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David J. Smyth: An Appreciation of His Work

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I: Introduction:

David J. Smyth (1936–2014), the founding editor of the *Journal of Macroeconomics* (hereafter *JMacro*), had a remarkable publication record prior to the birth of *JMacro* for a relatively young researcher. He published sixty-eight refereed articles and two substantial research monographs between 1967 and 1978. Over his academic lifetime he published a total of 169 refereed journal articles as well the two books. David's publication record, expertise and interest were varied and stretched from forecasting, macroeconomics and political economy. Having established his reputation in the areas of forecasting and macroeconomics, in the last decade of his academic research, David began to forge a reputation and expertise in the area of political economy.

David also published in the fields of industrial economics, labor economics, public finance and applied econometrics. While an extensive appreciation of David's enormous body of work would be of interest, the present paper is a more modest undertaking. We consider David's key contributions, together with his research collaborators, to the academic economics community. To this end, we will focus on his achievements; notably the founding

of the *JMacro* and his main contributions to the areas of macroeconomics, forecasting and political economy.

David founded *JMacro* in 1979 and continued to edit the journal until 1998. Since its founding, *JMacro* has published some of the more innovative contributions to the broad area of macroeconomics. In the twenty years under David's editorship, the journal was established as a leading field journal in that area.

David was a valued colleague, productive research collaborator and generous mentor to us. Jim (James Holmes) was a long-term research collaborator of David, beginning with his time at Buffalo; Jim was also at the inception of *JMacro*, serving on the original Editorial Board. Together, they published several influential papers and contributions in the area of macroeconomics, for example the 'Holmes-Smyth' effect. Colin (Colin Ash) was a graduate student of David's and his research assistant during his time at the University of Birmingham, UK. They went on to be research collaborators and, together with others, made a number of important contributions in the field of forecasting. Joshy (Joshy Easaw) got to know David during his last academic appointment as Research Professor at Middlesex University Business School in London, UK. They shared a common research interest in political economy, notably political business cycle, incumbent popularity and voter preference functions.

In what follows, we assess David's motivation and objectives for starting *JMacro.* We also evaluate some of the more innovative contributions David jointly made in the area of macroeconomics, especially around this period. Subsequently in Section III, we evaluate the notable contributions David has made in the field of macro forecasting and, finally, we consider the extensions and contributions David and his research collaborators have made in the understanding of voter preferences and incumbent popularity functions.

II: *JMacro* and Macroeconomics

While David was already an accomplished researcher and had published extensively in a number of top-rated journals, he appreciated that the outlets for journal publication in macroeconomics were limited and narrowly focused. His sentiments were expressed humorously in an address he gave to the *Eastern Economic Association*, which was subsequently published in their Journal under the title "How Not to Get Your Paper Published".

He said, "I write a lot of articles and I am willing to wager a considerable sum that I have had more papers rejected than anyone in this room. I have received more ridiculous, stupid, biased, incompetent and unintelligible referee reports and letters from editors after months and months and sometimes years and years, than anyone else here. I can give you lots of horror stories from the trenches." (1994, pp. 471). So, David was motivated to establish a journal that was different and would publish good but, perhaps, controversial macroeconomic papers authored by young and yet to be established researchers.

The first two papers by Holmes and Smyth published in the *JMacro* were among their best joint work in macroeconomics and remain relevant today. "Excess Demand for Labor, Unemployment and Theories of the Phillips Curve", published in the Fall issue (1979b), was a challenge to Lipsey's (1960, 1974) theory of the Phillips Curve.

Lipsey (1960) had presented what appeared to be the first theoretical underpinning for the empirically observed Phillips curve of the negative relationship between the rate of change in wages, $\dot{w}$, and unemployment, U. He argued that, independent of the phase of the business cycle, the excess demand for workers (X) was a decreasing function of the rate of unemployment, and that, when there exists a disequilibrium in the labor market, the rate of change of wages is an increasing function of the excess demand for labor: $\dot{w} = f(X)$. Wages,

adjusting under a Walrasian process, rise when there is excess demand for labor and fall when there is excess supply.[1]

Holmes and Smyth criticized Lipsey (1960) in their 1970 *Economica* (Holmes and Smyth (1970)) article, arguing that Lipsey's basis for a negative relationship between the rate of change in wages and unemployment was flawed. They stated that, because there is both an excess demand for labor and zero cyclical unemployment, i.e. $X > 0$ and $U = 0$, during an expansion, there can be many (an infinite number of) values of the excess demand for labor corresponding to only one value of *U*.

This implied that Lipsey's theory was not a causal theory and could not be rejected on the basis of empirical evidence. Two properties essential to a causal theory are that the causal variables occur before the explained variables, and the latter are a function of the former, e.g. if *B* causes *A*, then $A = f(B)$ (Simon and Rescher 1966). This last requirement is essential in order to potentially be able to reject a hypothesis. This is easy to understand from the "classic" definition of causality: if *B* causes *A*, then when *B* occurs, *A* occurs, i.e. $A = f(B)$.[2] In contrast, if $A \neq f(B)$, then when *B* occurs something other than the value of *A* predicted by $f(B)$ occurs. "Other than" includes an infinite set of alternatives. Another way to phrase this is that if $A \neq f(B)$, then we cannot say what will happen when *B* occurs. Anything is possible, or we are ignorant of the effect of *B*. Thus, if one can find evidence inconsistent with the assumption that the explained variable(s) are a function of the causal variable(s), one can reject the hypothesis that *B* causes *A* and conclude that *B* does not cause *A*.[3] When applied to Lipsey's theory that excess demand was a function of unemployment, a zero rate of cyclical unemployment in a business cycle expansion would predict or be

[1] i.e. $\dot{w} = f(g(U))$, when $f' > 0$ and $g' < 0$.
[2] The explained and/or causal variables, *A* and/or *B*, can be sets (Suppes 1970).
[3] This is the basis of statistical testing, when the explained "variable", i.e. *A*, is a set (the range of acceptable values) and *not-A* are the values in the rejection region.

consistent with any value of the excess demand for labor from zero to infinity. Hence, excess demand cannot be a function of unemployment. Combining this with the Walrasian hypothesis that the rate of change of wages was equal to some function of this excess demand implies that, in a business cycle expansion, there is no relationship between the zero rate of unemployment and the rate of change of wages; that rate could be anything according to Lipsey's "theory".

In his "Reply to Holmes and Smyth", Lipsey (1974) modified his theoretical model to include frictional unemployment. Corry and Laidler (1967) had previously demonstrated that when there is disequilibrium in the labor market that is associated with the phase of the business cycle, plausible economic behavior could produce frictional unemployment and a conventional Phillips curve in a contraction. However, in an expansion, while producing frictional unemployment, the same set of assumptions resulted in a relationship between $\dot{w}$ and U that was either vertical, or negatively, or positively sloped with no way to plausibly choose between them. Their argument can be viewed as essentially making a similar point to that of Holmes and Smyth in 1970; in an expansion, their three plausible slopes clearly imply $U \neq F(\dot{w})$, but if the relationship can be vertical, this also implies $\dot{w} \neq F^{-1}(U)$, the same as when there is only cyclical unemployment.

Lipsey claimed to demonstrate that, with the addition of several assumptions which he stated were "arbitrary", he could obtain a monotonic negative relationship between unemployment and the rate of change of wages in an expansion. Lipsey argued that what was important is that the relationship *can* be derived from some set of assumptions, even if they had no basis in plausible economic behavior, and stated "there is no reason to think that when better relations for job leaving and job finding are substituted for the present ones the (Phillips Curve) relation will suddenly become underivable" (ibid. pp. 64).

Holmes and Smyth's (1979b) reply to Lipsey (1974) demonstrated that his revised models provided no better basis for a negative functional relationship between excess demand for labor and unemployment. The first of Lipsey's models required two assumptions: that $dU/dt = 0$ and that labor force size does not change over time. As Holmes and Smyth stated, the assumption that "$dU/dt = 0$ implies that the rate at which people leave their jobs…is equal to the rate at which people find jobs … clearly, it is a valid assumption only in a stationary state equilibrium… (which is) a logical inconsistency in a model that purports to explain disequilibrium behavior." (ibid., pp. 355). Furthermore, even with this assumption, there is not a unique relationship between *X* and *U* unless one is prepared to accept that the size of the labor force and the equilibrium employment do not change over time. They noted that the empirical investigation of Lipsey (1960) required believing that the size of the UK labor force did not change between 1862 and 1957. Lipsey's second model, involving unfilled vacancies, *V*, required the assumption that the product of *U* and *V* is a constant. Holmes and Smyth, referring to an empirical paper by Dicks-Mireaux and Dow (1959), pointed out that Lipsey provided no theoretical basis for this assumption. Hence, Holmes and Smyth suggested the assumption that the product of *U* and *V* is a constant is an article of faith and, consequently, the Phillips curve is also an article of faith.

Holmes and Smyth went on to argue that a satisfactory theoretical basis for the observed Phillips Curve phenomena required a plausible, rigorous, micro foundation based upon conventional concepts of short-run demand and supply of labor, but suggested that it was not clear that a Phillips Curve could be obtained from such assumptions or how such a theory could be reconciled with the empirical evidence in a business cycle expansion of a positive relationship. Holmes and Hutton (1996) subsequently rigorously derived, from a micro founded model based upon plausible economic behavior, a theoretical relationship between employment and wages that switches from negative in contractions to positive in

expansions. In their model, profit maximizing firms and fully rational workers make optimal intertemporal decisions subject to uncertainty associated with the business cycle and its effect upon product prices, similar to the formulations of Sargent (1987) and McCafferty (1990). Furthermore, they estimated the relationship of the rate of change of wages and unemployment conditional upon the state of the economy. They reported strong empirical evidence supporting switching regimes – with a highly significant negative relationship in contractions, supporting the hypothesis that employment is primarily demand determined in a contraction and consistent with a Phillips curve, but an even more significant positive relationship in expansions, suggesting that employment is primarily supply determined in an expansion, and inconsistent with a Phillips Curve, but consistent with the disequilibrium hypothesis.

"Deficit Financing, Liquidity, and the Government Budget Constraint" (1979a) was one of a series (1972, 1979a and Holmes, Smyth and Hutton 2004) of articles in which the theoretical effects of taxes and deficit spending upon economic behavior was explored. In each paper, small but plausible changes to the conventional specifications of behavior in macroeconomic modeling were demonstrated to have radical implications for the effectiveness of public policy.

In their 1972 paper, Holmes and Smyth observed that the transactions demand for money or liquidity preference conventionally depends on gross personal or national income. Yet, it is unrealistic to assume that an individual would base their expected or planned spending on goods and services upon their gross income. One must pay income taxes, and these are often deducted before one's pay is received. In addition, an individual's saving-consumption expenditure decision plausibly affects the need to hold money in one's portfolio. Savings are often deducted before one's pay is received, as in the case of 401k, 403b, 457, and IRA plans, and are planned in advance. Thus, savings should require smaller

amounts of transactions balances of money than consumption expenditures. For these reasons, Holmes and Smyth changed the specification of the liquidity preference function from gross income to consumption expenditures or disposable income in an otherwise standard macroeconomic model and demonstrated that this had a radical impact on the predicted effect on aggregate income of an increase in either an income tax or value-added tax (VAT). An increase in the personal income tax reduces disposable income and thereby consumption expenditure, and an increase in a VAT should directly decrease consumption expenditures. Decreased consumption expenditures are a decrease in transactions and should decrease the transactions demand for money balances in an individual's portfolio.[4] Hence, a tax increase will shift not only the IS curve, but also the LM curve down. If the LM shifts down more than or equal to the IS, the overall effect upon income can be positive or zero – rather than the conventional negative effect; a radical, new result. Due to the lack of knowledge about the relevant parameters they concluded that the effectiveness of tax policy as a countercyclical tool is ambiguous and its use should be avoided as a stabilization policy.

A rigorous microeconomic foundation for their modeling was subsequently provided by McCallum (1989, pp. 35-41)[5] who derived a liquidity preference function for an infinitely lived, utility maximizing, representative agent based upon their (real) consumption expenditures and leisure over their lifetime. He specified that leisure can be increased by using money to economize on "shopping time". Holmes and Smyth's argument can be viewed as involving more "shopping time" for consumer expenditures than for paying taxes (and/or possibly saving). Hence, McCallum's "shopping time" model provides a rigorous

[4] They made a similar argument for a firm's demand for money, based upon the corporate income tax.
[5] Without reference to the Holmes and Smyth paper.

basis for making liquidity preference a function of consumer expenditures or disposable income. In addition, and in response to the criticisms of the New Classical school that the conventional IS-LM model lacked a rigorous microeconomic foundation, McCallum also derived the conventional IS function for a utility maximizing representative agent (ibid., pp. 102-107). However, when applied his rigorously derived macroeconomic model to empirical analysis, he inexplicably substituted gross income for consumption in the liquidity preference function, thereby eliminating the possibility of taxes affecting the LM function or obtaining the new results for the tax multiplier.

The argument that the tax multiplier is plausibly positive or zero is relevant to understanding why the "Obama stimulus" of 2008-9 failed to live up to its promises. This set of large fiscal stimuli consisted primarily of tax cuts and increased subsidies and transfers but not much of an increase in government spending upon goods and services. Indeed, it is puzzling why it was enacted given that Laurence Summers was President Obama's top economic adviser during this period, and he and Gregory Mankiw (1984, 1986) had empirically studied what they termed the "Holmes-Smyth effect". They concluded that the tax multiplier was plausibly not negative, contrary to what is conventionally assumed, but slightly positive (or zero). Their empirical evidence was subsequently reinforced by the comprehensive econometric investigations of Smyth and Smith (1988, 1990a, 1990b). All these econometric studies suggest that the Holmes-Smyth effect would result in the "Obama stimulus" being ineffectual or perverse and should be of current relevance to assessing the probable demand-side macroeconomic impact of the recent "Trump" federal tax cuts.[6]

Holmes and Smyth (1979a) extended their 1972 framework to an analysis of the effect of government debt, arguing that the stock of government debt had two plausible effects that had been neglected. First, government debt can be viewed as equivalent to an

[6] None of these studies considered the possible supply side effects, and these may be important.

income transfer from tax payers who finance the interest (and possible principle) payments to bond holders who receive the interest on the bonds. When these are the same individuals, as they must be in a representative agent model, "Ricardian equivalence" must hold. However, theorists (e.g. Summers and Carroll 1987, Poterba and Summers 1987) have argued, and empirical evidence has shown (Bernheim 1987, Andreoni 1989, Elmendorf and Mankiw 1999, Meissner and Rostam-Afschar 2017), that this equivalence is unlikely to hold. When Ricardian equivalence does not hold, a larger quantity of government bonds may be associated with a large, non-neutral income transfer. Holmes and Smyth parameterised the degree of non-neutrality and analysed its effects as follows: the greater the degree of non-neutrality, the more individuals' under-estimate their future tax liabilities relative to the interest paid on the bonds: they 'feel' wealthier. They argued that the amount of the income transfers should vary with the (average) maturity of the debt; the shorter the maturity the larger the "transfer". For example, a console, which will never be paid off, has a much lower future tax liability in the short-run than a one-year note. Hence, they suggested adjusting the amount of such liabilities by the average term to maturity. When these adjustments are made for the expected amount of real income transfer associated with government debt, and if this adjustment is positive, expected real disposable income exceeds measured real disposable income. This should determine consumer expenditures and should be formally incorporated into the IS equation.

Holmes and Smyth also argued that government debt is far more liquid and a closer substitute for money than other assets (e.g. Tobin 1971, Woodford 1990). Moreover, the shorter the term to maturity of a bond or note, the closer it should be as a substitute for money. They proposed a liquidity parameter measuring the degree of substitutability. Incorporating both the liquidity and the non-neutrality of government debt results in a revised LM equation.

These two revised IS and LM equations simultaneously determine the equilibrium level of income and the interest rate. The interested reader can refer to Holmes and Smyth (1979a) for a formal derivation of these results. This hypothesis about the plausible specification of how government debt affects both the IS and the LM curves can be described as a second "Holmes-Smyth effect". It implied that both the money and the government spending multipliers will become smaller as the amount of government debt becomes larger. Moreover, if the debt eventually becomes large enough, one or the other of these multipliers can become zero or perverse. How the increased size of the government debt causes the change in the effects of an expansionary monetary or/and government expenditure policy from conventional to unconventional can be easily understood from the effect of such debt upon the slopes of the IS and LM curves. An increase in the government debt causes the IS curve to rotate clockwise and potentially becoming vertical (or even positive), and at the same time, the same increase causes the LM curve to rotate counter-clockwise and potentially becoming vertical (or even negative). However, the requirement that output and interest rates be a function of the explained variables, as described in the discussion of the contribution regarding the Phillips curve, excludes the possibility that both can become vertical or "perversely" sloped at the same time.

The money multiplier measures the effect upon output of an increase in the quantity of money. If the government debt is not too large, an increase in the quantity of money supplied will shift the LM curve down along a negatively sloped IS curve. The larger the size of the outstanding government debt, the steeper the IS curve (as it rotates clockwise) and, thus, the smaller the effect upon output (the money multiplier) of the downward shift in the LM curve. Further increases in the stock of government debt can eventually cause the IS curve to rotate clockwise enough to become vertical or even positively sloped. If that occurs, an expansion

in the quantity of money supplied will result in no increase in output, or even cause a perverse money multiplier with output decreasing.

The increase in the stock of government debt outstanding will also adversely affect the government spending multiplier, which measures the effect upon output of an increase in government spending (for a given stock of government debt). If the stock of government debt is not too large, an increase in government spending, will shift the IS curve upward along a positively sloped LM curve and increase output. The larger the size of the outstanding government debt, the steeper the LM curve (as it rotates counter clockwise) and thus the smaller the effect upon output of the upward shift in the IS curve. A yet larger stock of government debt will cause the LM curve to continue to rotate counter clockwise until it eventually causes the LM curve to become vertical or even negatively sloped. If that occurs, an expansion in government spending will result in no increase in output, or even cause output to decrease; a perverse government spending multiplier.

This result suggested a new basis for the desirability of zero federal debt and of maintaining this state by a balanced budget - conventional monetary and fiscal policy would then then be effective,[7] and Holmes and Smyth argued that the "debilitating effects (of a large government debt on) both monetary and fiscal policy may be a key element in the recent failure of stabilization policy to achieve its goal." (ibid. pp. 105).

If the high ratio of U.S. government debt to GDP was of concern in 1979 when it was only 31%, how much more of a concern should it now be now that it is more than 100%?[8] The U.S. ratio is approaching that attained by Japan in the 1990s when their "expansionary" monetary and fiscal policies were widely recognized, but surprising, policy failures. Holmes and Smyth 's theoretical demonstration combined with the fact that government debt relative

[7] Cornyn, John. "Wisdom of a Balanced Budget Amendment." Dallas Morning News, December 1, 2010.
[8] https://tradingeconomics.com/united-states/government-debt-to-gdp

to GDP has been, and continues to be, very high may explain why recent expansionary monetary and government expenditure policies in the U.S. have been much less effective than expected, based upon conventional macroeconomic theory that neglects both the liquidity and income transfer aspects of the government debt. It is time perhaps to revise conventional macroeconomic theory along the lines suggested by Holmes and Smyth in 1979.

III: Macroeconomic Forecasting: Accurate and Useful?

The preceding section has highlighted David's concern to improve the effectiveness of macroeconomic policy, so it will come as no surprise to learn that another of his continuing research interests was also policy-related: the accuracy (or inaccuracy) of economic forecasts. His many empirical studies are of three broad types. Some examined the success of predictions in very specific sectors, as varied as a comparison of investment plans and outcomes in Australian manufacturing, and sports journalists' forecasts of major league baseball studies in the USA (Driver and Smyth, 1968, Smyth and Briscoe, 1969, Smyth, 1973, Smyth, 1974a, Smyth and Smyth, 1994, Smyth and Dua, 1995 and Dua et al, 1999). A second group of studies investigated specific properties of economic forecasts, for example their conservatism – under-estimating the magnitude and variability of changes – and their rationality (Smyth, 1974b, Ash and Smyth, 1981, Ash et al, 1998 and Ash et al, 2002).

A third group of studies probably had the most impact. Often working with co-authors Colin Ash and latterly Saeed Heravi, David examined in detail the overall accuracy of macroeconomic forecasts initially of the UK made by H.M. Treasury, the National Institute of Economic and Social Research (NIESR), the London Business School, James Morrell Associates, and the Organization for Economic Co-operation and Development (OECD), and subsequently of international forecasts for the seven largest OECD member countries:

Canada, France, Germany, Italy, Japan, the USA and the UK (Ash and Smyth, 1973, Ash and Smyth, 1975, Smyth, 1983, Ash et al, 1990 and Ash et al, 1997).

The novelty of the early studies (Ash and Smyth, 1973, Ash and Smyth, 1974 and Ash and Smyth, 1975), published in the first half of the 1970s, was that they offered a systematic formal evaluation, which was rigorous, consistent and quantitative, of continuing time series of forecasts and outcomes. In contrast, previous mainly in-house studies contained a wealth of descriptive information relating to individual forecasts, but little in the way of formal econometric analysis.

Core techniques of accuracy analysis used throughout these studies can be found in Theil (1961 and 1966) and Granger and Newbold (1970). They are summarized in Ash and Smyth (1973, Chapter 2). Various measures of overall accuracy are reported alongside diagnostic checks on the sources of forecast error: the proportions of error due to bias in the sense of over- or underestimating the actual mean, and due to mis-forecasting the systematic component of the variance of outcomes. For the optimum predictor, these proportions should tend to zero.

Later studies were enhanced by the addition of a battery of rationality tests. Using methods proposed by Holden and Peel (1985, 1990), Figlewski and Wachtel (1981) and Mullineaux (1978), the analysis comprised tests for the forecasts' bias and efficiency, and for the consistency of forecast revisions. Other simple diagnostic procedures included fitting time trends to errors to see whether accuracy improved or deteriorated over the sample period; inspection of potential conservatism, underestimating the size of changes regardless of sign; and turning-point analysis: for some purposes, such as the successful timing of changes in the direction of policy, it may be more harmful to make a smaller prediction error yet mis-forecast the direction of change, than to make a larger, directionally correct error.

In OECD terminology these studies are practical, not technical, tests of forecast performance. A technical test, arguably fairer to the forecaster, corrects for errors in exogenous assumptions, something which is almost impossible for anyone other than the original forecaster to do. On the other hand, a practical test is of practical value to the user, "who has to take decisions on the basis of the forecasts as they actually were" and who "when he pays a forecaster to produce them...is paying for him to get *everything* right" (Llewellyn *et al* (1985)).

The first study of official forecasts of the UK economy covered the years 1951-1971. For most of this period successive governments showed considerable caution in the publication of forecasts. For instance, when one Chancellor of the Exchequer was asked why more information was not provided on balance of payments' forecasts the substance of his reply was that "it was not usual to publish forecasts of figures for the very good reason that the forecasts are always wrong" (House of Commons, *Parliamentary Debates*, 1966). It was suggested to the authors informally that the UK Treasury's forecasts might not be authentic predictions but merely window dressing, displaying the course the government would like the economy to take rather than true predictions of the economic future. If so, what was intended to be "accuracy analysis" might instead shed light of the government's effectiveness in implementing its economic policy: no less interesting than an examination of its forecasting ability.

Perhaps not surprisingly, publication of the earliest accuracy studies attracted attention from the financial press and elsewhere. Malcolm Crawford (1972), Economics Editor of *The Sunday Times*, gave a detailed balanced summary of the findings reported in Ash and Smyth (1973): the authors had examined "the record of all the principle forecasters of the UK economy up to 1971 and going back as far as records exist...providing the most thorough examination of economic forecasting in the UK yet published". Other

commentators focused on some of the more negative findings. Reflecting on the OECD's track record (Smyth and Ash, 1975), *The Times* Business Diarist (1975, pp.19) found it "rather hard to resist the conclusion that economists are at least as bad at forecasting as governments are at managing their economies". And though the research recorded specific instances of poor forecasting, these errors could be exaggerated and misinterpreted. For example, philosopher Alastair MacIntyre, in his trenchant and controversial critique of predictability and explanation in the social sciences, *After Virtue* (1985) cites in evidence Smyth and Ash (1975) who "have shown that forecasts produced on the basis of the most sophisticated theory for OECD since 1967 have produced less successful predictions than would have been arrived at by using the common sense, or as they say, naive methods of forecasting rates of growth ... One could go on multiplying examples of the predictive ineptitude of economists" (pp. 89)

In fact, the results were more nuanced. Overall, the economists' forecasts were better than predictions from naive assumptions and more sophisticated time-series models. Judged by this criterion, genuine macroeconomic forecasting was not a waste of time. However, over the long periods covered by these studies, in some cases 20 years, there was no significant evidence of either general improvement (or deterioration) in forecasting accuracy.

As one might expect, while some forecasters' predictions of some variables were more accurate than those made by others, there was no clear overall "winner". As a group the forecasters were often least successful when predicting government consumption, fixed investment, stock building, the foreign balance of payment and inflation. Some of these variables are volatile and relatively difficult to forecast judged by the equally poor performance of the corresponding naive and time-series predictions. Downturns tended to be either under-predicted or missed altogether, and the increased frequency of turning-point errors contributed to the declining accuracy of longer-term forecasts.

Forecasting error was predominantly non-systematic, usually at least 50 percent of total error, and this low systematic proportion was maintained even as the forecasts' time horizon lengthened. However, the forecasters' track record gave no support for the general empirical validity of the rational expectations hypothesis. For example, about half of the OECD forecasts failed one or more of the rationality tests. In principle improved accuracy could be achieved by simple linear corrections or by incorporating information contained in recent known forecast errors.

The earliest of these national and international accuracy studies was published in 1973 (Ash and Smyth, 1973), the most recent in 1997 (Ash et al, 1997). Forecasting methods evolved over this long period and continue to do so as forecasters learn from experience. Two elements of past experience are as relevant now as they were at the time of the David research. The first is that "in as much as it appears that most forecasting error is attributable to non-systematic factors, it may well be that an essential pre-requisite for improved economic forecasting is further refinement in the traditional judgement of the forecasters themselves." (Ash and Smyth, 1973, pp. 263).

The second is its corollary: that although systematic error does not usually make a large contribution to inaccuracy, in some instance forecasts could be improved by incorporating information contained in recent forecast errors.

Three current UK macroeconomic forecasters are the Office for Budget Responsibility (the successor to the Treasury as government forecaster), the Bank of England, and the NIESR. All three have replaced single point forecasts with density forecasts in which a median forecast is seen as the central point of ranges of uncertainty. Frequency distributions of past forecast errors are illustrated in fan charts. Simulations probe the sensitivity of these forecasts to alternative judgements and wider economic scenarios (*Office for Budget*

Responsibility, 2012, and Britton *et al*, 1998). In the past, forecasters have commonly imposed a judgmental path on their equations' residuals, both to correct for whatever bias and inefficiency have been diagnosed in past prediction errors – a second-best attempt to attain the optimum – and, also, specifically to anticipate future non-systematic disturbances. Today these procedures are formalized and made explicit: David would surely approve.

IV: Political Economy: Incumbent Popularity and Voters' Preference Function:

The first of David's co-authored work in the area of political economy was on economic voting and appeared in 1986 (Smyth and Dua, 1986). David's research focussed on using Gallup aggregate presidential approval rating to estimate voter's preference functions for inflation and unemployment. This body of research considered a few variations and extensions. Firstly, there were partisan distinctions – estimation of the preference functions of Democrats, Republicans and Independent voters respectively (Smyth and Taylor, 1992a). They found that voters' preference for the trade-off between inflation and unemployment varied along partisan lines. Secondly, David and his co-authors contributed to the literature by considering different presidencies separately (see Smyth and Dua, 1989, Smyth et al, 1989 and Dua et al, 1995)). Thirdly, they were also the first to consider a disaggregated social preference functions between inflation and unemployment modelled using the national unemployment rate rather than group-specific rates (Smyth and Taylor, 1992b). They fitted presidential popularity functions to groups classified by age, region, sex and race. Therefore, David and his co-authors, in their extensive research in this area, considered when the social preference functions of voters were stable over different presidencies, political persuasion and different demographics.

David and his various research collaborators were also innovative as they considered voters' subjective preferences and perceptions in addition to actual macroeconomic indicators. For instance, Smyth and Dua (1988) departed from the existing literature which

focused on incumbent approval ratings compiled by Gallup and introduced the now commonly referred-to index of incumbent competence which is surveyed and compiled by Survey Research Centre (SRC), University of Michigan (see, Easaw, 2010 for more extensive discussion of this index). Indeed, Smyth *et al.* (1999) considered voters' preferences between inflation and unemployment using the SRC survey data. Smyth *et al.* (1994) also used survey-based inflation expectations when estimating voter preferences, enabling them to consider whether voters were forward-looking in their assessment when forming their social preferences.

An important innovation in this field has been the interaction between non-economic, or political, variables with economic ones. Smyth and Taylor (1986) and Smyth *et al*. (1994) also include "honeymoon" effects. This took the form of a dummy variable to control for any effect on inflation and unemployment that an incoming president may inherit from his predecessor. Another innovation was accounting for political scandals when estimating voters' preference functions. Specifically, in Smyth and Taylor (1986) they controlled for the Iran Contra scandal.

It is useful to highlight that much the econometric specification used in the various papers by David and his co-authors tended to model the voters' preference functions as a partial adjustment model. This is a prevalent specification as it has been argued that voters may have costs to change their preference, which tend to persist. This, however, has come under some criticism from political scientists, such as Beck (1991).

V: Concluding Remarks:

We have not here undertaken a comprehensive assessment of David Smyth's voluminous body of work. Such a task would require the involvement of far more co-authors and research collaborators than just the three of us and, consequently, would be a major undertaking. Nevertheless, the present paper makes an in-depth assessment of a small but focussed representation of David's work. It considers some of David's more significant and important contributions to the understanding of economics. It is a modest appreciation from three of David's research collaborators who remember him fondly – as a friend and companion, as well as a research collaborator and colleague.

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