TABLE 2
Cochrane-Orcutt Iterative Technique

Iterations	Rho
1	.27792
2	.27367
3	.27119
4	.26966
5	.26869

Final value of rho = .2686934
Number of iterations = 5
Standard error of rho = .1730005
t statistic for rho = 1.5531369

Right-Hand Variable		Estimated Coefficient	Standard Error	*t* Statistic
Constant		−26.4892	6.95029	−3.81123
WAGEPRIV	(−1)	.121325	.176944	0.685669
GOVEMP	(−1)	.165760E−01	.625947E−02	2.64814
OLDPOP	(−1)	−.330811E−01	.647257E−02	−5.11097
VETPOP	(−1)	.344020E−03	.394336E−03	0.872404
FARMPOP	(−1)	.727013E−01	.240896E−01	3.01796
TREND		.139556E−01	.356273E−02	3.91712

Number of observations	= 31
R-squared	= .989352
R-bar-squared (adjusted for degrees of freedom)	= .986690
Durbin-Watson (O gaps)	= 1.667518
Sum of squared residuals	= .271629E−02
Standard error of regression	= .106386E−01
Sum of residuals	= .315481E−04
Mean of dependent variable	= .900408
F statistic (6, 24)	= 371.664
R-squared in terms of changes	= .5760

NOTE: Regressions were done on the GMU Cyber 2.0 using Soritec, version 1.05G (Copyright 1983, the Sorites Group, Inc.).

Testing the Right Hypothesis with the Right Specification: A Reply to Tullock

In our article (Berry and Lowery, 1984), we considered the fact that the costs of goods and services have increased more rapidly in the public sector than in the private sector in the postwar era. We tested—using time series analysis—two major explanations that have been offered for this phenomenon. Beck (1976) argued that forces associated with "Baumol's disease" account for the differential rates of growth in costs;

in this view, the differential rates can be attributed to the greater labor-intensiveness (and thus, lower productivity) of the public sector as compared with the private sector. In contrast, Buchanan and Tullock (1977) contended that the voting power of bureaucrats is responsible for the differential rates of growth in costs; in this model, the share of the electorate employed by government should be the principal determinant of relative costs in the two sectors. Our empirical results are much more supportive of the Beck/Baumol hypothesis than the Buchanan/Tullock model. Tullock makes several main points in his comment.

Tullock's first argument is that Berry and Lowery's (1984) results are consistent with Tullock's (1974) "dynamic hypothesis."

Our first reaction to this claim is that it is simply irrelevant. Our article tested the competing explanations of differential cost growth offered by Buchanan and Tullock (1977) and Beck (1976). The "dynamic hypothesis" in Tullock's 1974 paper does not even directly address the question of differential cost growth. On second reading, though, Tullock's argument is not irrelevant. That is, to suggest that the "dynamic hypothesis" is consistent with our results, Tullock is forced to abandon the model he and Buchanan offered in the 1977 article, since it and the "dynamic hypothesis" are inconsistent if not contradictory. In Tullock's (1986) own words, he no longer "stand[s] firmly behind the [1977] article" (p. 216). Given the lack of empirical support found for the Buchanan/Tullock model in our paper, we think Tullock's rejection of the 1977 article is a sound course.

Beyond this, we challenge the assertion that our data are consistent with the "dynamic hypothesis." Tullock (1986) writes:

> This hypothesis postulates a dynamic process in which the growth of bureaucracy leads to the bureaucrats using their votes first for a gradual increase in the size of bureaucracy, and then, after they have reached adequate power, for a gradual increase in their wages. (P. 216)

Despite Tullock's use of the term "leads to," it is nearly impossible to specify exactly what causal relationship he refers to. In our reading of this statement, the size of the bureaucracy is both the dependent and independent variable. Our best guess is that Tullock is trying to argue that the *voting power* of bureaucrats is positively related to the number of government employees when bureaucrats' voting power is low, but positively related to public sector wages when voting power is high. If this is Tullock's hypothesis, then it is certainly not tested by our results, nor any others that we can think of. To conduct such a test, one would need a measure of the "voting power of bureaucrats" that is distinct from the dependent variable: the number of government employees. Further, the hypothesis is untestable unless a clear threshold for "adequate" power is established. The mere suggestion that over some periods the number of bureaucrats will increase while over others their wages will go up is at best a *description* of a time trend, not an *explanation* of government growth. In sum, the suggestion that our data fit the "dynamic hypothesis" is without foundation.

Tullock's second argument is that our results are inconsistent with the Baumol/Beck hypothesis.

Baumol's (1967) paper speaks of an economy with two sectors, one labor-intensive and characterized by constant productivity, the other less labor-intensive (thus more capital-intensive) and experiencing regular increases in productivity. Because of the productivity improvements, wages in the capital-intensive sector rise. Building a model in which wages in the labor-intensive sector must keep pace with wages in the other sector, Baumol (1967) derived his proposition 1: "The cost per unit of output of [the labor-intensive] sector . . . will rise without limit while the unit cost of the [capital-intensive] sector will remain constant" (p. 418). Thus Baumol's *principal hypothesis* relates to differential rates of growth in the *costs* of output in the two sectors. Beck (1976) applied this conception assuming that government has the characteristics of Baumol's labor-intensive sector and the private sector behaves as Baumol's capital-intensive sector. Hence, to test the Beck/Baumol hypothesis, we specified equations (4) and (5) in our article: the ratio of costs in the public sector to costs in the private sector is assumed to be determined by the average public sector wage; the level of public sector wages is, in turn, modeled as a function of private sector wages in the previous year. Our equations accurately reflect the Beck/Baumol model.

We simply cannot understand how Tullock interprets our findings as inconsistent with the Baumol/Beck model. For instance, he suggests that "there is no reason why [government] wages should rise any faster than they rise in other sectors of the economy" (p. 216). We agree. Indeed, the major hypothesis of the Beck/Baumol model is that public sector wages will rise *with* (and not faster than) private sector wages. And this is what our results showed; public sector wage increases have approximated those in the private sector over the postwar era (see the near-unity coefficients for WAGEMAN [0.88] and WAGEPRIV [1.13] in columns 4 and 5 of Table 3). Thus, not only do our results support the Baumol/Beck model, they refute Tullock's (1986) unsupported claim that "what actually happened . . . is that the wages in the public sector rose markedly more than the wages of the private sector employees" (p. 216). (In fact, the Reagan administration's own Office of Personnel Management has concluded that, on average, public sector wages were 20 percent *less* than wages for comparable work in the private sector in 1983, in sharp contrast to the situation in the 1960s when the strictures of the Pay Comparability Act were enforced [Rubin, 1985: 168].)

Tullock's third argument is that our test of the two explanations was faulty because several of the independent variables in our models are highly correlated with time, and in such a situation time should be included as an independent variable in regression equations.

We do not challenge Tullock's claim that several of our independent variables are highly correlated with time. But adding the variable "time" to our equations would *misspecify* the hypotheses being tested. The variable "time" has no theoretical meaning whatsoever. *Indeed, Tullock makes no attempt to provide it with a theoretical meaning;* he merely asserts that the covariance of independent variables with time is sufficient grounds for adding "time" to the set of regressors. But when time is added to an otherwise well-specified model, how is one to interpret the coefficient for time? Since it has no theoretical meaning, its estimated coefficient is uninterpretable. Furthermore, how would we interpret the coefficients of other variables? Should they be interpreted as representing the effect of independent variables on the dependent variable "when time is held constant?" Given that time has no theoretical context, such an interpretation would be meaningless.

The best way to analyze the effects of adding "time" to our models is to consider what we know about the effects of specification error on regression coefficient estimates. The major implication of adding a theoretically irrelevant variable to a model is an increase in the standard errors for the remaining theoretically relevant variables. Furthermore, the most dangerous time to add an irrelevant variable to a model is precisely when Tullock claims we should do so—when it is highly correlated with the other independent variables in the model. This is because the resulting multicollinearity makes theoretically relevant variables compete with the irrelevant variable for explanatory power, and thus the coefficient estimates for the relevant variables from any one sample are quite likely to be far off target; the degree to which standard errors for regression coefficients are inflated as a result of specification error is positively related to the size of the correlations between the irrelevant and relevant variables (Berry and Feldman, 1985:19–20).

The only situation in which it would be appropriate to include "time" as a variable in a model like ours is when some theoretically relevant variable in the model—which can be assumed to be highly correlated with time—could not be measured directly for one reason or another. Then time might be included among the independent variables as a surrogate for the unmeasured variable. We do not believe this is the case in our analysis; we have measured and included all variables central to the theories being tested. In essence, Tullock is asking us to intentionally misspecify our model, thereby purposely reducing the quality of our coefficient estimates. We must decline the request.

Note that we do not take lightly the high correlations between "time" and some of our independent variables; they have the potential to seriously complicate the analysis. But the most troubling aspect of the high correlations with time is the possibility that they might indicate high correlations of the independent variables with each other, and thus severe multicollinearity. Fortunately, the multicollinearity we face is not sufficiently strong to pose serious problems; high *t* ratios in the estimates for each of our multivariate models, despite our relatively small sample, is a clear indication that multicollinearity is not a major impediment.

In conclusion, we stand behind our original findings. Indeed, we are perplexed by the logic of Tullock's comment. In effect, he begins by criticizing us for testing the "wrong" hypothesis: the one he introduced in his 1977 article (which he now admits he no longer "stands firmly behind") instead of one (the "dynamic hypothesis") he presented in his 1974 article—this when the 1974 hypothesis is not even precise enough to be testable. Then he concludes with a suggestion that we misspecify the hypotheses we *are* testing by adding a theoretically irrelevant variable (time) to our regression models. Tullock's criticism of our conceptualization is poorly conceived, and his challenge to our method is incorrect.

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