System Average Rates of U.S. Investor-Owned Electric Utilities: A Statistical Benchmark Study

by

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I. INTRODUCTION

Due largely to the fact that they are regulated by public utility commissions, rates charged by electric utilities are often the focus of intense public scrutiny. Controversies in rate hearings surrounding differences in average rates charged residential, commercial and industrial customers have a long history, but in recent years increasing attention has focussed on "bottom line" differences across utilities in their overall system average rates (SARs, revenue per Kwh). Proposals to restructure electric utilities by, for example, deregulating the generation function and placing the remaining transmission and distribution functions under some form of performance-based regulation, or imposing some form of price controls, have heightened interest in understanding what factors contribute to variations across utilities in their SARs. State utilities commissions are therefore examining more closely differences in SARs charged by the various within-state utilities under their jurisdiction, as well as differences in comparison to out-of-state utilities.

The research reported in this paper emerged from issues brought before the California Public Utilities Commission (CPUC). California SARs are among the highest in the US. In 1984, for example, the real (inflation-adjusted) SAR for San Diego Gas & Electric (SDG&E), Southern California Edison (SCE), and Pacific Gas and Electric (PG&E) ranked 3, 12 and 24 in comparison with 96 other large investor-owned US electric utilities; a decade later, in 1993, the respective rankings were 17, 8 and 9. Why are SARs in California so high? Why are they so much higher than the SARs of, say, Puget Sound Power and Light, Montana Power Company, Appalachian Power Company, and Idaho Power Company? Do the high California rates reflect factors largely beyond the control of utility management, such as the unavailability of low-cost

generation sources like hydro power and coal; the high local costs of doing business such as franchise fees, state and local taxes, and state wage rates; greater regulation-imposed costs such as required conservation and favored purchasing mandates; or do the high California rates reflect inefficient management and waste? Understanding why it is that California's SARs are so high is an essential input in evaluating the likely consequences of alternative deregulation and price control policy initiatives.

In this paper we report results of a statistical benchmark study comparing the annual SARs of 99 investor-owned US utilities over the ten-year time period, 1984-93.[1] Our results are based on a statistical model relating SARs to regional, economic, and regulatory factors largely beyond the control of utility management, as well as to the effects of management. Following a tradition originally used in the agricultural economics literature that focused on estimating production function parameters free of management bias, we incorporate the effects of management decisions by specifying an error components model. Using a variety of estimation procedures and data from 99 utilities for up to ten years each, we obtain a surprisingly robust set of results. Specifically, we find that the SARs of electric utilities are significantly affected by various regional, economic and regulatory factors. Controlling for these factors, there is no statistically significant difference between the SARs charged by the three California utilities and the national average. Thus, inferences concerning management efficiency made on the basis of using *unadjusted* SARs can provide very misleading implications.

The outline of this paper is as follows. In Section II we consider measurement and data issues. In Section III we develop our econometric model, and devote particular attention to the error component stochastic specification. Since our data set consists of an unbalanced panel (not all utilities have all data available in each of the ten years), we also consider

computational issues and alternative estimation procedures. In Section IV we present econometric results, and in Section V we discuss implications of the empirical findings. In Section VI we summarize the analysis and offer some conclusions.

II. MEASUREMENT ISSUES AND DATA SOURCES

We require a sample of investor-owned utilities large enough to yield a statistically reliable benchmark, but with the provision that the utilities be reasonably comparable to those in California. Thus we selected investor-owned utilities that met four criteria: (i) retail sales greater than zero; (ii) residential sales at least 5% of total (retail and wholesale) sales; (iii) generation plant at least 5% of total plant; and (iv) data reported for all variables relevant to our analysis. These criteria generated suitable 1984-93 annual data for a total of 99 investor-owned utilities.[2] Because utilities differ in the completeness of their reported data, a company could be included in some years and excluded in others. Our data set therefore consists of an unbalanced panel, with utilities appearing in as few as one year and as many as ten; the "average" utility appears in the sample 8.87 years.

Our bottom line benchmark criterion is the system average rate (SAR). SAR is traditionally defined as total revenues from ultimate consumers divided by total sales in Kwh to ultimate consumers. To account for differential inflation rates by region, we deflate this SAR in nominal dollar quantities to 1993 constant dollars using the US Bureau of Labor Statistics regional all-items Consumer Price Index (CPI).[3]

Substantial variation in SARs occurs in our sample. In 1993, for example, the SAR ranged from 3.76 cents (Idaho Power) to 14.71 cents (Long Island Lighting Co.). In general, there is more SAR variation between utilities than there is within utilities over time; the ratio of the between-

to within-utility sample variance is 5.63, implying that about 85% of the sample variation in SAR is due to differences across utilities.

One major problem in comparing SAR across utilities is that the utilities differ in taxes paid and in the regulatory costs directly imposed on them. As a first step in moving toward more meaningful comparisons across utilities over time, we net out the following factors from the total revenues of each utility on a dollar-for-dollar basis: (i) franchisee fees (payments to cities and counties for the right to use or occupy public streets, roads and ways); (ii) state and local tax payments[4]; (iii) demand-side management expenditures; (iv) the excess burden of qualifying facility (QF) power purchases, defined as the amount by which QF payments exceed the cost that would be incurred to obtain the same amount of QF power through own generation by the utility and power purchases from other generating sources; and (v) California regulatory adjustments due principally to the Energy Cost Adjustment Clause mechanism, which adjusts annually for differences between actual and forecast fuel and purchased power expenses. We then define a net system average rate (NSAR) as total net revenues from ultimate customers with the above "taxes" removed, divided by Kwh to ultimate customers.

It is worth noting that use of NSAR rather than SAR does not change much the rankings of California utilities relative to others in the US. For example, in 1984, based on NSAR, SDG&E ranked second highest in the US, SCE was 12th, and PG&E was 22nd; by 1993, the respective rankings were 10, 28 and 14. However, using NSAR rather than SAR results in reducing the total sample variance by about 25%, with most of the reduction being between rather than within utilities; the ratio of the between to within variance of NSAR is 3.59, implying that about 78% of the sample variance is across utilities. In 1993, Long Island Lighting Company had the highest NSAR at 12.16 cents, while Idaho Power had the lowest at 3.25 cents.

The unadjusted NSAR, however, still embodies the effects of numerous differences across utilities, many of which are outside the control of utility management. We posit that five sets of factors affect NSAR, and now briefly discuss each in turn.

First, local costs of doing business vary across utilities and over time. Although the NSAR nets out inter-utility variations in franchise fees and state and local taxes, it does not control for varying costs of labor. To allow for this factor, we have collected data on state wage rates. We find that on average, California wage rates are about 13% higher than the national average.

Second, power production characteristics should affect costs and therefore NSAR. As seen in Table 1, California utilities have negligible coal and oil generation capacity, whereas for the rest of the nation, coal and oil together account for 60% of capacity.[5] Gas capacity averages about 14% nationally, but is over 50% for the three Califoria utilities. Nuclear power is also used more intensively in California than in the rest of the nation. Next we posit that the average age of a utility system affects NSAR. Because of inflation, gross plant and depreciation expenses are higher for newer systems, thereby increasing a utility's ratebase and raising its NSAR, other things equal. We measure average system age by dividing total accumulated depreciation by total depreciation expense. The three California utilities have an average age varying from 7.18 to 7.72, more than two years less than the average age of 9.86 years for the rest of our sample. Further, we expect costs of fuel and purchased power to affect NSAR. We compute the average cost of fuel as the sum of fossil, nuclear and other fuel costs divided by net generation. The average fuel price is 3.38 ¢/Kwh for SDG&E, 2.64 for SCE and 2.18 for PG&E, while the national average is 2.09. We also calculate the average cost of utility purchased power as total expenditures for utility

generated purchased power divided by megawatt hours purchased. SDG&E's average price of utility purchased power is 4.86 ¢/Kwh compared to a rest of nation average of 3.95, while that for SCE is 3.13 and PG&E is 2.74.

A third set of factors affecting NSARs comprises customer and service territory factors. We expect that as use per customer declines, all else equal, NSAR will increase because fixed costs will be spread over fewer sales. The weather, customer mix and housing stock in California all contribute to relatively low use per customer -- about one-half the national average. In 1984, for example, SDG&E ranked third, PG&E twelfth, and SCE fourteenth lowest in the US in Kwh per customer; in 1993 the respective rankings were third, seventh and ninth. Customer density -- the number of residential customers per square mile -- may also influence costs. Given the added expense of installing and maintaining complex distribution facilities and the higher costs of obtaining right-of-ways, we expect that other things equal, urban areas are more costly to serve. SCE and PG&E have much smaller customer density than the national average (67 and 37 vs. 195), but that for SDG&E (219) is slightly higher than the average. Customer composition, such as residential customers as a percentage of total customers, can affect NSAR, particularly when costs of distribution are large relative to transmission services.[6] The size of this share for the three California utilities is virtually the same as the national average -- 88%. A final customer and service territory factor that can reasonably be expected to affect NSAR is the system average load factor, calculated as the ratio of system average demand to the system maximum or "peak" demand, both in megawatts. Since utilities with lower load factors must allocate the cost of meeting peak demand across fewer units, we expect that, ceteris paribus, system average load factor and NSAR are negatively related. Load factors for the SDG&E and PG&E are close to the national average (58% vs. 62%), but for SCE it is lower at 49%.

A fourth set of factors affecting NSAR reflects the cost of complying with local, state and federal regulations concerning pollution abatement standards. To estimate the effects of air pollution regulations on NSAR, we attempted to obtain data on the amount of nitrogen oxide and sulfur dioxide generated by various types of generation units, in pounds per Mwh; our expectation was that such air pollution emissions would be inversely related to NSAR. Although we were successful in obtaining suitable measures for sulfur dioxide, we were unable to obtain reliable data for nitrogen oxide. Thus, we employ sulfur dioxide emissions as a proxy variable for the total emissions generated by the various types of generation units, and the percentage of capacity with sulfur control as a measure of the added cost of pollution controls. While sulfur controls affect 11% of the rest of the nation's generation capacity, for the three California utilities this share is zero. Not surprisingly, therefore, sulfur emissions are much smaller -- .001 pounds/Kwh for SDG&E, .003 for SCE, .0002 for PG&E -- than the .016 for the rest of the nation.

The fifth set of factors affecting NSAR consists of other regulatory requirements, most of which reflect compliance with state utilities' commissions. The effects of qualifying facility purchase requirements, as well as demand side management expenditures, are already accounted for by use of the NSAR rather than the "gross" SAR; QF excess burdens are especially large for SCE (1.09¢/Kwh) and PG&E (0.87) relative to the national average of 0.08¢/Kwh. Two other regulatory policies are of particular interest. The CPUC requires California investor-owned electric and telephone utilities to convert a portion of their overhead distribution lines to underground facilities, thereby incurring costs to install underground facilities in already-developed areas, and to remove existing overhead lines. Some local governments, for example San Diego, require additional underground

TABLE 1

VARIABLES INCLUDED IN THE STATISTICAL BENCHMARK STUDY

Variable	Units	*Average Value, 1984-1993* SDG&E	SCE	PG&E	Other National
Gross SAR	¢/Kwh	12.87	10.85	10.22	7.95
Net SAR	¢/Kwh	11.81	9.03	8.59	7.08
Local Costs of Doing Business					
Franchise Fees	¢/Kwh	0.24	0.09	0.07	0.004
State & Local Taxes	¢/Kwh	0.59	0.49	0.52	0.73
State Wage Rates	$/Year	30,603	30,603	30,603	26,755
Customer & Service Territory					
Electricity Use Per Customer	Kwh/Cust	12,811	17,250	16,681	25,564
Customer Density	Cust/Sq Mi	219	67	37	195
System Load Factor	Percent	58	49	58	62
Share Residential Customers	Percent	89	88	87	88
Power Production Factors					
Capacity Share - Coal	Percent	0	12	0	50
Capacity Share - Oil	Percent	7	0	0	10
Capacity Share - Gas	Percent	59	58	51	14
Capacity Share - Hydro	Percent	0	7	26	6
Capacity Share - Nuclear	Percent	20	17	13	11
Capacity Share - Other	Percent	14	6	11	9
Average Age of System	Years	7.18	7.72	7.61	9.86
Average Fuel Price	¢/Kwh	3.38	2.64	2.18	2.09
Average Price of Utility Purchased Power	¢/Kwh	4.86	3.13	2.74	3.95
Pollution Control Factors					
Capacity Share with Sulfur Controls	Percent	0	0	0	11
Sulfur Emissions	Pounds/Kwh	.001	.003	.0002	.016
Other Regulatory Requirements					
Realized Rate of Return	Percent	11.03	10.46	11.10	10.19
QF Purchase Requirements	¢/Kwh	0.10	1.09	0.87	0.08
Demand Side Management Expenditures	¢/Kwh	0.13	0.15	0.16	0.06
Percent Underground Distribution Lines	Percent	50	54	14	22

distribution line conversion. We measure the effects of such regulatory policies as the percent of total cable miles that is underground. While this percent is 22% for the rest of the nation, for SDG&E it is 50% and for SCE it is 54%. Finally, in rate case proceedings regulated utilities are assigned an allowed rate of return. Ceteris paribus, it is reasonable to expect that the greater the allowed rate of return, the higher the NSAR. Data on allowed rate of return were not available, but data on average realized rates of return for the three California utilities suggest allowed rates of return slightly above the national average -- 11.03% for SDG&E, 10.46% for SCE, 11.10% for PG&E, and 10.19% for the rest of the nation.

The data sources used to construct the variables described above include the Utility Data Institute's FERC Form 1 Database (containing annual financial and other information filed at the FERC by major utilities), the Edison Electric Institute's Electric Fuel Statistics (monthly data on the costs and quality of fuels used in electric plants), the Edison Electric Institute's Uniform Statistical Reports (voluntary filings by utilities containing information describing company operations), the US Bureau of Labor Statistics (Consumer Price Indexes), and the US Bureau of Economic Analysis (wages and employment by state).

In Table 1 we list the variables to be used in our econometric analysis, and also provide sample means of these variables for the three California utilities and for the remainder of our national sample.

III. SPECIFICATION OF THE ECONOMETRIC MODEL

We now proceed with a discussion of the specification of our econometric model. We first discuss issues of functional form, next we consider how to deal with those variables which might not be entirely exogenous to current utility management, and then we elaborate on how management effects can be incorporated using an error components stochastic specification that reflects

a hybrid of fixed and random effect considerations. We consider alternative procedures for estimating parameters in an error components framework, given the fact that our panel data set is unbalanced, and that the maximum number of times a utility is observed -- ten -- is rather small.

The dependent variable in our econometric model is the logarithm of the NSAR -- hereafter denoted LNSAR (later we report results on use of NSAR rather than its logarithmic transformation). We expect that relationships involving some variables, for example, real state wage rates, use per customer, customer density, real average fuel costs, real utility wholesale purchased power, and system age are most likely to affect LNSAR in a proportional manner, and thus we include as explanatory variables in our model the logarithmic transformations of these variables, which we denote as LRWAGE, LUPC, LCD, LRFUEL, LRWPP and LAGE, respectively. Other explanatory variables are already in percent form, and thus we include them without further transformation; these include generation capacity shares for coal, oil, gas, hydro, nuclear and other (largely, geothermal), named CAPC, CAPO, CAPG, CAPH, CAPN and CAPOTH), respectively.[7] A realized rate of return variable (ROR) is also included, as is a time trend.

While many of the explanatory variables are plausibly outside the immediate control of utility management (e.g., state wage rates, generation capacity, system age), several other variables might arguably reflect current management influences. First, use per customer might reflect utility-specific pricing and other conservation policies; to allow for this possibility, we will report regression results with and without the LUPC variable "instrumented", using the logarithm of weather cooling degree days as an instrumental variable. A second possible endogenous variable is LRFUEL, the utility real average fuel cost; here an appropriate instrumental variable is the state-specific average fuel cost. The final variable that could reflect

current management influence is the rate of return measure. In principle, we would prefer to employ an ex ante rate of return, since in rate hearings NSAR is set so as to produce an expected rate of return. However, the ex ante rate of return is unobservable. We explore two alternative ways of handling this situation.

First, one feasible procedure involves formulating the relationship between the *ex post* and *ex ante* rates of return (ROR) as follows. Let

$$ROR_{ex\ post} = ROR_{ex\ ante} + \delta \cdot shock + \upsilon \qquad (1)$$

where the *ex post* ROR is the sum of the allowed *ex ante* ROR plus factors that "shock" this anticipated ROR, plus a random effect υ. We postulate that the shock is a function of the deviation of use per customer from trend, measured as the residual from a utility-specific regression equation of LUPC on a time counter. We then solve the above equation for the *ex ante* ROR in terms of the *ex post* ROR and the demand shock, i.e.,

$$ROR_{ex\ ante} = ROR_{ex\ post} - \delta \cdot shock - \upsilon. \qquad (2)$$

We implement this formulation by including as regressors in the LNSAR equation both the $ROR_{ex\ post}$ and the "shock" (deviation from trend) variable, which we denote as DLUPC. Note that while we expect the coefficient on $ROR_{ex\ post}$ to be positive, perhaps counterintuitively, this formulation implies that the sign on the DLUPC variable should be negative.

A second possible procedure is to employ an instrumental variable, such as the Moody bond rating for each of the utilities. However, it is not clear how appropriate the Moody bond rating would be, for one could argue that Moody's ratings reflect their judgment on utility management efficiency, and thus the Moody rating would be a jointly determined rather than exogenous variable. Nonetheless, in our empirical results, we will report findings results on the use of Moody bond ratings as instruments.

We now turn to issues of stochastic specification for our econometric model, which we simply write as

$$y_{it} = \alpha + \beta' x_{it} + u_{it} \tag{3}$$

where y is the dependent variable, there are K regressors in x in addition to the constant term, $i = 1,\ldots,N_t$, and $t = 1,\ldots,T_i$ (in our unbalanced panel, not all utilities are observed in all years, thus N_t varies by year and T_i varies over utilities), and α and the K β's are unknown parameters to be estimated.

With panel data, a common specification is that the mean zero random disturbance term u_{it} is the sum of a mean zero utility-specific component ν_i that is constant over time, and an independent mean zero component ϵ_{it}, with the strictly positive variances of these two components being σ_ν^2 and σ_ϵ^2.[8]

For our purposes, the interpretation of ν_i is very important.[9] One can re-write (3) by substituting in the component error and gathering terms. This gives us

$$y_{it} = (\alpha + \nu_i) + \beta' x_{it} + \epsilon_{it} \tag{4}$$

where $(\alpha + \nu_i)$ can be envisaged as a random intercept in the LNSAR equation that varies across utilities. Since ν_i has mean zero, it reflects the difference in LNSAR for utility i from the national average, ceteris paribus. If ν_i is positive (negative), the LNSAR of utility i is larger (smaller) than the national average, holding fixed the other factors (the x's) affecting LNSAR. We postulate that that ν_i reflects the effects of management decisions of utility i on its LNSAR; we recognize, however, that the effects of other omitted variables might also be captured by ν_i.

If the ν_i's of the three California utilities -- SDG&E, PG&E and SCE -- were insignificantly different from zero, ceteris paribus, then in this framework we would conclude that once one controls for the various regional, economic and regulatory factors, the management effects of the three

California utilities are no different from the national average. On the other hand, if the ν_i's were found to be positive (negative) and significantly different from zero, then one might infer that the management impacts of the three California utilities result in a larger (smaller) NSAR than the national average, having controlled for the various regional, economic and regulatory factors.

With the above error components stochastic specification, the variance-covariance matrix of disturbances is homoskedastic but non-diagonal (see, for example, Greene [1993, ch. 16]). In particular, $E(u_{it}^2) = \sigma_\nu^2 + \sigma_\epsilon^2$ for each i, but $E(u_{it}u_{is}) = \sigma_\nu^2$ when $s \neq t$. Under these assumptions, estimation of parameters by ordinary least squares (OLS) is consistent but not efficient, and traditional OLS-based estimates of the standard errors are biased.

Two methods have commonly been used to obtain consistent and efficient estimates of α and β, and reliable estimates of their variances in this error components specification; these are usually referred to as fixed and random effects estimators. The random effects estimator employs a transformation based on consistent (often, OLS) estimates of σ_ν^2 and σ_ϵ^2, denoted s_ν^2 and s_ϵ^2, in which the x's and y are first transformed so that $x_{ijt}^* = x_{ijt} - \omega_i \bar{x}_{ij}$ for each of the regressors, and $y_{it}^* = y_{it} - \omega_i \bar{y}_i$, where

$$\omega_i = 1 - \left[\frac{s_\epsilon^2}{s_\epsilon^2 + T_i s_\nu^2} \right]^{1/2} ; \qquad (5)$$

OLS is then performed on the transformed y^* and x^*, which now satisfy the Gauss-Markov conditions for optimality of OLS.[10] In cases where the panel is balanced (when $N_t = N$ and $T_i = T$), and under the assumption of normality of disturbances, iteration of this feasible generalized least squares procedure has been shown to yield estimates that are numerically equivalent to maximum

likelihood estimation.[11] Although our panel is unbalanced, iteration is feasible, and thus we iterate the transformation process until changes from one iteration to the next are insignificant.

An alternative estimation procedure, known as fixed effects, replaces the common intercept term $\alpha + \nu_i$ (whose expected value is α) with utility-specific constant terms α_i for each i, and then employs OLS estimation. There are four important facts to note about this fixed effects estimator.

First, it is well-known that employing OLS with utility-specific intercept terms yields estimates of the β's that are numerically equivalent to subtracting the utility-specific means of each y and x variable from their observed values, and then doing OLS on this deviation-from-utility mean-transformed data.[12]

Second, use of such a deviation from mean estimation procedure results in relying completely on the "within-utility" sample variance of the x's and y's to estimate β (but not the α_i), and therefore results in β estimates that entirely ignore the between-utility variance. In our context, this complete reliance on within-utility variance could be somewhat detrimental, since as noted earlier, about 78% of our sample variance in NSAR is across utilities, and only 22% is within utilities over time. By contrast, the random effects estimator employs both between and within variance in estimating the β's.[13] Note also that use of the fixed effects estimator requires the estimation of an additional N-1 utility-specific parameters (almost 100 here) when compared to the random effects specification.

Third, under traditional assumptions, the fixed and random effects estimators are asymptotically equivalent. To understand this equivalence, note from Eq. (5) that the fixed effects estimator is equivalent to setting $\omega_i = 1$. Since s_ν^2 and s_ϵ^2 are both strictly positive, ω_i approaches unity with increasing T_i, because the denominator in square brackets in Eq. (5) converges

to infinity. In our sample, while N_t is almost 100, T_i is never larger than ten and sometimes is as small as one. Evidence from Monte Carlo studies, presented in Baltagi [1981], suggests that in finite samples (in his case, $T_i = 10$) the fixed effects estimators have much greater variance. Thus we believe that caution should be exercised in using large sample assumptions for T_i to rationalize results based on the fixed effects estimator.[14]

Fourth, in the fixed effects specification, the utility-specific intercept terms can be interpreted as $\alpha_i = \alpha + \nu_i$. Since the sum of all residuals from econometric estimation is zero, the utility-specific ν_i thus essentially reflect the mean of that utility's residuals over its T_i observations. If the mean of these T_i residuals is large and positive (negative), then in this framework the impacts of management on LNSAR results in higher (lower) rates than the average; but if the mean of these T_i residuals is small and statistically insignificantly different from zero, then there is no evidence supporting above- or below-average management impacts.

The above discussion on fixed and random effect estimators is based on the assumption that none of the x's is correlated with the ϵ's. If, for example, simultaneous equations considerations suggest that use per customer or average fuel price might be endogenous, then to obtain consistent estimates, one would want to modify both the fixed and random effect estimators, employing, for example, an instrumental variable approach.[15]

These econometric considerations suggest to us a hybrid approach in estimating management effects on NSAR. Since inclusion of utility-specific intercepts consumes valuable degrees of freedom, we will employ a compromise of the fixed and random effects approaches, using a fixed effects (utility-specific intercepts) specification for observations on the three California utilities, and a parsimonious in parameters random effects specification for observations on the remaining 96 non-California utilities in our sample.[16]

This hybrid specification allows us to test for the existence of a "California effect", yet does not overly parameterize the model. To test whether there is a "California effect" (in which, controlling for other factors, the California utilities are examined for whether their NSARs differ from the national average), we will test the joint null hypothesis that the three intercept terms of SDG&E, PG&E and SCE are simultaneously equal to zero. We will, however, also undertake a number of specification and diagnostic tests, and assess whether estimates and inference concerning the existence of any "California effect" are robust to alternative estimation methods.[17]

IV. ECONOMETRIC RESULTS

We begin with results from the most simple specification where we estimate by OLS a model in which in addition to the explanatory variables discussed above, the three California utilities have separate dummy variables; results are given in the first column of Table 2. As is seen there, the parameter estimates on the three California utilities are negative for SDG&E and SCE, positive for PG&E, but the small t-values imply that each is insignificantly different from zero. A joint F-test that the three California utility coefficients are simultaneously equal to zero is not rejected -- the test statistic is 1.12, while the 0.05 critical value is 2.60.

Several other results from this most basic model are also worth noting. First, the estimate of the LUPC coefficient is -0.39 and significant, implying that the elasticity of NSAR with respect to use per customer is about -0.4. Second, while the impact of ROR on LNSAR is positive and significant as expected, the coefficient on DLUPC (deviation from trend) is negative, as expected, although not very reliably so. Third, coefficient estimates on the generation capacity variables (interpreted as relative to coal) are signfiicantly positive for oil, gas and nuclear, and negative for hydro.

TABLE 2

PARAMETER ESTIMATES BASED ON ALTERNATIVE SPECIFICATIONS
(Absolute value of ratio of parameter estimate to standard error in parentheses)

VARIABLE	OLS	RANDOM EFFECTS	FIXED EFFECTS	2SLS	IV-RANDOM EFFECTS	BETWEEN ESTIMATOR
CONSTANT	7.108	7.735	13.806	6.955	7.445	7.331
	(36.64)	(17.87)	(13.34)	(14.34)	(19.63)	(13.04)
LUPC	-0.390	-0.513	-1.025	-0.375	-0.482	-0.372
	(21.69)	(12.42)	(10.41)	(7.72)	(13.34)	(7.38)
ROR	0.652	0.455	0.394	0.651	0.463	0.800
	(6.62)	(7.19)	(6.34)	(6.61)	(7.17)	(1.61)
DLUPC	-0.224	-0.048	0.452	-0.242	-0.082	-1.817
	(1.52)	(0.54)	(3.66)	(1.54)	(0.91)	(0.45)
CAPOTH	-0.020	0.029	0.042	-0.010	0.027	0.020
	(0.44)	(0.46)	(0.61)	(0.19)	(0.44)	(0.14)
CAPO	0.164	0.127	0.102	0.172	0.135	0.105
	(5.78)	(4.42)	(3.37)	(4.76)	(4.75)	(1.02)
CAPG	0.101	0.095	0.075	0.099	0.101	0.065
	(5.40)	(3.85)	(2.73)	(5.15)	(4.17)	(1.18)
CAPH	-0.213	-0.373	-0.694	-0.212	-0.363	-0.094
	(4.30)	(3.89)	(3.75)	(4.27)	(4.26)	(0.55)
CAPN	0.465	0.339	0.229	0.469	0.355	0.530
	(13.06)	(6.90)	(4.10)	(12.54)	(7.45)	(4.85)
LAGE	-0.252	-0.102	-0.073	-0.252	-0.109	-0.312
	(9.92)	(4.90)	(3.55)	(9.88)	(5.21)	(3.34)
LCD	0.024	0.018	-0.224	0.024	0.021	0.020
	(6.83)	(2.15)	(5.45)	(6.46)	(2.99)	(2.01)
LRFUEL	0.180	0.096	0.095	0.182	0.100	0.247
	(11.70)	(8.22)	(8.06)	(11.08)	(8.48)	(3.89)
LRWPP	0.034	0.019	0.017	0.035	0.020	0.049
	(5.85)	(4.51)	(4.13)	(5.86)	(4.56)	(2.08)
TREND	-0.013	-0.022	-0.013	-0.013	-0.022	-0.007
	(6.91)	(15.28)	(6.87)	(6.90)	(15.27)	(0.57)
SDG&E	-0.051	-0.027	-0.021	-0.043	-0.019	-0.079
	(1.26)	(0.74)	(0.27)	(0.89)	(0.58)	(0.68)

PG&E	0.038 (0.92)	0.056 (1.53)	-0.163 (2.99)	0.045 (0.98)	0.064 (1.88)	0.011 (0.09)
SCE	-0.030 (0.76)	-0.007 (0.24)	-0.117 (3.02)	-0.024 (0.54)	-0.002 (0.08)	-0.048 (0.42)
OTHER DUMMIES	None	None	Yes	None	Yes	No
R^2	0.836	0.809	0.962	0.822	0.771	0.861

Fourth, the remaining variables have the expected signs -- positive for LCD, LRFUEL and LRWPP, and negative for LAGE and TREND.

With this as background, we now turn to estimates based on the hybrid random effects specification for the 96 non-California utilities, with dummy variables included for just the California utilities; results are given in the column with heading "random effects". Qualitatively, the results are quite similar to those based on OLS. In particular, again the SDG&E and SCE coefficients are negative but insignificantly different from zero, while that on PG&E is positive but insignificant. The joint F-statistic for the null hypothesis that there is no "California effect" is 1.76, while the 0.05 critical value is 2.60, again implying that when other factors are taken into account, there is no evidence suggesting that California utilities charge rates different from the national average. With respect to the other explanatory variables, the random effect LUPC elasticity estimate is slightly larger in absolute value than with OLS (-0.51 vs. -0.39), while the ROR estimate is somewhat smaller (0.46 vs. 0.65). The random effects parameter estimates on the other explanatory variables are similar to those based on OLS, although some of the insignificant parameters change in sign.

For the random effects specification, one can compute $\rho = s_\nu^2/(s_\nu^2 + s_\epsilon^2)$ -- the proportion of the total residual variance reflecting utility-specific "between" variation. Rearranging this relationship and

solving for s_{ν}^2, then substituting into Eq. (5), yields an expression relating the Fuller-Battese transformation factor ω_i to ρ and T_i. Using the iterated Fuller-Battese random effects procedure, our estimate of ρ is 0.748, with an asymptotic standard error estimate of 0.01. This implies that for an average sample utility (having 8.87 annual observations), the Fuller-Battese transformation factor ω_i in Eq. (5) is 0.83. Finally, as a check on the normality assumption, we calculated a Shapiro-Wilks test statistic for normality of the residuals; the test statistic was 0.9839, which has a p-value of 0.1561, thereby lending support to the normality assumption.

As discussed in Section III, an alternative estimation procedure is the fixed effects estimator that restricts ω_i to 1.00. Estimates of our model based on the fixed effects estimator are given in the middle column of Table 2. The fixed effect model has been specified so that sample size-weighted estimates of the utility-specific dummy variable coefficients sum to zero across all utilities, implying that each of the dummy variable parameters should be interpreted as differences from a national average. All three California utilities have negative coefficient estimates, with those of SCE & PG&E reaching statistical significance. If true, these results would suggest that holding other factors constant, California utilities have *lower* system average rates than does the hypothetical "national average utility".

If ν_i is correlated with any of the x's, then although the fixed effects estimator is consistent (and in some cases efficient), the random effects estimator is not consistent. One situation where this might occur is if ν_i is envisaged as reflecting the impact of omitted variables, and if one or more of the omitted variables are correlated with the included variables. A large-sample specification test for testing whether ν_i and the x's are uncorrelated has been developed by Hausman [1978] and elaborated on by Hausman-Taylor [1981]. In essence, it amounts to testing whether the fixed and random

effects estimators yield estimates of the β's that are statistically insignificantly different from each other. Although results from such a Hausman test suggest a statistically significant difference between the fixed and random effects estimates at better than the 5% level, thereby favoring the fixed effects estimator, the fixed effects results appear less plausible on economic grounds.

For example, the fixed effect estimated elasticity of system average rate with respect to use per customer is -1.025 -- an estimate we believe is unrealistically large given the substantial fraction of a typical utility's variable costs in its total costs. Also, rather than having the expected negative sign, the estimated coefficient on DLUPC in the fixed effects model is positive and significant; similarly, while one expects customer density to have a positive impact on system average rate, the fixed effect parameter estimate on LCD is negative and significant.

An alternative consistent estimator when ν_i is correlated with one of the regressors involves use of an instrumental variable (IV) procedure. Given the large observed difference between the fixed and random effect coefficient on LUPC, we focus particular attention on the possible correlation of ν_i with LUPC. We therefore re-estimate the OLS and random effect models with LUPC "instrumented" using the logarithm of cooling degree days. Results are given in columns 4 and 5, respectively, of Table 2. Again we obtain the robust finding that none of the California utilities has a significant positive coefficient estimate; for both 2SLS and IV-Random Effects, the SDG&E and SCE effect estimates are negative and insignificant, while that for PG&E is positive but insignificant. Moreover, the IV-Random Effects estimates on the model's explanatory variables are very similar to the random effects estimates that do not take the possible correlation of ν_i and LUPC into account.[18] The

absence of any statistically significant "California effect" is therefore quite robust.

To assess this robustness further, we have examined a number of other model specifications. For example, we estimated models in which variables such as underground distribution lines, sulfur controls, sulfur emissions, state wage rate, percent residential customers and load factor were added separately or in combination; not only were coefficients on these variables statistically insignificantly different from zero, but estimates of the three "California effect" parameters remained insignificant. We also estimated a model in which the LRFUEL variable was instrumented using the state's average fuel cost; again, the estimated "California effects" were insignificant.[19]

Another diagnostic we examined concerned choice of functional form. To compare our logarithmic specification with a linear one, it is of course inappropriate to compare their R^2 values, since the dependent variables differ. To do a meaningful "head-to-head" comparison, we compared the linear and log models by calculating an R^2 in *levels*. In this case, the model with the higher constructed R^2 is preferred. We first estimated the model with all variables in their linear form, using the iterated Fuller-Battese procedure; the R^2 from this model (based on the original data) was 0.6129. We then compared these results with those based on the random effects model of Table 2. In particular, we computed the fitted value of LNSAR, converted it to NSAR by exponentiating it, and then obtained an unbiased estimate of the fitted NSAR by multiplying by $\exp(.5s^2)$, where s^2 is the estimated residual variance. Finally, we calculated an R^2 value by calculating the ratio of the sample variance of the unbiased prediction to the sample variance of net price.[20] The resulting R^2 value was 0.7548, which is larger than the linear R^2 of 0.6129, providing strong evidence that the logarithmic model is the preferred specification for analyzing net price.[21]

Yet another check on robustness involves use of what is commonly called a "between" estimator. It is of course well-known that in balanced panels the random effects estimator is a weighted average of the fixed effect "within" estimator and a between estimator, where the between estimation involves computing sample means of all variables for each utility, and then (in our context) running a regression of each utility's mean LNSAR on the sample means of the explanatory variables.[22] This suggests the following mental exercise. Suppose one collects sample means over 1984-93 annual observations of the left- and right-hand variables for each of the 96 non-California utilities, and then runs an OLS regression. Given the resulting parameter estimates based on only the non-California utilities, use the sample means of the explanatory variables for SDG&E, SCE and PG&E to generate predicted LNSARs for these utilities, and then prediction errors by subtracting the predicted LNSAR from the three utilities' mean actual LNSAR. Then test whether these prediction errors are statistically significantly different from zero.

The above exercise is numerically equivalent to running the same between regression, but adding dummy variables for each of the three California utilities, including these utilities in the estimation, and then testing whether the dummy variable coefficients for the three utilities are statistically significantly different from zero.[23] Results from such an exercise are given in the final column of Table 2. As is seen there, again one finds that the estimated California effects for SDG&E and SCE are negative and insignificant, while that for PG&E is positive but insignificant. We conclude, therefore, that once one controls for various regional, economic and regulatory factors, there is no evidence supporting the notion that the performance of California utilities is worse than the national average benchmark.

V. IMPLICATIONS AND INTERPRETATION OF FINDINGS

The results reported in the previous section are surprisingly robust. The common theme of these results is that once one controls for various regional, economic and regulatory factors outside the direct and immediate control of utility management, system average rates charged by the California utilities are not statistically different from the national average. An important issue raised by these results, however, concerns the interpretation of why it is that the *unadjusted* rates are so much higher in California than in the rest of the country. What insights can our estimated model provide in helping us understand what accounts for California's higher unadjusted system average rates?

One way of interpreting California's relatively high rates is to proceed as follows. Suppose a California utility had values of the various regional, economic and regulatory variables equal to those of the national average. In such a hypothetical case, what would that California utility's system average rate be, and how much of the difference between its actual and hypothetical rates could be attributed to each of the various explanatory variables?

We have undertaken such a calculation, based on parameter estimates from the random effects specification, and illustrate its results for SDG&E in Table 3.[24] As is seen there, while the 1984-93 national system average rate is 8.02¢ per Kwh (in 1993 constant dollars), that for SDG&E is 4.85¢ higher at 12.87¢/Kwh. Of this 4.85¢ difference, 3.06¢ (about 63%) can be attributed to the fact that SDG&E has a use per customer equal to about half the national average, and another 0.73¢ (about 15%) reflects the fact that SDG&E's generation mix involves more gas, oil and nuclear than the national average, and less hydro and coal. Together, differential use per customer and generation mix account for about 78% (3.79 of the 4.85¢) of the discrepancy between SDG&E and the national average system electricity rate. Higher

TABLE 3

DECOMPOSING THE SDG&E AVERAGE ELECTRICITY PRICE DIFFERENCE
FROM THE NATIONAL AVERAGE

Average Electricity Price (1984-93)
(¢/Kwh in 1993 constant dollars)

SDG&E	12.87
National Average	8.02
Difference	4.85

Decomposition of the Difference

	¢/Kwh	% Difference
Lower Electricity Use per Customer	3.06	63.1%
Generation Mix	0.73	15.1
Higher Average Fuel Price	0.41	8.5
Younger Age of System	0.25	5.2
Higher Franchise Fees	0.24	4.9
Higher Customer Density	0.19	3.9
Higher Demand-Side Management Expenditures	0.07	1.4
Higher Cost of Utility Purchased Power	0.06	1.2
Time Trend	0.04	0.8
Higher Rate of Return	0.03	0.6
Expenditures for QF Power	0.00	0.0
Lower State and Local Taxes	-0.14	-2.9
Remaining Influence of the Above Factors Not Separately Measurable	0.12	2.5
SDG&E Effect (Not Statistically Significant)	-0.21	-4.3
TOTAL	4.85	100.0%

average fuel price, younger age of system, higher franchise fees, and higher customer density together account for another 1.09¢ (about 22%) of the difference, while the remaining factors essentially cancel one another out.[25]

Finally, note that the SDG&E effect -- the effect of SDG&E management on system average rate -- is slightly negative, -0.21¢/Kwh -- implying that ceteris paribus, SDG&E's prices are slightly lower than the national average. From a statistical standpoint, however, this estimated SDG&E effect is not reliably different from zero.

The above decomposition was based on parameters from the random effects specification in Table 2. Alternative calculations could be done using parameters from other models; in all cases except for that of the fixed effects model, however, the qualitative findings regarding the factors underlying SDG&E's higher average electricity prices would be largely unchanged. Since the parameter estimate on LUPC is so very large and negative with fixed effects, a decomposition based on those estimates would suggest an even larger role for use per customer. However, as noted in the previous section, given our relatively small time series in the panel data, as well as some of the implausible parameter estimates obtained from the fixed effects estimates, we tend not to treat the fixed effects results as being reliable. But even with fixed effects parameters, the SDG&E effect would not be significantly different from zero.

VI. SUMMARY AND CONCLUSIONS

Using multiple regression methods, we have undertaken a statistical "benchmark" study comparing system average electricity rates charged by three California utilities with 96 other US utilities over the 1984-93 time period. Although system average electricity rates are much higher in California than for the national average, we conclude that use of such unadjusted prices provides no meaningful information on how one evaluates the performance of utility management. Rather, we find that average electricity prices are affected to a large extent by a number of factors outside direct and immediate management control, such as local costs of doing business, the availability of low-cost generation sources (e.g., hydro and coal), customer and service territory characteristics such as customer density, use per customer, and a number of regulatory and environmental factors. Once one controls for these various factors, the remaining impact of utility management on system average rates is rather modest, and for the California utilities the impact of utility

management (relative to the national average) is insignificantly different from zero. This finding of no difference in prices, holding constant the effects of factors outside of California utilities' control, is robust, being sustained in a large number of alternative models and estimation methods.

It would of course be desirable to decompose further the reasons for differences in system average rates. One possible line of research could examine distinct cost categories such as generation, transmission and distribution, or even a further disaggregation of these functions. Although of great interest, such a study would impose very challenging difficulties to any empirical researcher. The most obvious difficulty is the problem of obtaining reliable time series of disaggregated cost data that is comparable across utilities. The accounting procedures by which utilities allocate fixed and common costs to functional activities vary even between the all-electric utilities, are considerably more complex and idiosyncratic for combined gas-electric utilities, and probably have varied over time for all utilities as well. It is worth emphasizing that in capital-intensive industries such as electric utilities, how one measures fixed costs presents important difficulties, and we suspect that reliable conclusions that are robust to alternative accounting conventions would be difficult to obtain.

There are, however, a number of useful extensions of this research. Within the electric utility context, one might also want to take into account variations in the "quality" of electricity, such as the system reliability and average duration of any downtime.[26] More generally, the approach taken in this paper could in principle be applied to other industries, not only partially regulated ones such as natural gas and telecommunications, but also to firms in various deregulated industries.

FOOTNOTES

[1]The methodology of our "bottom line" statistical benchmark study differs of course from benchmark studies that focus on a much more disaggregated, detailed and homogeneous operational or functional level of analysis.

[2]We selected the 1984-93 time period since public data were not generally available in electronic format prior to 1984.

[3]We employ the BLS regional CPIs for the Northeast, South, North Central and West. Although the BLS publishes CPIs for selected metropolitan areas, the metropolitan CPIs do not cover all utilities in our sample; moreover, the BLS advises that the metropolitan-level data may not be as reliable as the regional data. It is worth noting that use of a regional CPI captures differential rates of price *change* across regions, but it does not incorporate regional differentials in price *levels*.

[4]Since all investor-owned utilities face the same federal statutory tax rates and provisions, we do not net out federal tax payments.

[5]The coal capacity indicated for SCE reflects partial ownership of coal generation facilities in Arizona. There is no coal generation capacity within California.

[6]Industrial users often receive services from high-voltage transmission lines, whereas residential users require service from low-voltage distribution lines that transform power to levels acceptable for home use. Thus it is reasonable to posit that NSAR and share of residential customers are positively related.

[7]Since these percentages sum to 100% for each utility each year, they are not independent; we drop the coal share, and thus regression coefficients should be interpreted as relative to coal.

[8]For the moment, assume that $E(\nu_i \nu_j) = 0$ for $i \neq j$, $E(\epsilon_{it} \epsilon_{js}) = 0$ for $s \neq t$ or $i \neq j$, and that $E(\epsilon_{it} u_j) = 0$ for all i, t and j.

[9]Our interpretation of this error component model builds on the work of Hoch [1955], Mundlak [1961] and Griliches [1957]. These writers were particularly interested in obtaining estimates of the β's (in their contexts, typically estimates of Cobb-Douglas production function parameters based on farm outputs and inputs data) that were not biased because of a failure to take management impacts into account. While these researchers focussed on consistent estimates of the β's, here we use their approach but instead focus more of our attention on obtaining consistent estimates of the management effects. For a review of this literature, see Chamberlain [1984].

[10]This transformation is due to Wayne Fuller and George Battese [1973,1974].

[11]On this, see Badi Baltagi and Qi Li [1992], and Trevor Breusch [1987]. Notice that the traditional concept of a likelihood function whose sample magnitude is to be maximized becomes quite unclear when the panel is unbalanced, since the disturbance vector has random length.

[12]See, for example, Greene [1993, Chapter 16].

[13]For a clear exposition of this point, see Yair Mundlak [1978].

[14]Results of Monte Carlo studies have also been reported in Baltaji-Rak [1992] and Taylor [1980], but all these studies typically focus their attention on small sample estimates of the β's, not of the α_i's.

[15]A discussion of estimation issues for error components models in the context of simultaneous equations is found in, inter alia, Baltagi-Raj [1992, pp.91-94].

[16]To do this, we employ the Fuller-Battese transformation for all observations other than those involving the three California utilities, and introduce indicator variables for the three California utilities, whose data are not transformed with the Fuller-Battese procedure.

[17]We also considered employing a more time-series oriented stochastic specification, such as one involving an ARMA process. While feasible in the context of balanced panels (see, for example, Galbraith and Zinde-Walsh [1995]), in our situation with missing observations and an unbalanced panel, there would be a very substantial decrease in the number of observations available for efficient estimation.

[18]It is worth noting that the weather cooling degree days variable performs very well as an instrument; its t-value in the first-stage regression was 11.72.

[19]We also undertook an analysis in which we instrumented the ROR variable using Moody's bond rating categories. Although this increased the size of the coefficient on the ROR variable, again each of the dummy variable coefficients on the three California utilities was insignificantly different from zero (point estimates for SDG&E and SCE were negative, while that for PG&E was positive).

[20]For a discussion, see fn. 16, p. 144 in Berndt [1991], which in turn is based on Aitchison and Brown [1966].

[21]It is worth noting that with the linear specification, occasionally some of the California utilities had a positive and significant "California" effect. Such findings are not reliable, however, since the linear specification is clearly dominated by the logarithmic model.

[22]See Green [1993, Chapter 16] for details and references.

[23]This occurs since the fitted and actual values are identical (the residual is zero) when each additional parameter is unique to the additional utility observation. For discussion, see Berndt [1991, Exercise 7, pp. 48-50]. It should also be noted that in the context of an unbalanced panel such as ours, both equations are estimated weighting each utility by T_i.

[24]To do this with a logarithmic specification, one must allocate the linear and joint multiplicative effects on price, and compute partial effects by evaluating at the sample means of the other variables. This involves use of Taylor's series approximations. Details of the procedures we employed are given in a technical appendix available from the authors upon request.

[25]The non-zero impact of time trend reflects the fact that data for all three California utilities were available for the entire ten-year time period, whereas the "average utility" was observed 8.87 years.

[26]For analyses of reliability in the electric utility industry, see, inter

alia, Doane, Hartman and Woo [1988a,b], Grosfeld-Nir and Tishler [1993], and Woo and Train [1989].

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