Technical Appendix to

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Sample attrition

Sample attrition due to plant retirements and ownership transfers that eliminated data reporting creates a concern about selection bias. Plant sales are particularly an issue in retail access states, as exempt wholesale generators (EWGs) purchased plants that incumbents were required to divest during restructuring. Since EWGs are not regulated by FERC in the traditional sense, these companies are not required to report operating data on their plants, and hence drop out of the database. If the plants dropping out of the sample were systematically higher or lower in efficiency relative to their plant-epoch effect and to surviving plants, selection bias could contaminate the restructuring estimates. One could construct plausible stories that plants selected for purchase were very efficient and thus good acquisition targets, or that inefficient plants left the database due to retirements or sales that offered the new owners the opportunity for substantial gain if efficiency could be improved.

Plant exits. We have performed several analyses regarding the plants that exit our sample. Figure T1 graphs the number of plants that exit the sample in each year by restructuring status and divestiture status. While restructuring states have greater numbers of plant exits in the latter part of the sample than do nonrestructuring states, most of this difference is accounted for by the divestiture of plants in restructuring states. If one excludes plants that were divested, there appears to be no consistent pattern of plant exits based on restructuring status. That is, we are not systematically missing data observations for more plants in either restructuring or nonrestructuring states.

Divestitures. In related work, James B. Bushnell and Catherine D. Wolfram (2005) analyze the effects of generating plant divestitures on efficiency. Their analysis explores which plants were selected for divestiture, and concludes that divestitures were the result of statewide policy decisions. Statistics on IOU generation ownership for restructuring states (table T1) suggests that utilities had little discretion over which plants they could divest. New York is the only restructuring state for which IOUs retained ownership of any significant generating assets, but these plants were either in New York City and covered by contracts that recognized their importance to the transmission grid or were plants that had formerly been owned by the Long Island Lighting Company (LILCo) and were transferred to Keyspan as part of the outcome of LILCo's bankruptcy. Divestitures of plants in nonrestructuring states (e.g., IN, KY, OH, NH, VA) typically were made by utilities that had multi-state service territories that included restructuring states. It is unlikely, therefore, that divestitures systematically selected plants with either high or low efficiency levels.

Robustness. To assess the impact of attrition, we estimated both basic GLS-IV specifications and specifications that included the *RETAIL ACCESS* variable, in each case restricting the sample to plants that were in the dataset in the last year, 1999. The results for this sample are for the most part very similar to the results for the full sample, suggesting that selection bias is not significant in our results. The one exception is the coefficient on *IOU*RETAIL*, which declines in magnitude and is statistically indistinguishable from zero. This may be consistent with anecdotal evidence suggesting that utilities deferred maintenance on plants they knew they were going to divest, which could be picked up with the coefficient on *IOU*RETAIL* until we limit the sample, or simply due to the difficulty of estimating the *RETAIL ACCESS* effect with so little data. Imposing this selection rule eliminates, for example, almost all California and New York IOU plants due to required divestitures before 1999.

Robustness of results to alternative instruments

The conclusion that same-plant input use declined after restructuring for IOU plants appears robust to a variety of alternative specifications, although the precise magnitude of the estimated efficiency gain depends crucially on the estimated relationship between inputs and output. In tables T2 and T3, we analyze the sensitivity of the results to alternative output specifications and instrument sets, using the basic GLS-IV specification of the regression model (similar to column 2 of tables 3 and 4).

Columns 1 through 3 of tables T2 and T3 introduce several nonlinearities in the instrument set. In column (1), we add an interaction term between ln(*STATE SALES*) and *RETAIL ACCESS*, allowing the relation between state demand and plant output to vary after implementation of retail access. Column (2) interacts ln(*STATE SALES*) with fuel type indicator variables, allowing the plant output to respond differentially to state demand depending upon fuel type. To generate the instruments used in Column (3), we calculated the average heat rate (the inverse of the fuel efficiency) before 1993 for each plant epoch. We then divided the plants into heat rate quintiles by state and allowed the coefficient on ln(*STATE SALES*) to vary by quintile. (Plant epochs for which we only have observations after 1993 were assumed to be in the most efficient quintile.) The fourth columns in both tables T2 and T3 include total annual cooling degree days and total annual heating degree days, both measured at the state level, in addition to ln(*STATE SALES*). Our theoretical framework suggests that labor and maintenance decisions will be made based on planned levels of output, which should be uncorrelated with weather realizations. As with any model, this is a generalization, and some components of our nonfuel expenses variable are likely to vary with temperature driven changes in output (such as chemicals used to treat cooling water), so we decided to assess the sensitivity of our results to using this instrument.[1]

For employment, reported in table T2, the first two instrument sets increase the estimated magnitude of the coefficient on ln(*NET MWH*) relative to the specifications reported in Table 3, though both coefficients are imprecisely estimated. The restructuring effect remains statistically significant and is very similar in magnitude to the coefficient estimates reported in Table 3. In columns (3) and (4) the coefficient on ln(*NET MWH*) becomes negative, although it is never statistically distinguishable from zero. The coefficients on *IOU*RESTRUCTURED* increase slightly. For nonfuel expenses, reported in table T3, changing the instrument set has varying effects on the estimated magnitude of the output elasticity as well as the impact of restructuring, relative to results in Table 4. Efficiency effects remain large relative to municipal plants in all specifications, and roughly the magnitude of those found in table 4 with respect to IOU plants in nonrestructuring regimes, with the exception of results in column (1).

The final column of tables T2 and T3 uses plant output in years (t-2) through (t-4) as instruments for year t output. This follows discussions in Richard Blundell and Stephen Bond (1998, 2000) on the use of lagged values of endogenous variables in applications to input demand and production function specifications, particularly in the case of weak exogenous instruments.[2] In our case, lagged values of output are highly correlated with output in all specifications. While the output coefficient is less precisely estimated with these instruments than in our base model, and

[1] We also estimated specifications that allowed the sensitivity to temperature to differ depending on whether the entity owning the plant faced its annual peak demand in the summer or the winter. These interaction terms were significant (plants owned by summer-peaking companies are more sensitive to cooling degree days than plants owned by winter-peaking companies), and the results were more similar to our base specification than those reported in columns (4) of tables T2 and T3.

[2] In specifications that use only the one-period lagged value of output as an instrument, both coefficients and standard errors are huge.

negative in the employment model, the estimated effects of restructuring are broadly consistent with our other results.

Table T4 reports first-stage results for various instrument sets. Column (1) reports the results from the first stage specification for our main estimating equation, which uses only ln(*STATE SALES*) as an instrument. Columns (2) through (5) report results from the first stages using alternative instrument sets, corresponding to columns (1) through (4) of tables T2 and T3.[3] All of the variants considered in columns (2)-(5) of table T4 provide similar explanatory power. Also, while the interaction between ln(*STATE SALES*) and *RETAIL ACCESS* is statistically significant, neither the interactions with the fuel type dummies nor the heat rate quintiles are statistically distinguishable from one another, suggesting that there are not statistically distinguishable differences in the relationship between plants' output and state-level demand within either of these categories. Interestingly, the coefficient estimates in column (3) suggest that the relationship between heat rate and demand sensitivity is nonlinear, and that the most efficient and least efficient plants are the most sensitive. This may reflect the fact that our data are measured at the annual level, so while an inefficient plant may be very responsive to demand on an hour-to-hour basis, measuring state sales at the annual level makes it difficult to pick up this strong correlation since the peaking plants run very few hours per year. Conversely, efficient baseload plants may shift their maintenance outages to accommodate more perceptible swings in demand.

Robustness of standard error to alternative correlation assumptions

In tables T5 and T6 we present results from a number of additional robustness checks. To facilitate comparisons, column 0 in each table replicates the base model specification from text tables 3 and 4. Column 1 of tables T5 (employment) and T6 (nonfuel expenses) present results that

[3] The results in table T4 correspond directly to the first stage for table T3 (as well as tables 4 and 5 in the main text) since we did not include ln(*WAGE*). Specifications corresponding to the first stage for table tables T2 and 3, which includes wages as an exogenous variable, are very similar to those reported in T4.

allow the error terms to be correlated across plants and years within the same state, using Stata's cluster option at the state level. Recall that the base model allows for correlation across plants within the same state-year (conditional on year effects and the other covariates), but not across years. While the standard errors increase slightly, the results remain substantively the same.

Robustness to alternative samples

Columns 2 through 4 of tables T5 and T6 test the robustness of the results to alternative sample construction. The sample used for column 2 assumes that combustion gas turbine (GT) plants listed separately from steam turbine or CCGT plants with the same name (apart from the GT designation) may in fact share inputs and output. We therefore aggregate these into a single observation, rather than excluding all GT plants as we do for our base sample. After aggregating these plants and excluding observations based on the remaining sample criteria, including our outlier tests, we are left with a sample of 10303 observations. As indicated in column 2, results for this revised sample are virtually identical to those for our base sample.[4]

In columns 3 and 4, we report the sensitivity of results to our regression diagnostic cuts. Column 3 reports the basic specification on the full sample with no outlier cuts (10227 observations). Column 4 reports results using a tighter screen for outliers, based on the David A. Besley, Edwin Kuh, and Roy E. Welch (1980) screen of dfbeta > $2/(N^{.5})$, which is equal to 0.02 for our sample, or dfits > 2*sqrt(k/n), equal to 0.6 in our sample (including plant epoch effects in k). As indicated in tables T5 and T6, the results are robust to this screen.

[4] The employment *RESTRUCTURED* effect is slightly smaller (at 0.026 v. 0.031) in the untrimmed sample (column 3 of table T5) and the standard error slightly larger (at 0.016 v. 0.015), which means the estimated coefficient is no longer distinguishable from zero at the five percent significance level, though it is quite close to the estimates in the remaining columns.

Robustness of results to including forward-looking output

We have explored specifications that allow for a fixed cost of changing input levels, particularly employment.[5] This could cause a plant that foresees declining output over a several year period, for instance accompanying restructuring, to reduce employment in a discrete change. If this were the case, the plant would appear especially unproductive prior to an employment reduction, and especially productive following the reduction until the lower output levels are realized. To investigate the effect of dynamic considerations on our results, we estimated input demand equations that included variables measuring the forward looking changes in output.[6] The coefficient on the forward looking changes in output were positive and statistically significant in the specifications similar to those in columns (1) of tables 3 and 4, but negative and statistically indistinguishable from zero once we instrumented for both output and forward changes in output. In both cases, the coefficients on our restructuring variables were virtually unchanged.

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[5] There is an extensive labor literature on the presence of fixed costs and other nonconvexities in employment decisions.
[6] This input demand equation is based on one derived by Paola Rota (2004), who specifies a dynamic model where firms face a fixed cost to changing their employment levels.

References

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Bushnell, James B. and Catherine D. Wolfram. 2005. "Ownership Change, Incentives and Plant Efficiency: The Divestiture of U.S. Electric Generation Plants." Center for the Study of Energy Markets Paper CSEMWP-140.

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FIGURE T1

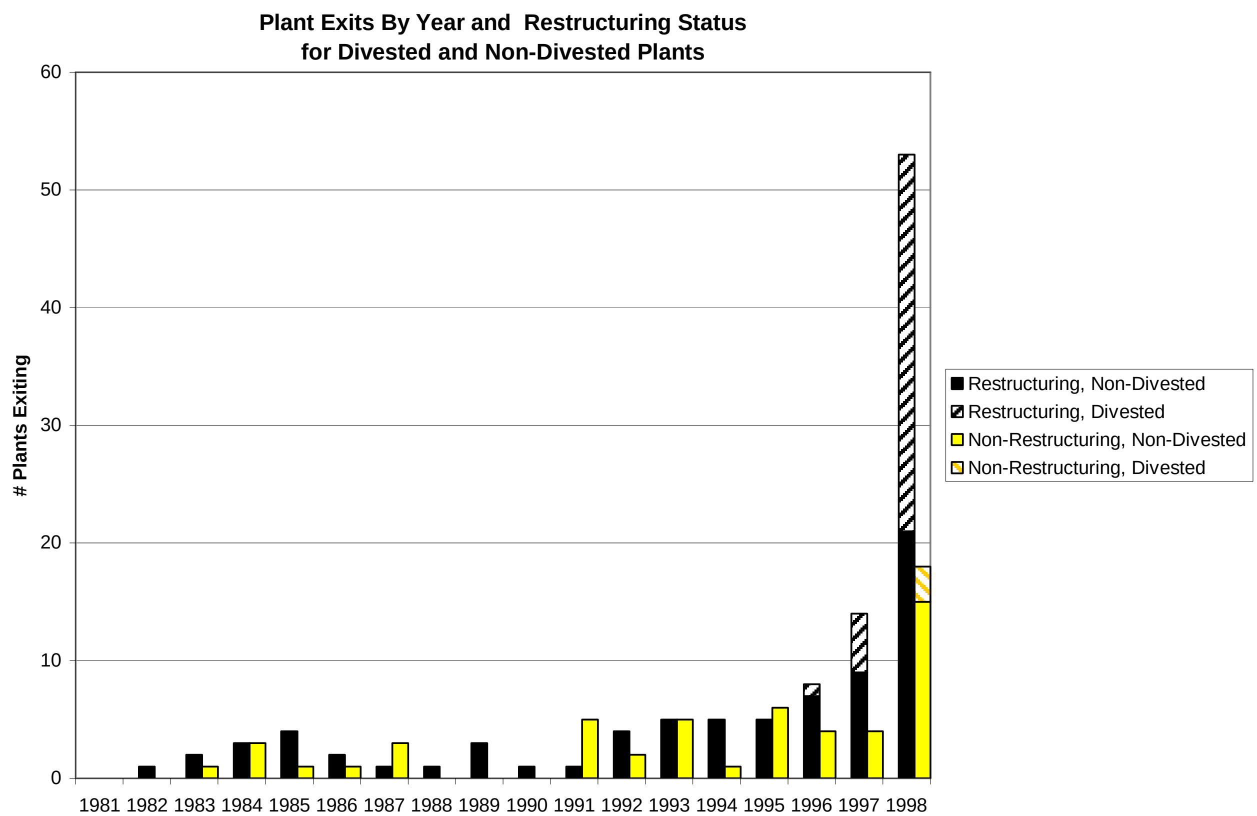
Plant Exits By Year and Restructuring Status
for Divested and Non-Divested Plants
Plants Exiting
60
50
40
30
20
10
0
1981 1982 1983 1984 1985 1986 1987 1988 1989 1990 1991 1992 1993 1994 1995 1996 1997 1998
Restructuring, Non-Divested
Restructuring, Divested
Non-Restructuring, Non-Divested
Non-Restructuring, Divested

Table T1: 2003 Electric Generating Capacity in States with Divestitures Total and IOU-Owned

State	State Total Fossil Fuel Generating Capacity (MW)	IOU Generation Ownership Percent (2003)
CA	36600	2
CT	5750	0
DC	590	0
DE	3084	0
IL	32486	5
MA	11983	0
MD	10373	1
ME	2860	0
MT	2448	6
NJ	15258	4
NY	28196	19
PA	33424	5

Table T2: Results Using Alternative Instruments – ln(*EMPLOYEES*)

	(0)	(1)	(2)	(3)	(4)	(5)
Instruments:	ln(*STATESALES*) Baseline – Column (2) of Table 3	ln(*STATESALES*) ln(*STATESALES*) X *RETAIL ACCESS*	ln(*STATESALES*) X Fuel-type	ln(*STATESALES*) X Heat Rate Quintile	ln(*STATESALES*), CDD, HDD	ln(*NET MWH*)$_{t-2,}$ ln(*NET MWH*)$_{t-3,}$ ln(*NET MWH*)$_{t-4}$
*IOU**	-0.031**	-0.030*	-0.030**	-0.035**	-0.039***	-0.042**
RESTRUCTURED	(0.015)	(0.016)	(0.015)	(0.014)	(0.014)	(0.018)
*MUNI*POST*	0.032**	0.035***	0.034***	0.029**	0.029**	0.033**
1992	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.015)
*MUNI*POST*	0.060***	0.063***	0.062***	0.054***	0.054***	0.038*
1987	(0.013)	(0.014)	(0.014)	(0.012)	(0.012)	(0.023)
ln(WAGE)	-0.012	-0.014	-0.014	-0.010	-0.011	0.002
	(0.013)	(0.014)	(0.013)	(0.014)	(0.014)	(0.022)
ln(NET MWH)	0.067	0.111	0.099	-0.011	-0.051	-0.058
	(0.064	(0.077)	(0.062)	(0.065)	(0.047)	(0.132)
ρ	0.72	0.67	0.68	0.76	0.75	0.56
Observations	10079	10079	10079	10079	10069	6726

Specifications include *SCRUBBER* as well as plant-epoch and year fixed effects.
Estimates are GLS-IV using a Prais-Winsten transformation for serial correlation.
Standard errors in parentheses, clustered for correlation within a state-year.
Temperature variables are not available for Alaskan plants (10 obs) in column (4).
The observation count is lower in column (5) because we are using lagged variables as instruments.
* significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Table T3: Results Using Alternative Instruments – ln(*NONFUEL EXPENSES*)

	(0)	(1)	(2)	(3)	(4)	(5)
Instruments:	ln(*STATESALES*) Baseline – Column (2) of Table 4	ln(*STATESALES*) ln(*STATESALES*) X *RETAIL ACCESS*	ln(*STATESALES*) X Fuel-type	ln(*STATESALES*) X Heat Rate Quintile	ln(*STATESALES*), CDD, HDD	ln(*NET MWH*)$_{t-2,}$ ln(*NET MWH*)$_{t-3,}$ ln(*NET MWH*)$_{t-4}$
*IOU**	-0.051**	-0.034	-0.049*	-0.055**	-0.054**	-0.050
RESTRUCTURED	(0.026)	(0.029)	(0.026)	(0.025)	(0.025)	(0.038)
*MUNI*POST 1992*	0.069***	0.072***	0.070***	0.069***	0.068***	0.063***
	(0.021)	(0.024)	(0.021)	(0.020)	(0.021)	(0.023)
*MUNI*POST 1987*	0.109***	0.103***	0.108***	0.110***	0.110***	0.094***
	(0.022)	(0.024)	(0.022)	(0.021)	(0.021)	(0.032)
ln(NET MWH)	0.417***	0.538***	0.435***	0.388***	0.398***	0.449**
	(0.090)	(0.104)	(0.083)	(0.086)	(0.087)	(0.195)
ρ	.33	0.35	0.34	0.33	0.33	0.42
Observations	10079	10079	10079	10079	10069	6726

Specifications include *SCRUBBER* as well as plant-epoch and year fixed effects.
Estimates are GLS-IV using a Prais-Winsten transformation for serial correlation.
Standard errors in parentheses, clustered for correlation within a state-year.
Temperature variables are not available for Alaskan plants (10 obs) in column (4).
The observation count is lower in column (5) because we are using lagged variables as instruments.
* significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Table T4: First Stage Results – *ln(NET MWH)*

	(1)	(2)	(3)	(4)	(5)
*IOU**	-0.099***	-0.098***	-0.103***	-0.098***	-0.104***
RESTRUCTURED	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)
*MUNI*POST 1992*	-0.026	-0.022	-0.031	-0.026	-0.024
	(0.038)	(0.038)	(0.039)	(0.038)	(0.039)
*MUNI*POST 1987*	0.021	0.021	0.017	0.023	0.022
	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)
ln(STATE SALES)	0.737***	0.697***			0.629***
	(0.214)	(0.209)			(0.205)
*ln(STATE SALES)**		-0.018**			
RETAIL ACCESS		(0.007)			
*ln(STATE SALES)**			0.742***		
COAL			(0.214)		
*ln(STATE SALES)**			0.733***		
GAS			(0.214)		
*ln(STATE SALES)**			0.729***		
OIL			(0.214)		
*ln(STATE SALES)**				0.818***	
1^{st} QUINTILE HR				(0.192)	
*ln(STATE SALES)**				0.731***	
2^{nd} QUINTILE HR				(0.198)	
*ln(STATE SALES)**				0.698***	
3^{rd} QUINTILE HR				(0.245)	
*ln(STATE SALES)**				0.665***	
4^{th} QUINTILE HR				(0.190)	
*ln(STATE SALES)**				0.863***	
5^{th} QUINTILE HR				(0.267)	
ANNUAL CDD					0.134***
					(0.044)
ANNUAL HDD					0.006
					(0.022)
R^2	0.96	0.96	0.96	0.96	0.96
ρ	.47	.47	.47	.47	.48
F-statistic on instruments	11.86	9.70	5.80	4.32	9.34
Observations	10079	10079	10079	10079	10069

Plant-epoch and year fixed effects included.
Estimates corrected for the presence of serial correlation using a Prais-Winsten transformation.
Standard errors in parentheses, clustered for correlation within a state-year.
F-statistics from test that instruments are jointly equal to zero reported in second to last row.
* significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Table T5: Robustness Table – ln(*EMPLOYEES*)

	(0)	(1)	(2)	(3)	(4)
	Baseline – Column (2) of Table 3	Clustering errors at the state level	Aggregating GT plants	Before cuts based on regression diagnostics	With tighter cuts based on regression diagnostics
IOU RESTRUCTURED*	-0.031**	-0.031**	-0.030**	-0.026	-0.034***
	(0.015)	(0.015)	(0.015)	(0.016)	(0.009)
*MUNI*POST 1992*	0.032**	0.032*	0.042***	0.033	0.045***
	(0.012)	(0.018)	(0.013)	(0.028)	(0.009)
*MUNI*POST 1987*	0.060***	0.060***	0.061***	0.041**	0.048***
	(0.013)	(0.016)	(0.014)	(0.019)	(0.009)
ln(WAGE)	-0.012	-0.012	-0.016	-0.039**	0.072
	(0.013)	(0.008)	(0.014)	(0.018)	(0.051)
ln(NET MWH)	0.067	0.067	0.105	0.089	-0.062***
	(0.064	(0.080)	(0.065)	(0.061)	(0.021)
ρ	0.72	0.72	0.67	0.68	0.67
Observations	10079	10079	10303	10227	7996

Specifications also include *SCRUBBER* as well as plant-epoch and year fixed effects.
Estimates corrected for the presence of serial correlation using a Prais-Winsten transformation.
Standard errors in parentheses, clustered for correlation within a state-year (except in column (2)).
All specifications estimated using instrumental variables using ln(*STATE SALES*) as an instrument for ln(*NET MWH*).
* significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent

Table T6: Robustness Table – ln(*NONFUEL EXPENSES*)

	(0)	(1)	(2)	(3)	(4)	(5)
Instruments:	Baseline – Column (2) of Table 4	Clustering errors at the state level	Aggregating GT plants	Before cuts based on regression diagnostics	With tighter cuts based on regression diagnostics	Including wage variable
IOU RESTRUCTURED*	-0.051**	-0.051	-0.053*	-0.051*	-0.080***	-0.051*
	(0.026)	(0.034)	(0.028)	(0.027)	(0.017)	(0.026)
*MUNI*POST 1992*	0.069***	0.069***	0.066***	0.066***	0.078***	0.076***
	(0.021)	(0.023)	(0.021)	(0.025)	(0.015)	(0.022)
*MUNI*POST 1987*	0.109***	0.109***	0.111***	0.108***	0.113***	0.102***
	(0.022)	(0.033)	(0.022)	(0.027)	(0.016)	(0.022)
ln(WAGE)						0.423***
						(0.090)
ln(NET MWH)	0.417***	0.417***	0.391***	0.423***	0.384***	-0.048**
	(0.090)	(0.110)	(0.092)	(0.091)	(0.095)	(0.024)
ρ	.33	.33	.32	.34	.23	.33
Observations	10079	10079	10303	10227	7996	10079

Specifications also include *SCRUBBER* as well as plant-epoch and year fixed effects.
Estimates corrected for the presence of serial correlation using a Prais-Winsten transformation.
Standard errors in parentheses, clustered for correlation within a state-year (except in column (2)).
All specifications estimated using instrumental variables using ln(STATE SALES) as an instrument for ln(NET MWH).
* significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.