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OPTIMAL STABLE POLICIES FOR UNSTABLE INSTRUMENTS

BY CHRISTOPHER A. SIMS

R. S. Holbrook (1973) showed that attempts to minimize deviations of an endogenous variable from a target may produce explosive time paths for the instrument variables. Cagan and Schwartz (1973) showed that for a situation where GNP is endogenous and a monetary aggregate the instrument, some estimated lag distributions in the literature imply the presence of instrument instability. A formal solution to the problem of instrument instability is complete when one simply notes that if one cares about the explosive time path of the instrument, the instrument ought to appear in the objective function—"minimizing deviations of the endogenous variable from a target" is not the appropriate objective. However, if we consider optimal policy with an infinite time horizon and with instrument variance entered in the objective function with small weight, the limit of such policies as weight on the instrument goes to zero is not the same as the optimal policy when the weight is exactly zero. Furthermore, optimal policy with zero weight on instrument variation does not depend on the time horizon. This situation of discontinuity has practical implications for economic applications of optimal control, as it implies that correctly computed optimal policies using realistically small weights on instrument variation and long but finite time horizons may be unstable and quite different from the stable infinite-horizon solution.

Optimal policy aimed at minimizing deviations of an endogenous variable from some target may produce an explosive time path for the instrument variable, as Holbrook (1972) has pointed out. Recently Cagan and Schwartz (1973) have shown that in a simple system with a monetary aggregate as instrument and current-dollar GNP as endogenous, this situation of "instrument instability" arises if some of the estimated lag distributions from income on money regressions in the econometric literature are taken as describing the influence of instrument on the dependent variable. They have argued that therefore discretionary monetary policy ought to be replaced by a simple policy rule that would prevent policy from adding to instability while preserving finite variance in the instrument.[1]

This paper grew out of consideration of the following question: In a situation of instrument instability, is there a well-defined best policy subject to the constraint that the instrument did not explode? The answer is yes. It is also found that if we put bounds on the level or the variance of the instrument, the optimal constrained policy does not converge to the optimal unconstrained policy as the bounds are relaxed. In the situation considered by Cagan and Schwartz, therefore, we can find the proportion of the forecastable variance in GNP which can be eliminated by use of a monetary aggregate as policy variable without inducing explosive oscillations in money. If it were to turn out that this proportion is close to unity, the Cagan and Schwartz objection to discretionary monetary policy would lose most of its force, whereas if this proportion were close to zero, their objection would be confirmed.

Economists working with larger models than the monetarist single equation are apt to scoff at the very idea of instrument instability. In the abstract, instrument instability is either no problem at all or a specification error in the objective function. If you are worried about an explosive time path for an instrument, the

[1] Cagan and Schwartz regard this point as further support for Friedman's (1959 e.g.) arguments against discretionary monetary policy.

size of that instrument ought to appear in the objective function. If you are not worried about the size of the instrument as a target, then there can be no objection to explosive time paths.

There are probably no variables in most economic models which are "pure instruments" in the sense that they ought not to enter the objective function at all. But there certainly are variables, like money supply or relative price variables, which should enter the objective function with weights so small that historically observed patterns of variation in them have negligible effects on the value of the objective function. If somewhat larger than usual oscillations in the money supply would reduce unemployment and inflation simultaneously, even by fairly small amounts, few economists would worry about the welfare effects of the oscillations in money. But if a policy optimization in an econometric model suggested oscillations in the money supply by a factor of 100 from quarter to quarter over a four-year horizon, most economists would be skeptical of the solution, for two reasons. First, oscillations that large achieved by the usual policy measures might impair the functioning of the banking system and securities markets, causing a real welfare loss, even if the model were correct. Second, small errors in specification in the model, or small random fluctuations in the model's policy multipliers, would be magnified by such large movements in a policy variable. Putting the same thing another way, the specification error in the model when it is extended to historically unprecedented policy inputs may in fact be large.

We could say then that the instrument instability problem, when it appears, should be resolved by making variation in the instrument part of the objective function, albeit with small weight. Unfortunately in actual economic control problems use of a finite time horizon becomes a computational necessity. Unless the time horizon is at a point so far in the future that we in fact are indifferent to what happens after that point, use of a realistically small weight on variation in the instrument in the objective function in a situation of instrument instability can be shown to produce very bad policy solutions. One possibility is to use an infinite-horizon solution with small realistic weight on instrument variation in the objective function. But if a fairly short horizon is a computational necessity, a good approximate solution can be found by optimizing subject to the constraint that the policy path be stable.

In what follows the best stable policy is explicitly derived for a simple bivariate linear model with quadratic objective function. We show also that this policy is arbitrarily close to any policy with long enough time horizon relative to a small enough weight on instrument variation in the objective function. In the final section are some suggestions as to how instrument instability can be recognized and its bad effects avoided in practical attempts to find optimal policies in econometric models.

1. Instrument Instability in a Simple Model

Suppose we have a structural equation

$$y(t) = a * x(t) + u(t), \quad (1)$$

where "$*$" is the convolution operator and is defined by

$$a * x(t) = \sum_{s=-\infty}^{\infty} a(s)x(t-s).$$

We will assume throughout that $a(t) = 0$ for $t < 0$. The variable y is the endogenous or output variable, x is the control variable, and u is the exogenous input or disturbance variable. Suppose the objective at each point in time is to minimize $y(t)^2$, and that over time our objective function is

$$Q = \sum_{s=1}^{T} R^s y(s)^2, \tag{2}$$

where $R \leq 1$ is the discount rate. If the u function is known and $x(s)$ for $s \leq 0$ is known, then optimal policy need consider only $u(t)$ and $x(s)$ for $s < t$ in the choice of $x(t)$, setting $x(t) = (1/a(0))(-u(t) - \sum_{s=1}^{\infty} a(s)x(t-s))$. This rule achieves $y(t) = 0$ over the full time horizon $t = 1, \ldots, T$.

Now if this rule has been followed for all past t and will be followed indefinitely into the future, the functions u and x are related by $a * x = -u$. If in addition $\sum_{s=0}^{\infty} a(s)^2 < \infty$, a will generally have an inverse under convolution[2] which is itself a square-summable function on the integers. The function a^{-1} may or may not satisfy the condition $a^{-1}(s) = 0$ for $s < 0$. In those cases where a^{-1} does satisfy this "one-sidedness" condition, we can write $x = a^{-1} * u$ to display the way current and past values of u determine $x(t)$. But where a^{-1} is not one-sided, the assumption that the optimal rule has been followed for all time still implies $x = a^{-1} * u$. We know that the optimal rule makes x a function of past values of itself and of current and past values of u. In fact, it is easy to see that we can always express $x(t)$ as a function of $u(s)$ for $-M < s \leq t$ and $x(s)$ for $s \leq -M$. If the influence of $x(s)$ for $s \leq -M$ on $x(t)$ dwindles to zero as M goes to infinity, $x(t)$ can be written as a function of current and past values of $u(t)$ alone. But where a^{-1} is two-sided we know this is not possible, so in this case the influence of the distant past of x does not dwindle away as time goes on. Initial conditions have an undamped, and possibly explosive, effect. This is instrument instability.

2. The Best Stable Solution

To make your candles last for a',
You wives and maids give ear-o!
To put 'em out's the only way,
Says honest John Boldero.[3]

Whittle (1963) takes what is labeled instrument instability in the foregoing section to be the standard situation in his treatment of linear regulation. In his treatment, $u(t)$ is a stationary stochastic process, and the problem is formulated as choice of the best linear relation between $x(t)$ and those values of $u(s)$ observable

[2] The exception, which may arise when a's Fourier transform has a zero for a real argument, is unimportant in that for such an a we can still choose a square-summable b for each $\varepsilon > 0$ to guarantee that $b * a(0) = 1$ and $\sum_{s \neq 0} b * a(s)^2 < \varepsilon$.

[3] Nursery rhyme.

at time t. Bestness, in this stationary stochastic framework, is defined as minimization of the variance of $y(t)$.[4]

Formulated in this way, the problem has a well-defined solution ordinarily, even if the variance of $x(t)$ is not added to the objective function. By the method described in Whittle, one creates a Hilbert space H_u from $u(t)$, $t = -\infty, \ldots, \infty$, under the covariance inner product. If constraints on information available to the policy maker can be expressed as limiting $x(t)$ to a closed subspace $\bar{H}_u$ of H_u, $a * x(t)$ is limited to the subspace $a * \bar{H}_u$ found by mapping each element z of $\bar{H}_u$ into $a * z$. The solution is then to set $a * x(t)$ equal to the projection of $u(t)$ on $a * \bar{H}_u$. When a^{-1} is one-sided and $\bar{H}_u$ is given the natural form making it the subspace spanned by $u(t + s)$, $s \leq h$, then $a * \bar{H}_u = \bar{H}_u$. In this case the form of a puts no limits on the choice of $a * x$, and all forecastable variance in $u(t)$ is eliminated. Where a has no one-sided inverse, on the other hand, not all forecastable variance in $u(t)$ is eliminated, but a determinate solution is found. Except when the Fourier transform of a has zeros for real arguments, the solution yields an $x(t)$ with finite variance.

We know from Section 1 that starting from any initial condition all forecastable variance in u can be exactly eliminated. When there is no loss of welfare from explosive time paths for x, one clearly does better eliminating all forecastable variance in u than by using the stationary solution for x. If the objective function puts weight on variance in x, the objective function for the stationary problem becomes

$$\bar{Q} = E[y(t)^2 + bx(t)^2] \tag{3}$$

while for the problem of Section 1 it becomes

$$Q = \sum_{s=1}^{T} (y(s)^2 + bx(s)^2)R^s. \tag{4}$$

When b is positive and $R = 1$, the solution to the problem of Section 1 with forecasts of unobserved u's treated as certainty-equivalents converges to the stationary solution as T goes to infinity. Therefore, as b goes to zero, the infinite-horizon solution converges to the stationary solution, even though when b is exactly zero, the certainty-equivalent solution does not converge to the stationary solution as T goes to infinity.

Before discussing the implications of these results, we will derive them precisely in a model with specific choice of a.

3. An Explicit Solution

Peter White will ne'er go right.
Would you know the reason why?
He follows his nose where'er he goes,
And that stands all awry.[5]

[4] Whittle does treat non-stationary and non-stochastic inputs, but his approach to two-sided a^{-1} is fairly represented by this simpler model.

[5] Nursery rhyme.

Let us take $a(t) = 0$ for $t > 1$, $a(0) = c$, and $a(1) = 1$. Let us suppose further that $u(t)$ is a covariance-stationary stochastic process. Making the natural assumption on information flow that $x(t)$ is chosen with knowledge of $u(s)$ for $s < t$, an explicit solution is possible for least-squares predicted future values of u at each point in time.[6] We will designate by $\hat{u}_1(t)$ the time path expected for u based on information available at time 1 (when $x(1)$ is chosen).

Consider the problem of minimizing $E(Q)$, where Q is defined in (4) above and the expectation is conditioned on $u(s)$, $s < 1$. This is well known to be equivalent to solving the deterministic minimization problem obtained by replacing u by $\hat{u}_1$ in Q. The first-order conditions for this problem yield the difference equation

(5) $$cRx(t+1) + (c^2 + b + R)x(t) + cx(t-1) = -c\hat{u}_1(t) - R\hat{u}_1(t+1),$$
$$t = 1, \ldots, T-1,$$

with the end-point condition

(6) $$(b + c)x(T) + x(T-1) = \hat{u}(T)$$

and an initial condition determined by the known value of $x(0)$. Equation (5)'s characteristic equation has two roots which, when expanded to terms of first order in b, are given by,

(7) $$G_1 = (-1/c)[1 + (b/(R - c^2))], \qquad G_2 = (-c/R)[1 - (b/(R - c^2))]$$

(for $R > c^2$; effects of b on roots somewhat different for $R < c^2$).

The general solution to (5) can be written as

(8) $$x(t) = -a^{-1} * \hat{u}_1(t) + K_1 G_1^t + K_2 G_2^t,$$

where $\hat{u}_1(t)$ is defined indefinitely into the future by the least-squares forecasting rule and into the past by setting it equal to u's known past values. The constants K_1 and K_2 are determined by the initial and endpoint conditions.

If $b \neq 0$, (6) and the initial condition will force the K_j applying to the unstable root to zero as T goes to infinity. If b is zero and c is less than one, the unstable root $(-1/c)$ will not be eliminated as T goes to infinity. In fact, it is easily shown that when $\hat{u}(T) = 0$,

(9) $$K_1 = k_1 A, K_2 = k_2 A,$$

where $A = x(0) + a^{-1} * \hat{u}(0)$ and k_1 and k_2 depend only on T, b, c, and R. As T goes to infinity with b, c, and R fixed, k_1 goes to zero and k_2 to one. The sum $k_1 + k_2$ is identically one. Solving for $x(1)$, we find

(10) $$x(1) = -a^{-1} * \hat{u}_1(1) + (G_1 k_1 + G_2 k_2)A.$$

Imagine now that we find each value of $x(t)$ by solving the foregoing problem over the horizon $t + 1, \ldots, t + T$. This will produce

(11) $$x(t) - (G_1 k_1 + G_2 k_2)x(t-1) = -a^{-1} * \hat{u}_t(t) + (G_1 k_1 + G_2 k_2)a^{-1} * \hat{u}_t(t-1).$$

The right-hand side of (11) is a stationary stochastic process with finite variance.

[6] These are of course also conditional expectations if $u(t)$ is Gaussian.

Thus it is easily seen from the left-hand side of (11) that $x(t)$ is a stationary process if the absolute value of $G_1k_1 + G_2k_2$ is less than one.

If we allow T to go to infinity and then let R go to one and b to zero, (11) becomes

$$x(t) = -cx(t-1) - c\hat{u}_t(t) - (1-c^2)\sum_{s=0}^{\infty}(-c)^s\hat{u}_t(t+s+1). \tag{12}$$

It is a mildly tedious but straightforward matter to verify that (12) is exactly the solution to the stationary problem obtained from Whittle's approach.

Some remarks about these results are now in order. The moving-horizon solution (11) with non-zero b may lead to a (disastrous) unstable policy if b is small and T not large enough. This despite the fact that as time goes on the variation in x comes to dominate the loss function; the short horizon makes it seem always worthwhile to worsen the instability in x, no matter how bad the situation has already become. With very small b the T necessary to guarantee stability for x may be very large, and the size of this T has nothing to do with the interval over which $u(t)$ is showing interesting behavior. Thus choosing T just longer than the forecasting "horizon" may lead us awry.

Note that the performance of the stable policy (12) relative to the unstable optimal policy depends crucially on how $u(t)$ behaves. If $\hat{u}_t(t) = \hat{u}_t(t+s)$ over a range of s fairly far into the future, then it is easily verified from (12) that we will have approximately $a * x = -\hat{u}$, where the right-hand side is interpreted as the stochastic process of one-step-ahead forecasts of $u(t)$. In this case, then, the effect of the control on y is little affected by the restriction to stable solutions. The assumed behavior for $\hat{u}_t(s)$ appears, e.g., if $u(t)$ is a smooth process plus a white noise. On the other hand in this example, with c positive and less than one, strong negative serial correlation in u could make the stable policy have much less effect on y than the unstable policy.

To obtain (12) from the moving-horizon solutions we had to set $R = 1$. So long as $R > c$, a discounted objective function does not lead to unstable x; but the stable, infinite-horizon solution with $R < 1$ is not the best stationary solution. Finite-horizon solutions with T great enough to guarantee stability but not large enough to bring the solution all the way to the form of (12) provide an analogy. In both cases, the average value of $\bar{Q}$ over time is higher than the best achievable value, but remains bounded. When $R < 1$, some current reduction in y variance is always being purchased at the expense of a larger deferred cost in x variation. Thus it is not obvious that the stationary solution ought always to be applied. It may be useful to provide a benchmark against which to measure the expected losses from other stable policies, but the benefits from moving toward the best stationary solution may not exceed the short-run costs in the presence of discounting.

The case where c is close to one requires special consideration. When c is exactly one, a^{-1} does not exist. The best stationary policy still exists, but it is identical with the best unstable policy, and both involve an $x(t)$ with infinite variance. The explosiveness in optimal x in this case is linear—$x(t)$ is a process with uncorrelated increments—instead of exponential as when c is less than one.

This case is more difficult to deal with than either the stable case or the exponentially explosive case, because we can no longer hope that the solution will be insensitive to the choice of b for small b and large T. In the stable or exponentially explosive cases, the fact that we know that b should be small is enough to determine a good policy; in the $c = 1$ case, there is no way of avoiding the need for a precise choice of b and a very long time horizon in arriving at the best policy.

Though cases with c exactly one are not likely in practice, cases with c close to one, whether above or below, are quite likely.[7] Though with c above one the optimal policy with $b = 0$ is stable and with c below one it is not stable, in both cases the solution will be sensitive to b until b gets extremely small, so that the $b = 0$ solution may lose its practical relevance.

4. Implications for Practice

I think most of the remarks at the end of the previous section are important considerations in controlling actual econometric models. Time horizons used in controlling such models have not been so long that we can reasonably claim to be indifferent to what happens beyond T, and in the one example I know of where the effect on policy of changes in objective-function weights on instrument variation has been examined [Holbrook (1973)], the effect is large. This suggests, without proving, that instrument instability may be a problem, so that finite-horizon solutions are leading us down an unstable path.

Though finding a stationary stochastic optimal policy for a large econometric model is not impossible, the Monte Carlo techniques required in the case of nonlinear models would probably be prohibitively expensive. Expense probably also rules out any attempt to work with extremely long time horizons.

There is, however, what seems to me a practical way to avoid serious error, via appropriate terminal conditions on policy. The instability in the system which is stored up in the terminal period state vector is, in economic terminology, a stock with negative value. To guarantee reasonable results from a finite-horizon optimization, the terminal value of that stock must either be constrained or entered into the objective function. A natural way to evaluate the stock is to return all policy variables to zero (or their trend values) and let the system run until it damps, then add up the losses. In practice then, the value of this stock could be entered into the objective function by optimizing in the usual way subject to the constraint that policy variables be set at zero over the last k periods. These last k periods would ordinarily be chosen to come after forecastable variation in disturbances has died away; so they would represent a net increase in the time horizon; but because the additional periods have no free policy choices attached, they do not add to the dimensionality of the optimization problem.

The foregoing suggestion would certainly prevent unstable solutions. Where system dynamics are slowly damped, a solution with such terminal conditions might, for small T and k, fail to be a very good solution; the finite-horizon solution without such terminal conditions could well be better. But the usefulness of such terminal conditions can be tested by varying T with k held fixed. If the variation

[7] They would arise, e.g., in the discrete-time equivalent of an exponential lag distribution with locally smooth $x(t)$. See Sims (1971).

in T leaves the first-period decision nearly unchanged, the terminal conditions are functioning correctly. A similar insensitivity to T for the solution without the terminal conditions does not confirm the appropriateness of that solution. It is easy to verify in the simple example of the preceding section that even with $b = 0$, terminal conditions like those suggested here bring us close to the best stable solution with short time horizon when c is not too close to one.[8]

5. The Money/Income Example

The estimated lag distribution relating GNP to current and past values of the monetary base (MB) from my 1972 *American Economic Review* article is, for $t = 0, \ldots, 8, 0.603, 0.593, 0.509, -0.029, -0.011, -0.865, -0.037, -0.296, 0.072$.[9] The Z-transform of this lag distribution has a pair of roots with absolute value 0.813 and angular coordinate corresponding to a frequency of 10.21 quarters. With this small an absolute value, the naive variance-minimizing policy would make MB explode very rapidly (at a rate of 23 percent per quarter).

Now suppose that the residual $u(t)$ in the explanation of GNP by current and lagged MB has an autoregressive representation of the form $u(t) = 1.5u(t-1) - 0.5625u(t-2) + e(t)$, where $e(t)$ is white noise. (This form of serial correlation for the residuals was assumed in the AER article, and tests found no conclusive evidence against it.) Then using information on past values of GNP and MB alone, and assuming the estimated lag distribution is the true one, the unstable optimal policy would use MB to exactly eliminate the effects of lagged u's, so that for observed values of GNP we would have $\text{GNP}(t) = e(t)$. Computation shows that the residual standard error in $\text{GNP}(t)$ after the optimal stable policy had been applied would be 1.2 times as large as the standard error of $e(t)$. This is a noticeable, but not overwhelming, loss in ability to control. The loss appears even smaller when one realizes that the results of the exogeneity test in the AER paper imply that *none* of the potential for controlling GNP by variations in MB has been used in the postwar period, with MB variance having had a purely destabilizing effect. The assumed serial correlation pattern for $u(t)$ implies that the reduction in standard error of GNP available from optimal control without stability restrictions is 77 percent, whereas with the stability restriction the reduction available is 72 percent.[10] If the lag distribution were known exactly, there would be little ground for arguing that instrument instability renders policy powerless in this instance.

Of course the lag distribution is not known exactly. The problem of instrument instability, as an analytical phenomenon, can only get worse in the presence of persistent uncertainty about the true shape of the lag distribution. Using the standard distribution theory for least-squares regression estimates, the probability

[8] For example, with $T = 6$, $c = 0.7$, the terminal condition $x(T) = 0$ yields the best stationary solution almost exactly, though with $c = 0.9$, $T = 6$ is not large enough. With $c = 0.9$, $T = 18$ will provide as much accuracy as $T = 6$ with $c = 0.7$.

[9] The GNP on $M1$ lag distribution from my article used by Cagan and Schwartz is, despite my assertions to the contrary in the text of the article, fairly strongly biased by seasonal factors. This bias does not affect the conclusions of the article concerning the exogeneity test, but it does make the estimated shape of the distribution quite unreliable.

[10] These reductions are in comparison with a policy of exponential growth of money supply at its historical trend rate.

distribution of the size of the roots of the Z-transform of an estimated lag distribution is intractable, as far as I know. Furthermore, small errors of specification may, for a long lag distribution, have very large effects on the accuracy of the estimated roots of the Z-transform.

The foregoing remarks raise some open questions. Instrument instability and uncertainty may yet be shown to interact in a way making good policy extremely conservative. Instrument instability in itself, however, even in the spectacularly explosive forms found by Cagan and Schwartz, is not necessarily a major barrier to success for an active policy.

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