "compromise" zone, he must, by definition, have supported the extrapolation of the trade-off into the "runaway inflation" zone. Thus Chapple (1996a, 227) asserts that I have rewritten "Phillips's work so as to make his positive ideas presage the modern wisdom". By "modern wisdom" Chapple means the natural-rate model. Chapple also claims that I have been "stressing the importance of inflationary expectations in Bill Phillips's own work on the Phillips curve" (Chapple 1998, 73). But I stressed the opposite: "Phillips did not repeatedly [emphasis in original] stress the role of inflationary expectations in his own work - for one very good reason: he was not concerned with situations of ongoing inflation" (1995a, 238).

I have never argued that Phillips was the author of the natural-rate model. Whenever I have discussed the matter (including in the *Encyclopedia* essay that Chapple cites), I have argued the reverse. I corrected Chapple's mistake in my first reply (1997a, 58): "At no stage did I make any claims for Phillips as a precursor of 'a Friedmanite long-run vertical (no trade-off) Phillips curve' (Chapple 1996a, 226) ... Chapple is clearly unfamiliar with what I have written on the long-run Phillips curve". I then listed five articles where I have discussed the long-run Phillips curve and suggested that Chapple should have read this material before jumping to his conclusions.

But Chapple, in his second essay, repeats the incorrect assertions contained in his first essay without apparently having read any of these five articles. Even the titles of these five essays belie Chapple's incorrect assertion: for example, 'The Validity of the Expectations Augmented Phillips Curve' (1991), and 'Does the Expectations Trap Render the Natural-Rate Model Invalid in the Disinflationary Zone?' (1997e).¹⁵ In an essay that Chapple claims to have read, I stated that the natural-rate (N-REAP) model is inconsistent with Phillips' curve and "The Phillips curve, in both its original and its N-REAP form, has proved to be an inadequate guide to policy. It is a profound injustice to the memory of Bill Phillips that his name should be associated with either of these policy failures" (1997f, 504). But Chapple implies that I wish to dissociate Phillips from the former policy disaster but associate his name with the latter.¹⁶

9. Concluding Remarks

The usual propositions of the standard story about Phillips are that:

1. Phillips had no adequate theory;

2. Phillips was ignorant of inflationary expectations;
3. Phillips confused real and nominal wages;
4. Phillips implicitly advocated that tolerating permanently high levels of inflation could permanently solve unemployment.

The evidence can sustain none of these propositions, but evidence is sometimes not required to sustain received textbook opinion. Textbooks can define the boundaries of a subject and opinions are handed down (and textbooks often written) by those who Chapple (1996a, 228) calls "the best minds in the profession". In this hierarchy, professional respectability is acquired by deferring to those opinions. Chapple's assertions in defence of the textbook story are informative both when they are false ("we are not told"; "is nowhere defined") and when they are quite literally true ("I have been unable to find ...").

This episode of macroeconomic history provides an extraordinary combination of textual, oral and archival resources for those interested in policy formation, the interaction between the political and academic market places and the sociology of economic knowledge. Moreover, since the low inflation Phillips curve has returned to the centre of policy discussions, this provides macroeconomic historians with a unique perspective on contemporary macroeconomic discussions. Equally, Phillips' work was not vulnerable to the Lucas Critique since Phillips authored the 'Phillips Critique' several years before Lucas (1976) – a theme that will be elaborated upon in a subsequent paper.

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Notes

¹ My research into Phillips' life and work has always previously been improved by inputs from those who worked with him; but given the publication deadline I was unable to access most of these sources for this reply. Nevertheless, I am grateful (without implication) to Richard Lipsey, J.C.R. Dow and Richard Watson for their thoughts. I am also grateful for the opportunity of writing again about Phillips.

² Phillips "enumerated the inflation-unemployment trade-off for the purpose of policy making in the early 1960s" (Wulwick 1994, 85).

³ This essay was clearly referenced in the material Chapple cited in his bibliography.

⁴ For example, Chapple (1998, n4) accuses me ("e.g. Leeson 1997e, p. 145") of neglecting some aspect of Klein's work. But there is no reference to Klein on that page. On page 118 of that essay I reported, without comment, that Dicks-Mireaux and Dow (1959) had found a Phillips curve in Klein's work.

⁵ Indeed, the empirical curve was derived by assuming away the effects of the rate of change of the demand for labour (1958, 290). Thus when inflationary expectations drive the system upwards into the runaway inflation zone this rate of change variable must be re-introduced: the expectation of increasing demand drives Phillips curve observations off to the right in the stagflationary direction.

⁶ Phillips (1958, 283) wrote: "employers will be bidding more vigorously for the services of labour than they would be in a year during which the average percentage unemployment was the same but the demand for labour was not increasing". In Phillips' model, when this "expectation effect" is removed, and demand held constant in the compromise zone, the rightward displacement of Phillips curve observations ceases.

⁷ It appears that the Melbourne Paper (1959) was virtually unknown prior to Blyth's introduction to Phillips' *Festschrift* (Bergstrom et al. 1978). Blyth's essay was presented to Phillips on his sixtieth birthday (along with Lipsey's essay and the rest of the *Festschrift*). As it turned out, Blyth's essay was published as an obituary in 1975; but Lipsey's essay was long finished by then. Lipsey (correspondence 24 August 1998) recalled that he has "never read Phillips' 1959 essay on the Australian Phillips curve, although Phillips told me about it in general terms in his office one day and I urged him to publish it along with the US curve that he had been working on. But he did not do so at the time."

⁸ Phillips went on to demonstrate that the stability assumption is "fallacious except on the assumption that the complete response of the rates of production and consumption to any change in price occurs instantaneously. If there are any time lags in any of the responses the system will usually fluctuate ... The competitive model which we have been considering, and other so-called 'self-equilibrating' models of economic processes are, in fact, examples of what are known in other fields of study as 'negative feed-back control systems'. In order to see intuitively why these systems are often oscillatory and may well be unstable (which means in practice that they tend to produce fairly large and regular cyclical movements) let us consider again the competitive market. ... It is intuitively plausible, and can in fact be proved, that if, in this case, what are usually called the equilibrating or corrective forces are strong enough to make the amplitude of the induced movements in excess demand greater than the amplitude of the exogenous movements, the system will be unstable, that is, the fluctuations will increase and tend towards a regular and sustained limit cycle".

⁹ Hansen's (1951, 249) theoretical exercises suffered from the same defect that Phillips' sought to overcome with his own: "the relations with empirical facts are quite platonic."

¹⁰ Chapple (1998, 5, n2) informs us that Lipsey is mistaken in suggesting that the Phillips curve remained a relationship between prices and output in Phillips (1956, 1957). But Phillips (1957) took his 1954 model and added inventory demand as an additional component of aggregate demand. Phillips (1957, 272) stated that "the desired value ... of inventories held in excess of minimum working inventories ... [will] be influenced by [the] expected rates of change of prices". In contrast, Chapple tells us that "I have been unable to find any discussion of such a relationship in Phillips (1957)". Chapple also asserts that "There is a very brief suggestion in Phillips (1957, p.99) that product prices are positively related to real aggregate demand". But in the 'Theoretical Specification of an Interdependent System' Phillips (1956, 101) stated that "Theoretical considerations lead us to the hypothesis" that consumer demand depended upon "the rate of change of the average level on product prices". The quote from Lipsey (1978, 50) provided by Chapple is one half of a sentence the second half of which reads: "... its underlying rationale was seen by Phillips to lie in the pressure of derived demand in factor markets affecting factor prices, which then fed through on to product prices. The discussion that Chapple dismisses as "brief" contained a wage-price-deficit spiral broken by contractionary aggregate demand policy. There is also an early version of the 'Phillips Critique' (1956, 99-100). Chapple (1998, 5) asserts that "contrary to what is suggested by Lipsey (1978, pp. 49-50) ... Phillips (1954) does not assume that the zero inflation level of production corresponds to full employment". But Lipsey does not suggest this. Instead Lipsey (1978, 49, 53-4, 70) maintains the opposite of the position that Chapple attributes to him: "Phillips regarded the shedding of the dichotomy as crucial", the dichotomy being that at less-than-full-employment "the price level was fixed" and at full employment "real variables were fixed and expansionary disturbances to the system affected only monetary variables (the price level in particular) ... It was obvious to him that any disturbance to the system had both real and monetary effects in the short term". Lipsey (1978, 56) correctly states that Phillips' (1958, 299) policy conclusion relates to product market equilibrium (at zero price inflation). Chapple (1998, 17) pedantically refers to another possibility mentioned by Phillips; that of labour market equilibrium (at zero wage inflation). But Chapple neglects to point out that the labour market equilibrium outcome is prefaced by the remark "as is sometimes recommended"; sometimes, but not by Phillips (1958, 299). Indeed, in the Australian Phillips curve there appears to be no labour market equilibrium since neither wage changes nor earnings changes had been zero during his estimating period, and the Phillips curve did not cross the horizontal axis (1959, Figs. 1, 6). Chapple cites from Lipsey (1978, 56) but neglects to point out that the cited paragraph contained a reference to Lipsey's (1960, 31-32) warning about the need to investigate the casual links between wage and price inflation. It was this type of analysis that persuaded Phillips (1962) to abandon his hypothesis about prices affecting wages after a threshold.

¹¹ Klein (1947, 116) also asserts, without supporting evidence, that inter-war US data reveal a negative correlation between the first differences in the general wage rate and the lagged wage rate and unemployment.

¹² Klein and Ball (1959, 465-7, 481, 475-7) were (like Phillips) expressing the prevailing anti-inflation preoccupation of economists of the time ("the current problem of inflation in the Western Capitalist world"). They cite Phillips (1958) and (like Phillips) Bent Hansen; they also highlight the power of "union strength on the side of cost-push inflation", introducing a political factor dummy variable to capture this militancy. Import prices were taken to be exogenous despite the "extraordinary significance" of import prices in the early 1950s, but they cautioned against falling "into the trap of ascribing our present difficulties with control of inflation to any single source in the economy."

¹³ Phillips' sentence about international adjustments not being "dealt with" which Chapple (1998, 9) uses to clinch his argument, did not end where Chapple implied that it did; Phillips (1954, 298, n1) went on to report that "attempts to regulate" the current balance of payments through "internal credit policy or quantitative import controls" were "likely to cause cyclical fluctuations" and probably had been partly responsible for such post-war cyclical movements in the balance of payments.

¹⁴ The Alogoskoufis and Smith article is relevant to Phillips' implicit prediction of stagflation, in that the authors found that the persistence of price inflation is significantly higher under managed exchange rate regimes than under gold-based regimes of exchange rate fixity. But since they abstracted from the nonlinearities of Phillips' model and were unaware that Phillips constructed a Lucas critique years before Lucas, I stated immediately after the words that Chapple cites that "My reading of the theoretical Phillips curve leads me to a stronger conclusion than Alogoskoufis and Smith". Chapple should have reported these words; he should also have reported that I have never suggested that Phillips' model should be confused with natural-rate models.

¹⁵ Even the name of one of the journals is a clue: the *Cambridge Journal of Economics* is committed to perspectives that are at odds with the natural-rate story.

¹⁶ Chapple (1998, 79) states that it "is worth quoting in full" two passages from my first reply: two hundred and fifteen words; but "in full" implies something more than Chapple offers. Chapple quotes almost an entire paragraph, but deletes the final sentence: "Phillips outlined the economic forces that would exacerbate instability in these high inflationary regions" (1997a, 51). This is completely inconsistent with the cosy, equilibrating, delusion-dissipating forces of the natural-rate model.

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