

# Political factors in international economics: an overview

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The intellectual interaction between politics and economics is not new. The older term to describe what economists do—political economy—which until recently survived only in French translation and on the masthead of one of the premier professional journals, is a case in point. A more pertinent example is provided by developments over the past two decades in the field of industrial organization, the shift from an exclusively public-interest view of economic regulation to one in which regulation is seen as the result of interaction between utility-maximizing politicians and various interest groups.<sup>1</sup> The extension of such ideas to international economics is, I believe, both necessary and desirable. And, as these papers indicate, with the right experimental design, it can also prove highly illuminating.

## I. A summary of findings

Of the five articles included in this volume two deal directly with political variables. The other three do so indirectly.

The papers by Sebastian Edwards and Guido Tabellini (ET) and by Nouriel Roubini focus on similar issues, the government's budget deficit, the inflation tax and how they vary among countries. Although the authors of the two papers use different bodies of data and conduct somewhat different statistical tests, their conclusions are essentially the same: Modern public finance theory cannot account for the differences in deficits and inflation tax across countries; political variables have to be taken into account.

The papers by Takatoshi Ito and by Ked Hogan, Michael Melvin and Dan Roberts deal with political influences on macroeconomic policy within countries over time, Japan in Ito's case and the United States in Hogan *et al*'s. Ito's concern is the link between Japanese macroeconomic behavior and parliamentary elections. He examines two competing hypotheses: that the government has systematically manipulated the trade balance prior to an incipient election to bolster the real side of the economy, and that the timing of the election has itself been influenced by the state of the trade accounts and thus the real economy. He finds no consistent

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relationship of either sort between trade and elections, but presents other results that point to a change in policy regime in Japan in the mid-1970s.

Hogan *et al* (HMR) analyze the interrelationship between US economic policy and the behavior of the balance of payments in the mid-1980s. They examine the impact of innovations in the trade balance on US interest rates and exchange rates, and use the estimated relationships to make inferences about US economic policy. The conclusion they reach is that monetary policy in the second half of the 1980s turned expansive as political concern over the US trade deficit mounted.

Mark Stone uses arbitrage pricing theory to investigate the responsiveness of secondary market prices of developing-country debt to macroeconomic variables over the period April 1986 to October 1989. He then compares these estimates to others that he makes for sovereign debt in the interwar period. Since the effects are small in both instances and since the interwar market was a substantially more liquid market than the current one, he concludes that other forces, among which are political factors affecting sovereign risk, must be influencing such prices currently.

## II

### II.A. The inflation tax

The starting point for both the Edwards and Tabellini and the Roubini investigations is an optimal-taxation model of the form used by Mankiw (1987), in which, given the patterns of their expenditures, governments choose the tax rates on income  $\tau$  and on real money balances  $\pi$  (the inflation rate) such that marginal costs of each in terms of welfare loss are equal:

$$\langle 1 \rangle \quad h'(\pi_t) = kf'(\tau_t),$$

where  $h'(\pi_t)$  and  $f'(\tau_t)$  are the marginal costs of the taxes on money and on output, respectively, and  $k$  is the Cambridge  $k$ , the reciprocal of velocity.

One implication of this equation that is exploited in both papers is that changes in government expenditures should produce proportionate changes in the two tax rates. An additional set of implications is that  $\pi$  and  $\tau$  should both be non-stationary. A further implication that Roubini alone makes use of is that the government revenues associated with the two types of taxes should be cointegrated (Grilli, 1989). Yet another implication, that Roubini also tests (albeit with only limited success), but that ET do not, is that the real government deficit depends only on transitory shocks to government spending and transitory income. Neither paper finds much support for the public-finance theory.

ET's basic data set consists of annual observations for the period 1963 to 1987 for 52 developing countries. For many of the countries they are unable to reject the non-stationary hypothesis for either  $\tau$  or  $\pi$ , a finding that is consistent with the optimal tax model. For most countries, however, they find no significant positive relationship between inflation and the ratio of taxes to GDP, which, in contrast to the results of the unit-root tests, runs completely counter to the implications of the model. Given the more fundamental nature of this latter relationship, they conclude that 'the various versions of the theory of optimal inflation as a component of a dynamic optimal tax plan are rejected by the data.' They then go on to examine political influences on both inflation and government budget

deficits. The empirical variables that they employ with most success are measures of the frequency of changes in government, the overall frequency of such changes and the frequency of changes due to *coups-d'état* and to regular transfers of power separately. Their rationalization for the use of these variables is that 'Political instability and polarization lead to a form of collective myopia, even if policymakers and the voters are rational and forward looking.' In the presence of such instability, a policymaker who would otherwise want to pursue optimal policies will 'borrow in excess of the optimum and let his successor pay the bills.' They find significant positive relationships between these measures and both inflation and, to a lesser extent, government budget deficits across the countries in their sample.

Roubini's analysis of an even broader body of data than that of ET, leads to somewhat stronger rejection of the optimal-tax model.<sup>2</sup> For the vast majority of the 92 countries in his sample, Roubini finds no significant temporal relationship between  $\pi$  and  $\tau$ . In contrast to ET and employing a somewhat different procedure than they, he finds evidence of non-unit-root behavior of  $\pi$  and  $\tau$  in most instances. In addition, he is seldom able to reject the hypothesis of no cointegration of the two variables.

He goes on to consider several essentially political explanations for these results. He finds, using proxy variables like those of ET, that political instability is positively correlated with government deficits across the 71 countries of his sample for which he has deficit data. The alternative hypothesis that the distribution of income matters for deficits, receives no support.

If the findings reported in these two papers are robust, and I strongly suspect that they are, they raise several important questions. The more fundamental have to do with the political process itself and the reasons for the differences in political variables across countries. Since the other studies provoke a similar set of questions, let me defer any remarks on this score until I have discussed those papers.

More specifically germane to the ET and the Roubini papers are issues having to do with the optimal tax model and its previous empirical implementation. Key among these is the question of what we mean by 'optimality.' On the surface the answer appears simple and the model therefore quite incontrovertible. But the very results that the authors cite, particularly when combined with what we have learned from the industrial organization literature of the past two decades, suggest that it is not.

Actions that are optimal in a purely economic sense may not be optimal at all once political influence enters the picture. In a model like that of Becker (1983), the ratio of the marginal deadweight costs from the two taxes would not be unity as in equation <1>, but equal to the marginal political effectiveness of the groups being taxed. The same conditions would be met on the expenditure (subsidy) side, the ratio of the marginal deadweight costs of subsidies equaling the ratio of the marginal political effectiveness of the groups receiving the subsidies.

Thus it is entirely possible, for example, that the political pressure exerted by those who gain from inflation and the subsidies that the inflation tax is financing greatly exceeds the pressure exerted by those who pay the tax or otherwise lose.<sup>3</sup> In such instances, the resort to the inflation tax will be excessive, the marginal deadweight cost from the inflation tax far exceeding the marginal deadweight cost of the tax on income.

The authors of these papers clearly are cognizant of such political and economic

interplay, but the optimal taxation literature ignores it completely. The public interest theory of government behavior, which has fared increasingly less well in industrial organization since Stigler's initial salvo (1971), remains the paradigm in public finance.

I suspect, moreover, that the poor performance of the model in these papers is not simply some artifact of the particular samples under investigation. The Mankiw results supporting the optimal tax model for the United States become 'curioser and curioser' the more closely they are examined.<sup>4</sup>

The major problem that I have with those results is that they simply do not square with other information about Federal Reserve policy. If innovations in government expenditures were matched by innovations in money, one would expect to see this reflected in how market participants behave: traders and financial-market economists would direct substantial resources to forecasting the pace of government expenditures *and* use these forecasts as a major input in their assessment of Federal Reserve monetary policy. If this is what they are doing, however, it is one of the best kept secrets on Wall Street. In over a decade's worth of interaction with traders and other market participants I have heard not so much as an idle reference to such a mechanism. Nor do I know of any attempts by financial-market economists to exploit one in forecasting the movements in the monetary aggregates. Similarly, there is no evidence that I know of in official Federal Reserve statements about the course of policy that even hints at such a relationship.<sup>5</sup>

### II.B. Real fluctuations, elections and policy

The focus of Takatoshi Ito's investigation of the relationship between the timing of Japanese parliamentary elections and real economic conditions is on conditions in the tradeable goods sector. One view is that the government pursues a beggar-thy-neighbor policy, manipulating exchange rates and exports prior to an incipient election to stimulate the economy and thus improve its chances at the polls. The alternative hypothesis is that the government takes advantage of export booms caused by other factors and calls elections only when a boom is underway.

Ito finds no consistent relationship of either sort when he examines the data for the period 1955:I to 1990:I. Export booms do not cause elections, nor do elections cause booms. In this respect, the investigation comes up empty. There is, however, more to the story.

Ito does find an association between parliamentary elections and overall economic conditions, the probability of an election rising when inflation is low and real growth high. It appears, moreover, that this association is strongest for the earlier two decades of his sample period, the period running from 1955 to 1974.

This difference in behavior between the earlier and later subperiods is particularly interesting. Japanese policymakers themselves claim that policy was conducted differently in the two periods, that the high inflation of the mid-1970s marked a watershed after which the Bank of Japan became much more sensitized to increases in inflation, reacting both more quickly and more forcefully to it.<sup>6</sup>

Though American economists have become conditioned to thinking about regime changes in terms of the Federal Reserve's behavior in the early 1980s, and perhaps the Bank of England's behavior at roughly the same time, a regime change also apparently took place in Japan five years or so earlier. An intriguing question,

one to which I will return below, is how to account for the changes in policy regime in the three countries.

### *II.C. Trade, the Fed and the bond market*

In 'The trade balance news and exchange rates: is there a policy signal?' Hogan *et al.* investigate the links between innovations in the US balance of trade data, and movements in short-term US interest rates and in the dollar-pound, dollar-yen, and dollar-DM exchange rates. The stylized facts are significant positive relationships between trade-deficit innovations and all three exchange rates; a significant strengthening of all three relationships in the 1983-89 subperiod and an occasionally (they use day-of-the-week dummies to allow the coefficient to vary) significant negative relationship between such innovations and US short-term interest rates over the subperiod beginning in 1984.

What is responsible for these findings? The authors consider several explanations. One centers around Federal Reserve policy; another, the threat of trade restrictions.

According to the Fed-policy explanation, the appreciating dollar and associated trade deficits in the early 1980s eventually led to a shift in policy, or at least to market participants' expectations thereof. In the face of larger-than-anticipated trade deficits, market participants expected the Federal Reserve to pursue an easier interest-rate policy than they had previously anticipated. As a consequence, they increased the probability that they attached to a lowering of the Fed's funds-rate target and hence increased their estimates of the future levels of money supply and prices. The dollar depreciated on the news; shorter term interest rates fell.<sup>7</sup>

Is this interpretation reasonable? I believe it makes sense, but only for part of the subperiod. For one thing, it was during these years that the US government abandoned its earlier policy of benign neglect of the exchange rate. This in fact was the motivation for the authors' choice of subperiods. In addition, it is consistent with both market-participants' behavior at the time and the official Federal Reserve record of policy.

During these years the trade data, which in the earlier period had been largely ignored, became the subject of intense scrutiny in financial markets.<sup>8</sup> Market participants actively sought information that would provide hints with regard to the impending releases. Financial-market economists responded by substantially increasing the efforts that they put into making such forecasts, efforts that in the first subperiod had been rather minimal.

The Federal Reserve's account of its policy actions also lends support to this conclusion. A continuous source of such information is the 'Record of policy actions of the Federal open market committee,' still referred to (erroneously) in the financial markets as 'the Fed minutes.' This is the report that the Federal Reserve itself releases following the regular meetings of the Federal Open Market Committee (FOMC).<sup>9</sup> Of particular interest in the report is the section entitled 'Domestic policy directive,' the 'marching orders' that the FOMC gives to the Open Market Desk at the Federal Reserve Bank of New York.

Typically there is a sentence in the directive—sometimes two sentences—listing the specific economic factors that policymakers regard as important. In very recent years, this sentence has begun with the dangling participial phrase 'Taking account of . . .,' after which follows the list of economic factors that are of concern, and a

statement, which upon translation, provides an indication of the direction in which the FOMC was leaning at the time of the meeting.<sup>10</sup> The items that Fed watchers through experience have learned to regard as important here are the ordering of the economic factors of concern, the phraseology of the concluding statement and how the two may have changed from the previous directive.

I have reviewed these directives for the sample period used by HMR, from the last FOMC meeting of 1979 to the March 1990 meeting. In general over this period, domestic economic variables have taken precedence, monetary aggregates, inflation and real business conditions, variously described and in varying order, occupying the first three positions in these catalogues of factors. The exception is in late 1985 and in 1986. In July of 1985 'developments in foreign exchange markets' displaces inflation in the number three spot. By early 1987, however, the continued decline in the value of the dollar in the foreign exchange market created a shift in sentiment on the part of many FOMC members. Concerns about the potential impact on the overall US price level began to displace concerns about real income. Then in May of 1987, the Federal Reserve began to exert upward pressure on the federal funds rate.<sup>11</sup>

The alternative explanation associating trade-deficit innovations with changes in the probability of protectionist policies, however, also receives some support. HMR use three proxy variables to try to capture shifts in sentiment about protectionism. One of these, the share of imported automobiles in total US auto sales enters significantly in the equation linking exchange rate changes and trade innovations.

In the small, the HMR results are of interest because they provide a rather specific example of the role that political factors play in the formulation of monetary policy on a month-to-month basis.<sup>12</sup> In addition, however, they provoke a larger set of questions, questions having to do with the nature of the political influences on economic policy and the mechanisms linking the political and the economic.

Why at one point in time, or in one particular country, does one set of political factors dominate while at some other point, or in some other country, a largely opposite set does? Alternatively stated, what ultimately accounts for the changes in the political environment that lead to changes in a country's policy regime, or for the broad differences in political stability across countries that are associated with money-supply growth and inflation?

On the surface the answers to such questions appear to be so far beyond our ken that the temptation is to avoid them completely. When we put on our researcher hats we can usually do so. When we switch to our forecaster hats, however, we do not always have that luxury. Neither do economic agents. Any attempt at assessing the longer term economic outlook ultimately involves policy projections that depend crucially on assumptions about the political environment and its impact on those policy variables. Over relatively short periods we can endogenize those variables via the expedient of a reaction function. The political process is inertia ridden, or in Becker's terminology promotes the 'tyranny of the status quo.' Hence, shifts in such relationships are likely to be small. Over longer time horizons, that is less likely to be true. The process of endogenization has got to be pushed back a step further. To illustrate what I mean, as well as some of the problems inherent in such an undertaking let me use an example taken from the experience of the last several decades that has spurred my own interest in such questions.

### III. The links between politics and economic policy

Consider the economic situation of the United States and the United Kingdom in the latter half of the 1970s.<sup>13</sup> For over a decade, both countries had experienced more or less steady increases in their longer term average rates of inflation, with the process beginning somewhat earlier and hence having proceeded further in Britain than America. In back of this acceleration in inflation in the two countries lay continually more expansive monetary policy, itself the result of policymakers' attempts to maintain 'full employment' and otherwise stimulate the real side of the economy.

Then in 1974 the first oil-price shock hit and inflation temporarily went higher. Reacting to this further surge in price levels, policymakers in both countries contracted the growth in money supplies. The resultant monetary shocks, coupled with the effects of the oil-price shock itself on output, led to the second recession in five years, one that in the case of the United States ranked as the most severe of the post-war period.

What was the outlook for the two countries, not just over the next few quarters or years, but also over the longer term? How would one begin to assess it? Managers of firms contemplating longer-term investments in real assets wanted to know; so also did participants in financial markets.<sup>14</sup>

One obvious component of such a forecast was the purely economic side of the model—the equations linking nominal income and its price-level and real components to money supply and other policy variables. Another was the set of insights that stemmed from the Friedman–Phelps analysis of the Phillips' Curve and Friedman's conditional forecast of accelerating, not just higher inflation where policymakers continue their attempts to exploit whatever short-run tradeoff existed between unemployment and inflation. A third were the insights derived from inflation experience in Latin America, particularly the finding that inflation had adverse real effects, effects over and above those associated with the holding of high-powered money.<sup>15</sup>

The most important ingredient was, however, the hardest come by, to devise a method of forecasting policymakers' actions over the long time horizons of interest. Would policymakers again rise to the Phillips' Curve bait? If so, when inflation moved even higher and another tightening of monetary policy occurred, would it too only be temporary, or would the higher inflation and the increased costs it entailed provoke a fundamental shift in policy, a change in regime?

No easily adaptable strategy presented itself from the macroeconomic textbooks or journal articles. One possibility was to push the process back a step and examine the link between deficits and money. But that ran into the obvious problem of simply substituting one policy variable for another, besides which the relationship appeared sporadic and highly non-linear for the United Kingdom and virtually non-existent for the United States.

An alternative solution to the problem was to borrow from industrial organization, to try to limit the range of possible policy outcomes by taking explicit account of political factors. Given estimates of the costs and returns associated with alternative policies, knowledge of their impacts on voting behavior and of how that in turn actually filters through to policy, one could in principle project the time path of money supply. In practice, this was exceedingly difficult. The technology necessary for anything even close to precise numerical estimates did not then, and in fact even now does not, exist.<sup>16</sup> Nevertheless, this rudimentary

political model, coupled with Harberger-type analysis of the costs of inflation, did lead to one important qualitative conclusion. It suggested the eventual erosion of support for inflationary monetary policies in both countries and hence a substantial increase in the likelihood of changes in their policy regimes.<sup>17</sup>

In back of these conjectures, however, was a hidden assumption, that the political element encompassed more than just costs and returns narrowly conceived. The possibility that the inflation process would degenerate to the point reached in the high-inflation countries of Latin America, therefore, seemed remote. At the same time, it seemed that sentiment against inflation and the policies that had produced it had increased by more than a shift in such costs and returns could explain.<sup>18</sup>

I continue to believe that this was the case. 'Ideas have consequences,' Richard Weaver argued in his essay of that name. Though Weaver's concerns were not macroeconomic, consider his statement in the context of the example under discussion.

On the purely economic level, ideas about monetary policy and its effects underwent a substantial change during the years in question. One can quite plausibly argue that the misunderstanding of the relationships between real and nominal variables contributed to the initial inflationary bent of monetary policies in both countries. In similar fashion, learning, engendered both by actual experience and economic theorizing, may have been an important factor leading to the reversals in policy that followed in the 1980s.

The influence of ideas on economic policy, I believe, also runs quite a bit deeper. Hayek in much of his work has stressed the relationship between economics and philosophy—epistemology, in particular. The belief in the necessity of activist government economic policy, Hayek argues, ultimately has its roots in epistemology, in particular in the belief that all economic and social phenomena are essentially capable of being controlled, that none are, to use his terminology 'the result of human action and not human design.'<sup>19</sup> I think that it is possible, without greatly stretching the point, to view much of the economic history of Britain and America in this century from such a perspective. Indeed, for the first half of this century direct government action has been the intellectual 'default' in both countries in matters of both microeconomic and macroeconomic concern. Only in the past two decades has there appeared to be much sentiment to the contrary.

The alternative view is that philosophical considerations of this sort are largely beside the point, that they are simply so much window dressing to be used by political agents to rationalize actions that are motivated by totally other concerns. From a methodological standpoint this is certainly much tidier. Where this view, however, seems to me most obviously to run afoul is when we consider differences in policy among countries. The papers by Edwards and Tabellini and by Roubini, and the evidence that they contain on the positive covariation between political instability on the one hand and both inflation and government deficits on the other, are prime examples.

What accounts for this political instability, particularly in the countries at the extreme end of the spectrum that have pursued consistently ruinous inflationary policies? This is of course the time-series forecasting question, but in an even more transparent guise. Ultimately it has got to be answered. The place to look for such answers, again I believe, is in the realm of ideas.

#### IV. Conclusions

The papers in this volume provide us with several sets of stylized facts that are likely to prove useful as building blocks for economists studying the links between politics and economic policy. That economists will delve more deeply into such issues I regard not only as inevitable but entirely fitting. The question of what determines economic policies, or recast to a forward-looking context, of how policies are likely to change through time, is the natural next step after we have analyzed and understood the consequences of those policies.

Extending our knowledge of the links between economics and politics will, I believe, eventually take us farther afield than we now realize. My own predilection is that ultimately we have to get involved in questions that in our own era have largely been the province of political philosophy, intellectual history and philosophy itself.

The major problem with our incursions into these areas is the obvious one, perhaps best illustrated by Adam Smith's famous metaphor of pin production. The advantages of our doing so are that economic theory provides both the powerful tools and the intellectual discipline required for the achievement of even moderate success.<sup>20</sup>

#### Notes

1. Noll (1989) provides a recent review of the industrial organization literature. See Krueger (1990) for a summary of developing country experience that explicitly incorporates this framework. The macroeconomic literature on political business cycles (*e.g.*, Nordhaus, 1975), and on optimal monetary policy (*e.g.*, Kydland and Prescott, 1977; Barro and Gordon, 1983) and the now voluminous literature in public choice theory stimulated by the work of Buchanan and Tullock (1962) provide additional examples of departures from the public-interest view of government behavior.
2. Roubini's sample includes data for both developed and less developed countries for varying periods (depending on data availability) over the years 1950–88. IMF publications again were the major source.
3. See the discussion of moderate inflations in Sargent (1985) for a similar argument.
4. In this regard also see Porterba and Rotemberg (1990). Using long-term time series for the United Kingdom and the United States and postwar time series for both countries and France, Germany, and Japan, they find no consistent relationship between inflation and income tax rates. In France, Germany, and the United Kingdom the two covary negatively; only in Japan and the United States do they covary positively.
5. As I discuss below, these statements have generally focused on the behavior of real output and inflation with foreign-exchange-market, balance-of-payments, and domestic-financial-market conditions also occasionally receiving mention. This is also consistent with estimates of Federal Reserve reaction functions, in which real cyclical variables and inflation generally enter significantly with international variables playing an occasional role (see, for example, Darby and Lothian, 1989).
6. See the discussion by Shimamoto (1983).
7. I have described the mechanism somewhat differently than they. The results of the Federal Reserve's actions, as I have described them, are the same as that which would have occurred had the Fed engaged in unsterilized intervention in the foreign exchange market. The actual transactions are not. I am assuming that the intervention in the foreign exchange market is indeed sterilized, the Fed selling Treasury securities to offset its purchases of foreign currency, and that this is followed by a subsequent lowering of the funds rate target and consequent open-market purchase of such securities.
8. The statements about market participants' sudden emphasis on the trade data are based upon my own recollections of this episode. I believe, however, that these recollections would be substantiated by a review of the publications of research departments in commercial banks and investment banks over this period, or alternatively by a review of the credit

columns in the *New York Times* and *The Wall Street Journal*. In this vein, HMR cite statements in the foreign exchange column in *The Wall Street Journal* as indicative of such a change in emphasis.

9. Though much sanitized and only available with a one-meeting lag, this document is nevertheless scrutinized closely by market participants and Fed watchers. Their major reason for doing so is to monitor the motivations behind FOMC actions. Occasionally, however, doubts exist about whether the FOMC has in fact taken any action. In such instances, the *Record of Policy Actions* achieves added importance. See Mayer (1987) for an interesting discussion of some of these issues.
10. As an example, consider the sentence from the directive for the meeting of the Federal Open Market Committee of February 6 and 7, 1990 as reprinted in the *Federal Reserve Bulletin* of May 1990 (p. 338): 'Taking account of progress towards price stability, the strength of the business expansion, the behavior of the monetary aggregates and developments in foreign exchange and domestic financial markets, slightly greater reserve restraint or slightly lesser reserve restraint would be acceptable in the intermeeting period.'
11. These data therefore basically corroborate the HMR account, but suggest that the time span over which policymakers were reacting to the trade deficit was shorter than particular subperiods into which HMR divide the sample would imply.
12. These results also provide evidence on a somewhat related question, why financial markets in one time period appear to focus very heavily on one type of economic information to the exclusion of most others and in another time period focus on something different. Rather than signalling fickleness of one sort or another on the part of market participants, as is sometimes alleged, such shifts in emphasis are, I believe, largely attributable to similar shifts on the part of policymakers.
13. The empirical generalizations in the discussion that follows for the most part are based on results summarized in Darby and Lothian (1983).
14. My involvement in these questions began in 1976 at Citibank in response to a request from a client of the bank for an assessment of the long-term economic prospects of the United Kingdom. This firm already had a UK subsidiary and, given the turbulent economic conditions there at that time, was reevaluating its position. The resultant report (Lothian and Burrell, 1976) and the considerations that entered into it provide the motivation for much of the discussion below.
15. I have in mind here the studies by Arnold Harberger, Larry Sjaastad, and their students in the Latin American Economics Workshop at the University of Chicago in the latter half of the 1960s. (See, for example, Harberger, 1966, and the later amplification of these views in Friedman, 1977).  
Another finding for Latin America that appeared relevant to Britain and America at this time concerned the relationship between money supply and the price level, how it was likely to be affected were inflation to accelerate and become both more variable and more difficult to forecast. It had been observed that the lags between the one and other tended to shorten under such conditions, economic agents presumably adjusting their pricing behavior in the face of these greater difficulties. Such a relationship was of course later formalized in Lucas (1973).
16. Consider each of the three steps in the process. Identifying the (net) costs of inflation has still not proceeded much beyond Friedman's (1977) discussion. Even relatively successful voting models, as Peltzman (1990) points out, are of little use in forecasting. Correspondingly, principle-agent problems, and the like, are liable to plague attempts to translate ballot-box behavior into actual policy.
17. My conclusion at the time was that this would happen in something inside of a decade in Britain, with the inflation situation in the latter half of the 1970s, and perhaps the first half of the 1980s, deteriorating further.
18. Citibank (1978) contains a statement of this position. Obviously, this again is conjecture. It is, however, testable along the lines of Kalt and Zupan (1990).
19. In this paper as in other work, Hayek links the belief in the efficacy of an interventionist stance by government to epistemological notions traceable to the work of Descartes, and even earlier the Greek sophists.
20. See Hayek (1967b) for an insightful discussion of the relative costs and benefits of specialization as opposed to more general education. He stresses the interplay between philosophy and economics in particular.

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