

Growth is not correlated with Inflation

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Abstract A paper by W. R. J. Alexander concluded, on the basis of econometric analysis involving variables additional to the two principal ones, that a decrease in inflation rate would result in a significant gain in the growth rate of national output. This note shows that this verbal conclusion does not follow from the results of the algebraic analysis which precedes it, and more generally, that time-series analysis, with or without additional variables, is unlikely to be able to contradict the conclusion of simple two-parameter cross-section correlation studies - namely that the growth rates of countries are not correlated with their inflation rates.

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Introduction

My paper "Is low inflation an important condition for high growth?" (Stanners 1993) concluded: "No evidence has been found to support the notion that a low rate of inflation has in the past and in various countries been associated with improved growth rate".

In a further note (Stanners 1996), I examined the results of a paper by R. Barro (1995), concluding that these results, although presented forcefully as supporting the case for low inflation, were on the contrary further evidence for the statement quoted above.

The occasion of the present note is a paper by W. R. J. Alexander (1997).

Alexander, like Barro, gives strong support in the conclusions of his paper to the policy-aim of a low level of inflation.

Alexander's paper

The paper deals "in a pooled time series and cross-section fashion" with 11 OECD countries over the period 1966 to 1988. Remarking that reported work which regresses simple growth data against inflation data "must necessarily be flawed", it begins with a regression of growth data on net capital and labour increments, and then successively adds the annual inflation rate, the annual change in the inflation rate, government expenditure, and exports. Highly significant coefficients are found for all parameters - positive for capital and labour inputs, and for government consumption and exports, negative for inflation and delta-inflation. No results are reported for regression of growth on the inflation data alone. Nor are inter-country (cross-sectional) results given, on the grounds that the data are too sparse.

I have no particularly adverse comment on the work of the paper apart from its last paragraph which states the paper's conclusion. The purpose of this note is to point out, not only that the conclusion does not follow from the numerical analysis, but that, in general, work involving additional modelling parameters cannot lead to any useful (in the sense of being predictive in the real world) statement on the relationship between the two parameters, growth and inflation. Such work falls into the trap of assuming that some theory must be better than none.

The paper's conclusion is as follows:

"There is little existing empirical evidence on the relationship between inflation and growth, and much of what evidence there is fails to control appropriately for growth in real inputs. The present work uses a small sample of OECD countries for which capital stock and labour force data are available to investigate, in a pooled time series

and cross-section fashion, the relationship between inflation and growth. Strong evidence is found contrary to the hypothesis that there should be no association between inflation and real growth. Even if inflation had been at a constant rate for some time, the result implies lost growth on a scale similar to that claimed by Grimes. Yet, to the loss of growth from a high level of inflation that this finding implies must be added further loss from an increase in inflation. Taking these combined effects, a reduction in inflation from, say, 6% to 2% would imply an improved growth performance of the order of 0.93176%, or close to 1% p.a. There is no need to stress the long-run benefits of such an increment to growth."

Comments on the paper

In what follows, I will follow the paper in using the adjective "atheoretical" to describe work involving simple regression using the raw data on growth rate and inflation available in official statistics. No argument is given in the paper as to why "any study of growth which fails to control for the growth of the capital stock and the growth of the labour force must necessarily be flawed", or why two, and only two, further variables are added. The author remarks that "there is no shortage of candidate regressors", but "many of them are dubiously measured socio-political variables", and merely adds, "let us check the effect of allowing government consumption and exports to enter the equation". There is no later discussion of the results of this "check", apart from the tabulated (significant) results and the observation that the inflation relationship is "robust" when these variables are added. No attempt is made to confront or reconcile the new results with existing ones. The paper passes from the tables of coefficients and t-statistics to the quoted conclusion without a backwards or a sideways look.

The last sentence of the conclusion, together with the reference to "lost growth", makes it clear that the paper is supporting a prescription for action in the real world. However, the gap between the analysis and the prescription involves at least five unstated and undiscussed assumptions:

First, that the response of a complex real economy to a given administrative measure is likely to be forecastable by an arbitrary few-variables model. There is no mathematical model which comes near to a complete description of real economies. The use of a simple model, not, as might be valid, to throw tentative light on the past, but to give unguarded and unqualified prescriptions for the future of a highly complex economy, can only introduce confusion.

Second, that a statistically established relationship is a causal one, and that the causal process is in the chosen direction. Cause is a highly complicated concept. Perceived correlation and time-sequence can certainly play a part in establishing a conviction of cause, but correlation cannot determine cause. The decades-long history of controversy over the statistics related to smoking and lung cancer, where the data were vastly more plentiful and the context much simpler and better-understood than in the present instance, demonstrates both parts of that observation.

Third, that the variable chosen as causal, namely inflation rate, is in reality capable

of being set at a pre-determined low value, and that the claimed consequence of this setting, an improved growth rate, can be stated without reference to the other variables. This tacitly assumes that each other variable is also capable of being set at a pre-determined value - for example, held constant. It also ignores the fact that an observed long-term independence of inflation and growth rates cannot be refuted by showing that growth is correlated with inflation *and* other parameters. An easily visualised (imaginary) example makes this clear. Suppose it is established that men's weights do not correlate with height. To show subsequently that men's weights are correlated with height *and* girth, does not oppose this. It means that *for a given girth*, weight correlates with height. Both observations can be true, if tall men tend on average to be thin and short men fat, i.e., if height and girth are themselves correlated. In one of the models in the paper, it might be argued along those lines that high inflation would, *other things being equal*, have a negative effect, but that the real world is so arranged that this effect is cancelled by a quasi-automatic increase in labour and/or capital inputs.

Fourth, that a result based on yearly-varying data, thus including short-period business cycles, can confidently be used to prescribe a long term policy-aim. The paper is dealing with year-by-year inflation rates, as do the other papers cited in support. That is, the reported correlations largely reflect the poorly understood dynamics of business cycles, involving factors such as moods and administrative reactions, rather than the underlying factors governing the long-term prosperity of the country. There seems, indeed, to be little doubt that for most advanced countries in the period since World War II, year-by-year growth data are inversely correlated with year-by-year inflation data (even if the correlation coefficients of Gomme (1993) cited (again to 5 significant figures) by Alexander have in fact no evidential value in this regard due to the absence of data in this reference on the statistical significance of these coefficients). However, if long-run *time-averaged* data for a large number of countries show no correlation of growth rate with inflation rate, then it is difficult to see what is the significance of these time series results. Many variables must cycle either with or counter to growth, and the fact that prices show some tendency to do so is surely not surprising. But to state positively that growth would be helped if just one of these variables, arbitrarily chosen, were somehow held at the value attained at the top of the growth cycle is not logically valid.

The main fault is thus to imply that an algebraic finding that there is no correlation between A and B can be contradicted by an algebraic finding that there is a correlation between A, B, C, D, E and F. This implication is conveyed in the paper simply by omitting C, D, E and F in the verbal re-expression of the latter algebraic finding, so that in effect it reads, "there is a correlation between A and B". Further errors in the conclusions of the paper are that B *causes* A, and that a prescription for future action in the real world can be based on this cause-and-effect. A substantively weak conclusion is made to appear strong by simply omitting "let us assume" and "other things being equal". Even if the model was a complete description of the economy, and causality was proved, the advice to the minister in control of the economy would have to be in terms of controlling suitably all of the variables concerned.

Logically, the paper's conclusion should be:

"If the complex real economy of a country did behave according to one of the six models, and if it was possible in a real economy to ensure that none of the other variables changed during a certain period, then any change in the rate of inflation in that period would be *associated with* a change of opposite sign in the rate of growth." It might go on: "If it is in fact observed that inflation in different countries has no systematic relationship to the growth rate of these countries (and nothing in the paper refutes this¹), then *according to the model*, this would be because, in the absence of means of controlling the variables, any change in inflation rate is systematically counterbalanced by opposing changes in other variables. For example, in the second model, an increase in inflation rate would be balanced by increases in capital and/or labour inputs (or vice versa)." No mention of cause or antecedence would be permissible.

Tentative remarks of this nature might have been technically interesting on their own terms. The last observation, for example, happens to be a re-statement of two currently unfashionable ideas which the paper briefly cites in an introductory discussion: "In the Phillips curve tradition it was long assumed² that there was a negative correlation between inflation and unemployment. According to the Tobin-Mundell hypothesis an increase in inflation ought to cause a substitution away from money to investment in fixed capital, with a consequent positive impact on the rate of economic growth."

But the conclusion is far from tentative. It states categorically that a four percentage point reduction in inflation would cause (the word "imply" is used but later text leaves little doubt that "cause" is meant) additional growth of 0.93176% (sic) per annum.

The treatment of inflation at some length in the conclusion is in contrast with the omission of any reference at all to the other variables. The statistical analysis showed the *positive* effect of an increase in government expenditure or exports just as clearly as the *negative* effect of inflation. If true, this would be of technical interest, seeming to favour small, open, high government countries, but this is not mentioned in the conclusions, perhaps because it does not lean in the currently approved direction regarding government expenditure.

More general comments

In saying that other atheoretical studies are flawed, the paper falls into the trap of assuming that some theory must be better than none. My atheoretical paper of 1993 undertook a comparatively simple task. Everybody in politics and the media seemed to take it as axiomatic that low inflation was good for growth, so surely the evidence should have been plain to see at a glance. I looked in simple ways, but fairly diligently, at the superficial data on inflation and growth and could find no evidence for this.

More recently, I became aware of a further example of atheoretical evidence of the robustness of output in the presence of inflation through a remark of R. Barro in his book "Macroeconomics" (p201):

"(In) the post-World War I German hyperinflation we have something close to a laboratory experiment for studying the consequences of high and variable rates of monetary growth and inflation. Over the period from 1921 to 1923, the rates of inflation ranged from near zero to 500% per month! Further, the available data suggest that relatively small changes occurred in aggregate real variables, such as total output and employment."

The observation regarding aggregate real variables is not referred to again, in a chapter chiefly dealing with levels of real money balances, transaction costs and real government revenues obtained from printing money.

The references given by Barro (Bresciani-Turroni, 1937 and Cagan, 1956) confirm that pre-monetarists who studied the post-WWI or post-WWII inflations in Germany, Hungary, Russia, Austria and Poland, related in a seemingly unsurprised and off-hand way that output, although cut, maybe by a third or a half for some months, was far from halted. Remembering that the German 1913 Mark was worth 10 thousand Marks in January 1923 and *10 billion* in November 1923, while unemployment was 4% as late as July 1923, this superficial evidence thus adds powerfully, in my view, to the evidence that less apocalyptic inflations may well be growth-neutral.

Barro, in his paper, and Alexander, set themselves a very much more demanding task than mine. I and others have looked atheoretically at the raw growth/inflation data bearing on a belief, held almost unanimously by intelligent people to whom this evidence is readily available, and we have reported on how much we searched and on what we found - namely no supporting evidence. Alexander's task is to prove that the popularly held belief is a truth which lies hidden behind the seemingly opposed raw data. It seems to me much more credible and in line with historical precedent that people may believe something which makes a plausible story, or which they want to believe, or which it is in their interest to believe, or simply which it is fashionable to believe, than that they have collectively, almost unanimously, and relatively suddenly, stumbled by chance on a truth so deeply hidden as to be detectable only by econometrics.

Alexander makes the mistake of assuming that some theory is better than none, and indeed states with double emphasis that an atheoretical treatment *must necessarily* be flawed. This is clearly not so, as I hope the above has illustrated. When the situation is that there is no mathematical model which comes anywhere near to a complete description of real economies, there may be no alternative to studying atheoretically the untreated growth and inflation data. A conclusion reached by such a study may or may not be correct, but it cannot possibly be modified by an argument involving one or more (but in the nature of things never enough) additional variables. Adding more un-understood relationships to the one you already have can only introduce more doubt and confusion, even if, as is too often seen, this confusion allows ill-based and tendentious conclusions to be laid plausibly before the reader.

Economics and truth

Economics is uniquely beset by this type of problem. In literature, history and the non-numerical social sciences, plausibility, wit, cleverness, the appeal to the audience,

are of the essence. Tendentiousness is not necessarily a vice in such studies. In the hard sciences, at the other extreme, the audience is irrelevant. The appeal is to fact. Tendentious results fail to be replicated. Economics is alone in having to deal with numerical data in a rhetorical context, i.e., a context in which the accrediting appeal is to the audience rather than to the factual world, for propositions which are presented as numerical and factual. A small but telling, and far from rare, example of the effect of falling between these two stools is that the number 0.93176% quoted above in Alexander's conclusion - a change of 1 in the last place representing 10^{-7} , or say one millimetre in 10 kilometres - simply could not appear in any engineering context, since engineers are continually dealing with numbers in the real world. Absolutely nothing in engineering (and little for that matter in physics) is accurate to 1 in 10 million. Why then can such a number appear in a subject in which quantities cannot even be defined with rigor, much less measured with a stated accuracy? The reason may be that economists are not as a daily experience brought up sharply by confrontation with fact. A competent economist may often be confronted by a colleague who disagrees with him, but almost never by one who can say flatly "the facts prove you wrong". A number emerging from a computer with 6 significant digits is transferred to the published page. It is rarely tested against reality, only on whether it passes muster with the audience.

This is a milieu in which it is only too easy to pursue a numerical investigation until the desired verbal conclusion can plausibly be extracted, and then to stop. In the absence of confrontation with fact, professional intellectual rigor must remain the only barrier to tendentious argument. This is not a novel observation, of course. Many similar remarks could be cited - see for example Clower (1989) and Colander (1989). Little can be added, indeed, to the opening words of Malthus' *Principles of Political Economy* (1820):

"It has been said ... that the conclusions of Political Economy partake more of the certainty of the stricter sciences than those of most of the other branches of human knowledge. Yet we should fall into serious error if we were to suppose that any propositions, the practical results of which depend upon the agency of so variable a being as man, and the qualities of so variable a compound as the soil, can ever admit of the same kinds of proof, or lead to the same certain conclusions, as those which relate to figure and number. ... The science of political economy bears a nearer resemblance to the science of morals and politics than to that of mathematics. ... This conclusion ... is further strengthened by the differences of opinion which have prevailed among those who have directed a large share of talent and attention to its study."

Conclusions

The main fault of Alexander's paper is to imply that an algebraic finding of no correlation between A and B can be contradicted by an algebraic finding of correlation between A, B, C, D, This implication is conveyed in the paper simply by omitting any mention of C, D, and further variables, in the verbal re-expression of the latter algebraic finding, so that in effect it reads, "there is a correlation between A and B". The further verbal implications in the conclusions of

the paper that B *causes* A, and that a prescription for future action in the real world can be based on this cause-and-effect, are erroneous on several counts.

Thus, the paper does not, contrary to the implication of its conclusion, refute the evidence that the long-term growth rates of countries are not correlated with their inflation rates.

Notes.

1. The paper analyses growth "in a pooled time series and cross-section fashion" in terms of:

- 1) Capital input, labour input
- 2) Capital input, labour input, inflation
- 3) Capital input, labour input, inflation, change of inflation
- 4) As 1), plus government consumption, exports
- 5) As 2), plus government consumption, exports
- 6) As 3), plus government consumption, exports

It does not give, or refer to, results of this type of analysis for growth against inflation alone. It is remarked also that "there are too few countries in the sample for cross-sectional work".

2. This formulation was indeed "long assumed", but is a misreading of the original Phillips curve. A. W. Phillips (1958) did not put forward the idea that there is a tradeoff between inflation and unemployment. He noted that there was during the period 1861 to 1959 a rather well defined relationship between the rate of increase of nominal *wages* and the level of *unemployment*. Prices and output as such were not involved. He did not recommend that price inflation should be stimulated or accepted in order to promote employment, but that "aggregate demand" should be regulated to keep prices stable (i.e., to promote zero price inflation rate) while nominal wages (in these circumstances also real wages) had a positive "inflation rate" reflecting productivity gains. This view might today be applauded by any central bank.

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