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Examining Uncertainty in Demand Response Baseline Models and Variability in Automated Responses to Dynamic Pricing

Johanna L. Mathieu, Duncan S. Callaway, and Sila Kiliccote

Abstract

Controlling electric loads to deliver power system services presents a number of interesting challenges. For example, changes in electricity consumption of Commercial and Industrial (C&I) facilities are usually estimated using counterfactual baseline models, and model uncertainty makes it difficult to precisely quantify control responsiveness. Moreover, C&I facilities exhibit variability in their response. This paper seeks to understand baseline model error and demand-side variability in responses to open-loop control signals (i.e. dynamic prices). Using a regression-based baseline model, we define several Demand Response (DR) parameters, which characterize changes in electricity use on DR days, and then present a method for computing the error associated with DR parameter estimates. In addition to analyzing the magnitude of DR parameter error, we develop a metric to determine how much observed DR parameter variability is attributable to real event-to-event variability versus simply baseline model error. Using data from 38 C&I facilities that participated in an automated DR program in California, we find that DR parameter errors are large. For most facilities, observed DR parameter variability is likely explained by baseline model error, not real DR parameter variability; however, a number of facilities exhibit real DR parameter variability. In some cases, the aggregate population of C&I facilities exhibits real DR parameter variability, resulting in implications for the system operator with respect to both resource planning and system stability.

I. INTRODUCTION

We have traditionally relied upon the supply-side for power systems services; however, in recent years we have begun to rely more upon the demand-side, with commercial buildings and industrial (C&I) facilities participating in demand response (DR) programs. This shift results in a number of interesting challenges. While it is simple to measure control response of a power plant, the control response of a C&I facility is usually estimated using a baseline model that has uncertainty, which makes it difficult to determine exactly how much power is shed during a DR event. Moreover, while traditional power plants respond to control signals predictably and repeatably, C&I facilities can exhibit variability in their response. These two issues are illustrated in Fig. 1. In this figure, we plot the actual and baseline-predicted load for an office building on two DR event days (referred to as ‘DR days’) and one normal day during the summer of 2007. The left and middle plots show that responses to DR signals can be variable, while the right plot demonstrates baseline model error. Without quantifying the baseline model error, it is difficult to determine if the observed variability in response is a result of real variability in DR behavior (e.g., building managers/occupants overriding pre-programmed DR strategies; broken equipment; variability in response as a function of occupancy, weather, etc.) or simply unmodeled load variability (i.e. model error).

The purpose of this paper is to understand how power from grid-interactive C&I facilities varies in response to open-loop control signals, and what that implies for the system operator, which is tasked with matching supply and demand in real time. Specifically, we aim to understand how much observed variability is attributable to control response variability versus unmodeled load variability. If all observed variability resulted from unmodeled load variability, the system operator could expect consistent DR behavior and would only need to deal with the usual amount of demand-side variability. However, if control response variability is present, the system operator may need to deal with more demand-side variability than usual, requiring additional power systems services (e.g., reserves). In extreme cases, control response variability could result in area control error (ACE) and system stability issues.

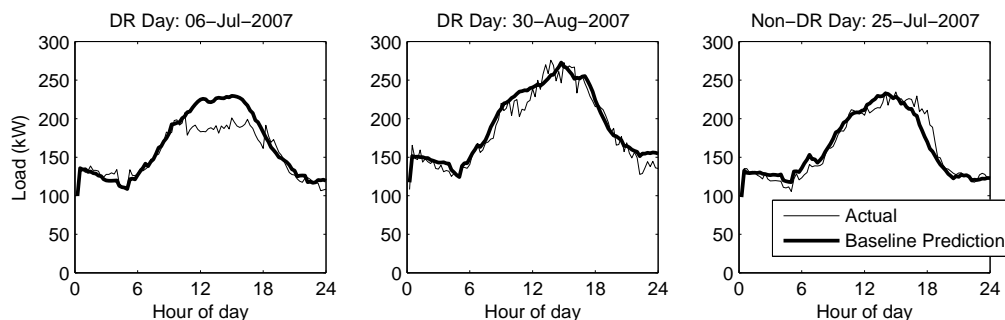


Fig. 1. Actual and baseline-predicted demand for an office building on three different days during the summer of 2007. The left and middle plots show data from DR events days (the difference between the actual and the baseline prediction is a combination of the control response to the DR signal and model error), while the right plot shows data from a normal day (the difference between the actual and the baseline prediction is model error).

TABLE I
NUMBER OF FACILITIES BY YEAR AND TYPE.

	Office Buildings	Prisons & Jails	Manufacturing Facilities	Retail Stores	Retail Stores with PV ^a	Museums	TOTAL
2006 (Zone 1)	3	0	0	1	0	0	4
2006 (Zone 2)	6	1	0	1	0	1	9
2007	7	1	1	3	1	1	14
2008	12	1	8	2	3	1	27
2009	17	3	8	1	3	1	33
TOTAL	45	6	17	8	7	4	87

^aRetail stores with solar photovoltaics (PV).

In order to analyze variability, we must first compute the error associated with DR parameter estimates (e.g., demand shed estimates). It is uncommon to conduct detailed error analyses on DR baseline models. In Section IV, we reference a few studies that have attempted to estimate baseline model error; however, all employ methods that underestimate the true error. Moreover, none of the studies present errors associated with DR parameter estimates. Therefore, we have developed a method to compute error estimates associated with DR parameter estimates. We use this method and data from 38 C&I facilities that participated in an automated DR program in California to understand DR parameter variability.

A note on terminology: The DR community uses several different terms to denote the counterfactual power usage on DR days: baselines, predictions, and forecasts. In this paper, we use the term ‘baseline predictions’ to refer to ex-post estimates of counterfactual power usage computed with regression parameters (identified with historical demand/temperature data) and *actual* temperature data for the purpose of Measurement and Verification (M&V). We reserve the term ‘forecast’ for ex-ante estimates computed with *forecasted* temperature data, which we do not discuss in this paper. We use the term ‘DR parameter estimates’ to refer to values, such as demand sheds, computed with actual demand data and baseline predictions. The DR community often refers to these values as ‘DR calculations’; however, we prefer our terminology because it makes clear that the values are uncertain. The term ‘DR parameter estimates’ should not be confused with ‘DR estimates,’ engineering estimates of expected demand sheds.

The rest of this paper is organized as follows: In Sections II and III, we describe our data and baseline model. In Section IV, we explain our error analysis. Then, in Section V, we present our results and discussion with respect to baseline model error and DR parameter variability. Lastly, in Section VI, we conclude.

II. DATA

We use 15-minute interval whole building electric load data from 38 large C&I facilities in California that participated in Pacific Gas and Electric Company’s (PG&E’s) Automated Critical Peak Pricing (CPP) Program between 2006 and 2009. PG&E called CPP DR events on up to 12 summer weekdays per year when system-wide load was expected to be high, which, in California, usually occurs on hot summer days as a result of air conditioning. On DR days, electricity prices were raised to three times the normal price from 12 to 3 pm (moderate price period), and five times the normal price from 3 to 6 pm (high price period). These prices were fixed (i.e. not modified in response to changes in load), and so they were a form of open loop control.

In exchange for participating in the program, facilities paid lower energy prices on non-DR days. All 38 facilities used the Open Automated Demand Response (OpenADR) Communication Specification [1] to receive DR event notifications, which were provided by 3 pm the business day before the event. Each facility implemented a different set of pre-programmed DR strategies and executed the same strategies from event-to-event. Strategies included changes to the heating, ventilation, and air conditioning (HVAC) system, light dimming/switching, and industrial process shedding [2].

In 2006, DR events were called separately in two geographic zones; nine were called in Zone 1 and eleven in Zone 2. In both 2007 and 2009, twelve events were called, while in 2008 eleven events were called. Several facilities participated in only a portion of the DR events in a year. If we knew that a facility did not participate in a certain DR event, we did not analyze data from that DR day.

Facilities’ demand profiles change year-to-year due to equipment upgrades, changes in usage patterns, etc. To reduce the chance of creating baseline models with data from before and after significant structural changes only one year worth of data were used to create each model. In total, we have 87 facility-years worth of data (Table I), where a facility-year is defined as one year of data for one facility. Twelve facility-years of available data were not analyzed because of significant structural changes visible in the data.

To create the aggregate populations, we excluded facilities that did not participate in all of the DR events in a year and facility-years for which we were missing more than one week of data. In sum, nine facility-years were not included in the aggregate populations (hence the discrepancy in number of facilities between Tables I and IV). All aggregate results are computed from baseline models built with the aggregate data, not the aggregate output of individual baseline modes.

From the National Climatic Data Center [3], we acquired hourly outdoor air temperature data for each facility from the nearest weather station. Unfortunately, some of the temperature data are spotty. We linearly interpolated the data to assign an approximate temperature to every 15-minute interval, though when six or more hours of data are missing we do not interpolate. In some cases, when the data for a station were particularly spotty, we have filled the holes with data from another nearby station. Temperature data for the aggregate populations were generated by weighting and averaging data from the individual stations. We weighted the data by the number of facilities in the aggregate population associated with that station.

III. BASELINE MODEL & DR PARAMETERS

Electric load baseline models are used for different purposes depending upon the type of DR program: demand/capacity bidding programs use baseline models to compute financial settlements, while dynamic pricing programs, such as PG&E’s CPP Program, use baseline models primarily for M&V. Electric utilities generally use simple baseline models, many of which involve averaging the daily electric demand over several days (e.g., those with the highest energy usage) before the DR day [4], [5]. More accurate regression-based baseline models, which have long been used for M&V by the energy efficiency community [6], [7], [8], are increasingly used for DR M&V [4], [5], [9], [10]. More sophisticated baseline modeling methods (e.g., neural networks) have been proposed, but are seldom used in practice.

We use the regression-based baseline model described in [11] because it performs similarly to or better than most baseline models commonly used for DR M&V. Therefore, our assessment of the magnitude of baseline model error is conservative. Another advantage to using a better baseline model is that it allows us to better determine if a facility exhibits real variability in its response to a DR event.

A brief description of the baseline model is as follows: We expect demand to be a function of time-of-week. Regression coefficients, α_i , are assigned to each each 15-minute interval from Monday to Friday, t_i where $i = 1 \dots 480$. We also expect demand to be a piecewise linear and continuous function of outdoor air temperature, T , as described in [6], [7]. Observed temperatures are divided into six equal-sized temperature bins¹ and a regression coefficient, β_j where $j = 1 \dots 6$, is assigned to each bin. Each coefficient is multiplied by a temperature component $T_{c,j}$, computed from T , as described in [11]. We model the same temperature effect across all occupied mode hours (transitions between occupied and unoccupied are manually determined by looking at plots of average daily demand profiles on non-DR days). Estimated occupied mode demand, \hat{D}_o is:

$$\hat{D}_o(t_i, T(t_i)) = \alpha_i + \sum_{j=1}^6 \beta_j T_{c,j}(t_i). \quad (1)$$

We model a different temperature effect across all unoccupied mode hours. Since the facility often experiences a smaller range of temperatures during unoccupied mode (usually nighttime), we model the temperature effect as linear with only one regression coefficient, β_u , which is multiplied by outdoor air temperature T . Estimated unoccupied mode demand, \hat{D}_u is:

$$\hat{D}_u(t_i, T(t_i)) = \alpha_i + \beta_u T(t_i). \quad (2)$$

Since all 2006-2009 DR days were called May 1 to Sept 30, baseline models were constructed with non-DR day demand data during the same period. We did not use data from holidays, weekends, or days that appeared to have had power outages (i.e. days when the minimum power use is less than a percentage of the average minimum daily power use during the summer) to build the baseline models.

The parameters α , β , and β_u are estimated with Ordinary Least Squares (OLS). We use the OLS estimator because, though it not ‘best’ (in a Gauss Markov sense) due to autocorrelation and heteroscedasticity (see Section IV), it still produces unbiased regression coefficients [12], [13]. However, the standard errors associated with the regression coefficients are underestimated, so we do not use them.

The parameter estimates and temperatures on DR days are then used to predict demand on DR days. Four DR parameters (Table II), computed from the baseline predicted demand and the actual demand, are used to characterize changes in electricity use on DR days. These parameters were first defined in [11]; however, here we define Daily Peak Demand and Daily Energy slightly differently: as absolutes, not percentages.

IV. ERROR ANALYSIS

Most error analyses on regression-based baseline models use the standard errors associated with the regression coefficients [6], [10], [8]. However, these errors underestimate the true error due to a number of issues. First, the regression parameters are correlated. Specifically, time-of-week is correlated to temperature: the highest temperatures tend to occur in the afternoon and the lowest temperatures occur overnight. Second, the regression residuals are autocorrelated. In Fig. 2, we show autocorrelation functions (ACF) and partial autocorrelation functions (PACF) computed with regression residuals from two

¹Through trial and error, six bins were found to allow for enough change points and not cause over-fitting problems. This value is not optimized.

TABLE II
DR PARAMETERS.

Parameter	Definition	If this value is positive...	Importance
Average Demand Shed (kW)	Predicted minus actual average demand during the DR event. ^a	...the facility reduced power use during the event.	Key indicator for how well the facility performed.
Rebound (kW)	Actual minus predicted average demand in the hour after the DR event (6-7pm).	...the facility increased power use after the event.	Could affect a facility's demand charges; synchronized rebounds could create a new system-wide peak.
Daily Peak Demand (kW)	Actual minus predicted maximum demand on the DR day. ^b	...the facility had a higher demand peak than it would have if there was no DR event.	Could affect a facility's demand charges; will not affect the system-wide peak unless the individual peaks are synchronized.
Daily Energy (kWh)	Actual minus predicted total energy use on the DR day.	...the facility used more energy than it would have if there was no DR event.	Gives us a sense for if energy shifting or shedding strategies predominate; helps us understand DR's effect on energy use and the environment, a research gap [14].

^aThe average demand shed is computed separately for the moderate price period ('Shed 1') and the high price period ('Shed 2').

^bThe actual and the baseline peak could happen at different times during the day.

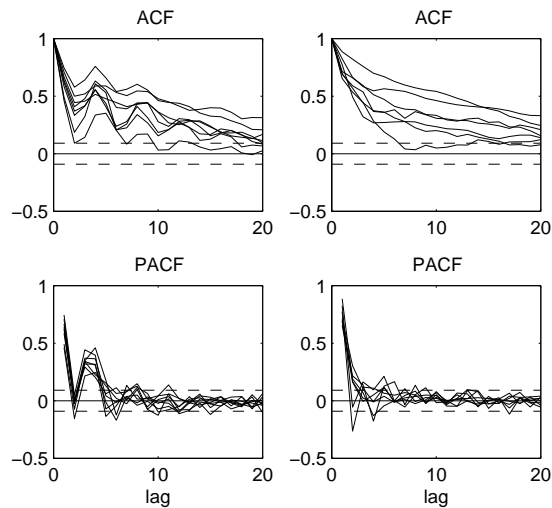


Fig. 2. ACF and PACF computed with the regression residuals from an office building (left) and a retail store (right) in 2008. Each line was created with data from a week (Mon-Fri) in which there were no DR days, holidays, or power outage days. Dashed lines show the 95% confidence interval ($\pm 2/\sqrt{n}$, where n is the number of data points in the data set).

facility-years. In both cases, the residuals are lag 1 autocorrelated, which is the case for all facility-years. In some cases, we find higher order autocorrelation.

Third, the regression residuals are heteroscedastic. Specifically, we find that the variance of the regression residuals (referred to as the 'error variance') is a function of time-of-week. For a typical commercial building, error variance tends to be lower at night and higher during the day when fluctuating occupancy affects loads. For some facilities, the error variance is high during transition periods (e.g., when the facility is being populated in the morning). Fig. 3 shows plots, created using (1) and (2), of error versus time-of-week. For the retail store, error is clearly a function of time-of-week, while for the office building, the effect is smaller. These results not only demonstrate heteroscedasticity, but also the importance of computing errors as a function of time-of-week. We have not computed error as a function of temperature or predicted demand because error does not seem to be a strong function of these variables.

These issues suggest that one should use caution in interpreting the standard errors associated with the baseline model regression coefficients. Fortunately, we do not need to calculate this in order to calculate the error associated with DR parameter estimates.

A. Method

The goal of our error analysis is to determine the error associated with each DR parameter estimate for each facility-year and each aggregate population. Other studies have used regression residuals to generate baseline model error estimates [9]; however, regression residuals are self-influenced: the model is built and tested on the same data set. Therefore, error estimates generated with regression residuals underestimate the true error.

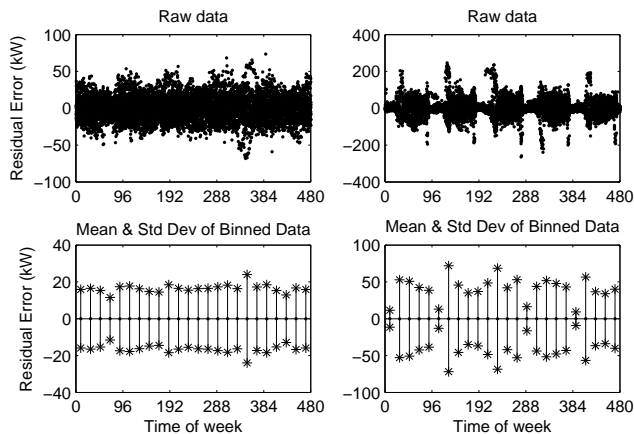


Fig. 3. Error versus Time-of-Week for an office building (left) and a retail store (right) in 2008.

To avoid self-influence, we use a resampling technique called ‘Leave One Out Cross Validation’ (LOOCV). LOOCV is a type of K-fold cross validation, which involves randomly partitioning the data into K subsamples, reserving one subsample, building the model with data from the remaining subsamples, testing on the reserved subsample, and repeating this process for all K subsamples. The results for each subsample are combined resulting in an estimate of the prediction accuracy. In LOOCV, K is equal to the total number of observations, n . LOOCV is useful when n is small, though the technique is computationally intensive.

We treat the demand on each non-DR day as an observation. Therefore, n is equal to the number of non-DR days used to create the baseline prediction model ($\sim 90 - 95$ days per facility-year). We leave out one non-DR day, build the model with data from the rest of the non-DR days, predict the demand on the day that has been left out, compute the quantities associated with the DR parameters (e.g., average demand between 12 and 3 pm), compare the predictions to the actual quantities to generate an error observation, and repeat for each non-DR day. Since we consider error as a function of time-of-week, only residuals computed with data from Mondays are used to determine errors on Mondays, etc. Therefore, for each DR parameter for each day of week there are only $\sim 18 - 20$ error observations. It is difficult to determine the true error distribution with so few error observations. Therefore, we assume that the error observations are normally-distributed and report error estimates as one standard deviation of the error observations.

We do not recommend using this error analysis method on baseline models parameterized with DR day data (e.g., morning adjustments [5]). For those models, this method will underestimate true model error if power use outside of the DR period is affected by the DR signal, which is common, especially for facilities that pre-cool, rebound, or otherwise shift energy use to the morning or evening on DR days.

B. Other Sources of Error

Error estimates generated using the method described above capture most of the error associated with DR parameter estimates including demand/temperature measurement error; error resulting from the fact that the weather stations are not co-located with the facilities; error resulting from temperature data interpolation; and unmodeled load variation on days similar to those used to build the baseline model. There are two other sources of error we have not addressed: over-fitting and extrapolation. DR days are generally called on the hottest days of the summer which means that, in some cases, baseline predictions are made with temperatures: (1) higher than those on non-DR days, resulting in extrapolation error; and (2) experienced only a few times on non DR-days, resulting in over-fitting error. For 26% of our DR day baseline predictions, the highest temperature on the DR day is greater than the highest temperature used to build the baseline model. In a preliminary investigation, we found that model error associated with extrapolated baseline predictions is comparable to that associated with non-extrapolated baseline predictions. Other baseline models, such as those that model a load as a purely linear function of temperature and those that use fewer data to build the model, may be more susceptible to over-fitting/extrapolation error.

V. RESULTS & DISCUSSION

A. DR Parameter Errors

The error analysis method presented in Section IV-A allows us to assign error estimates to DR parameter estimates. In Fig. 4, we show DR parameter and error estimates for all 2009 facility-years and the 2009 aggregate population. In most

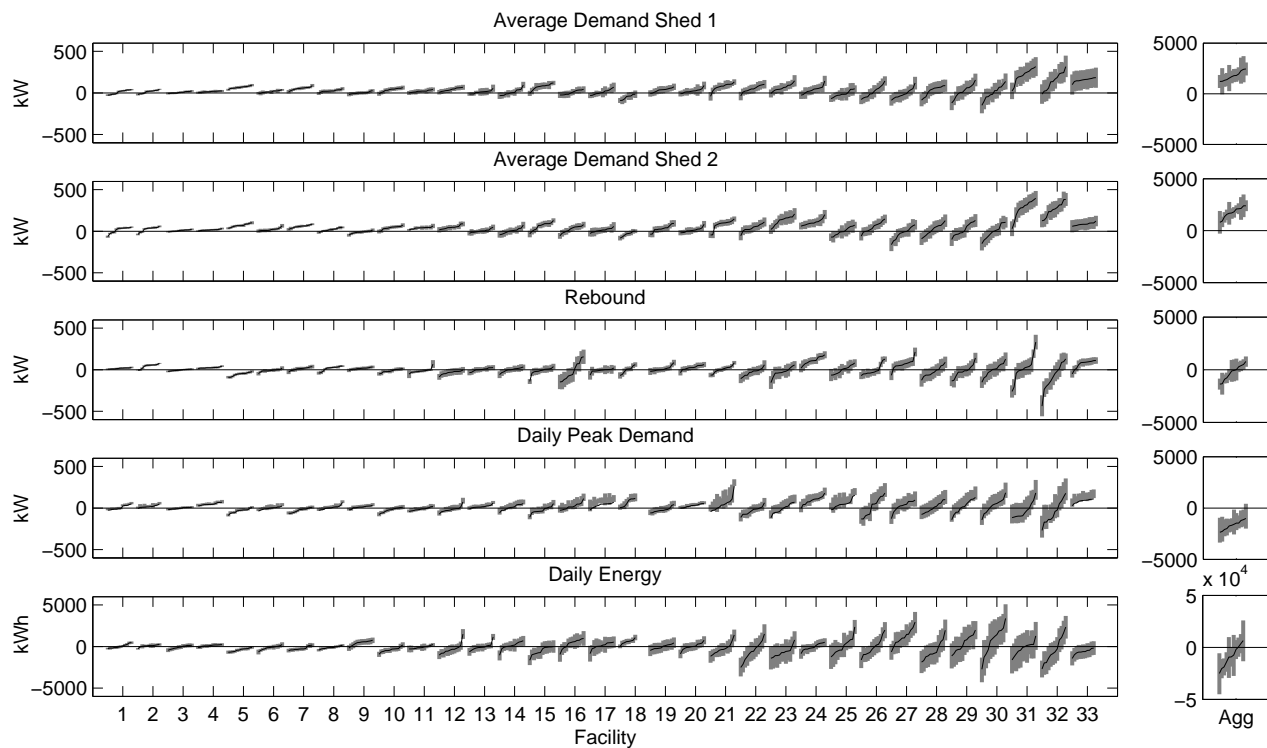


Fig. 4. DR parameter estimates (black) and error estimates (grey) for all 2009 facility-years. Facilities are arranged in order of smallest to largest mean error for Average Demand Shed 1. For each facility and each DR parameter, parameters are arranged in order of smallest to largest. Results for the 2009 aggregate population are shown on the right.

cases, the error estimates are large relative to the DR parameter estimates. In addition, observed DR parameter variability is often large. However, given the magnitude of the error estimates, we would expect some observed DR parameter variability.

This interpretation of Fig. 4 illustrates how including error estimates along with DR parameter estimates allows us to draw the right conclusions from the data. Without error estimates, it would be easy to classify a facility with observed shed variability as a variable shedder, and, therefore, conclude that such a facility is difficult to control. However, if the error associated with that facility’s shed estimates is large, then it is possible that the control response is actually consistent and we are simply unable to measure the exact response because of baseline model error.

There are several other things to learn from Fig. 4. Some facilities that shed power during DR events consume less energy on DR days, while some do not, meaning that they shift load outside of the DR period. We find that the Daily Peak Demand is often biased low, because regression-based baseline models tend to under-predict maximum values (i.e. outliers). We also learn that, for most facilities, when error estimates are large for one DR parameter, they are large for all DR parameters. The aggregate population results demonstrate that DR works: the aggregated facilities shed power during DR events and reduce the peak demand on DR days, despite the fact that individual facilities may become peakier. Also, on average, the aggregated facilities exhibit almost no rebound and save some energy on DR days, indicating that there is some net curtailment—the facilities do not simply shift all load outside of the DR period.

We do not discuss the statistical significance of the DR parameter estimates because the error estimates are not confidence intervals. Since a facility’s DR behavior from one DR event to the next is not independent, Bayesian techniques should be used to not only determine appropriate confidence intervals, but also pinpoint DR parameter estimates. This would involve pooling information across DR events (i.e. using knowledge about a facility’s behavior during one DR event to help us predict its behavior during another DR event). We do not tackle this here because we are interested in using the error estimates to assess DR parameter variability, not statistical significance.

B. DR Parameter Variability

Observed DR parameter variability has two possible sources: unmodeled load variability and real parameter variation. For example, consider the Average Demand Shed. We generally observe shed variability from one DR event to the next. We would like to know if observed shed variability is a result of real shed variability (i.e. a facility curtails a different amount

from event-to-event) or if it results from unmodeled load variability (i.e. baseline model error). If observed shed variability results exclusively from unmodeled load variability, then we can expect consistent control responses and the system operator need only deal with the usual level of demand-side variability. If real shed variability exists, the system operator may require additional reserves to deal with more demand-side variability than usual.

In Section V-B.1, we derive a metric, the Average Demand Shed Variability Metric (SVM), to discern between unmodeled load variability and real parameter variation. Similar derivations yield metrics for each DR parameter: the Rebound Variability Metric (RVM), Daily Peak Demand Variability Metric (PVM), and Daily Energy Variability Metric (EVM). In Sections V-B.2 and V-B.3, we present DR parameter variability metric results for the individual facility-years and the aggregate populations, respectively.

1) *SVM Derivation*: On a DR day, the *Observed Load (OL)* is equal to the *Real Baseline Load (RBL)* minus the *Real Shed (RS)*:

$$OL = RBL - RS. \quad (3)$$

Neither *RBL* nor *RS* can be measured. *RBL* is estimated with the *Predicted Baseline Load (PBL)*. The difference between *RBL* and *PBL* is the *Unmodeled Load (UL)*:

$$UL = RBL - PBL. \quad (4)$$

To compute the *Observed Shed (OS)*, the *PBL* is subtracted from the *OL*:

$$OS = OL - PBL = UL - RS. \quad (5)$$

Our goal is to determine the variance of *RS*. Therefore, we take the variance of (5), which results in:

$$\text{Var}(OS) = \text{Var}(UL) + \text{Var}(RS) - 2\text{Cov}(UL, RS). \quad (6)$$

We can estimate $\text{Var}(OS)$ by taking the variance of the 9 – 12 observed sheds and $\text{Var}(UL)$ by taking the variance of the ~ 95 error observations (since DR events can occur on any weekday, error observations are used without regard to day-of-week). Therefore, we define the shed variability metric (SVM) as:

$$\begin{aligned} \text{SVM} : &= \text{Var}(OS) - \text{Var}(UL) \\ &= \text{Var}(RS) - 2\text{Cov}(UL, RS). \end{aligned} \quad (7)$$

While the SVM does not tell us the exact value of $\text{Var}(RS)$ due to the complicating covariance term, it does tell us if real shed variability likely exists or not. Also, since $\text{Var}(RS) \geq 0$, the SVM may tell us something about the sign of the covariance term. If the covariance term is positive, then as unmodeled load increases, real shed increases. This could occur when the equipment that drives *UL* is also the equipment that is curtailed. Alternatively, if the covariance term is negative, then as unmodeled load increases, real shed decreases. This could occur when load is higher than predicted, electricity consuming services are in high demand, and occupants/building operators override automated DR strategies; or when load is higher than predicted, the HVAC system is operating at or beyond its maximum capability, and consequently a reduction in HVAC setpoint has a limited effect.

2) *Individual Facility-years*: To compare facilities by SVM, we normalize the measurements of *UL* and *OS* such that $\text{Var}(UL) = 1$. Therefore, the minimum value of SVM is -1 (i.e. when $\text{Var}(OS) = 0$). Each DR parameter variability metric is normalized similarly.

Histograms showing DR parameter variability metrics for the 87 facility-years are shown in Fig. 5. To understand what these histograms tell us about real parameter variability, we can compare them to distributions generated for the case when real parameter variability is zero. If real parameter variability were zero, the covariance term would also be zero, resulting in a DR parameter variability metric of zero. However, we are unable to compute the ‘true’ values of the DR parameter variability metrics because we can only estimate observed parameter variance from ~ 11 observations. Assuming that the observations are normally-distributed, we would expect the distribution of observed parameter variances to follow a scaled χ^2 distribution with $N - 1$ degrees of freedom [15]:

$$\frac{(N - 1)x}{\sigma^2} \sim \chi_{N-1}^2, \quad (8)$$

where x is the sample variance, N is the number of observations, and σ^2 is the true variance. Therefore, the expected variability metric distributions for the case when real variability is zero is that given in (8), shifted left by 1 (resulting from the subtraction of $\text{Var}(UL) = 1$ in (7)). These distributions (for $N = 11$) are plotted in Fig. 5. One caveat associated with these results is that we have assumed that we know the ‘true’ value of $\text{Var}(UL)$, though, in reality, it is an estimate (computed from ~ 95 observations). When we normalize the measurements of *UL* and *OS* such that $\text{Var}(UL)=1$, any error in our estimate of $\text{Var}(UL)$ will affect our estimate of $\text{Var}(OS)$, which, in turn, affects our estimate of the SVM.

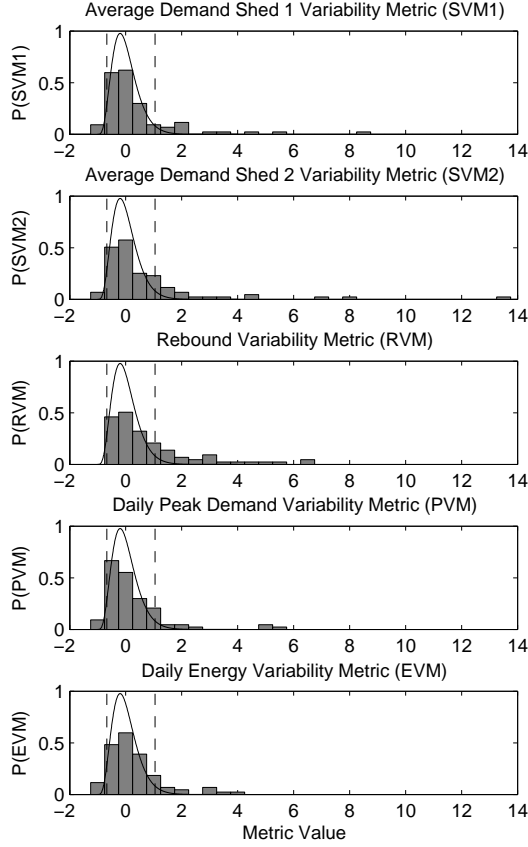


Fig. 5. Histograms showing DR parameter variability metrics for the 87 facility-years. Solid lines show the expected distributions if real parameter variability were zero and $N = 11$ (dashed lines show the 95% confidence interval). Disproportionally positive variability metrics result from real parameter variability. Disproportionally negative variability metrics result from negative covariance and, subsequently, real parameter variability.

TABLE III
FACILITY-YEARS WITH VARIABILITY METRICS INSIDE AND OUTSIDE THE 95% CONFIDENCE BOUNDS.

Metric	Inside Bounds	Outside Bounds		
		Below	Above	TOTAL
SVM1	65 (75%)	8 (9%)	14 (16%)	22 (25%)
SVM2	62 (71%)	4 (5%)	21 (24%)	25 (29%)
RVM	62 (71%)	2 (2%)	23 (26%)	25 (29%)
PVM	71 (82%)	6 (7%)	10 (11%)	16 (18%)
EVM	69 (79%)	6 (7%)	12 (14%)	18 (21%)

* Percentages do not always add properly due to rounding.

If none of the facility-years exhibited real parameter variability then we would expect only 5% of facilities to fall outside of the 95% confidence bounds. However, for each parameter, we find that substantially more than 5% of the facility-years fall outside of the bounds (Table III). This implies that some facility-years exhibit real parameter variability. Facilities with disproportionately positive variability metrics likely exhibit real parameter variability. Facilities with disproportionately negative variability metrics likely exhibit positive covariance and, subsequently, real parameter variability. For the remainder of the facility-years, any observed parameter variability may simply result from model error and sampling.

Through simulation we find that, in order to achieve the distributions shown in Fig. 5, it is likely that a number of facility-years have large real parameter variability, while the majority of facility-years have little to no parameter variability. Also, it is likely that for the vast majority of facility-years the covariance term is positive which implies that as unmodeled load increases, real shed increases, which is consistent with intuition. Additionally, we find that all combinations of the variability metrics are all positively correlated, with SVM1 and SVM2 being the most correlated ($\rho_{x,y} = 0.76$).

TABLE IV

DR PARAMETER VARIABILITY METRICS COMPUTED FOR THE AGGREGATE POPULATIONS. BOLD VALUES INDICATE P-VALUES ≤ 0.05 .

Year	Facilities (Peak ^a)	Shed 1		Shed 2		Rebound		Daily Peak Demand		Daily Energy	
		SVM1	p-value	SVM2	p-value	RVM	p-value	PVM	p-value	EVM	p-value
2006 (Zone 1)	4 (2.7 MW)	-0.819	(0.01)	-0.269	(0.67)	0.077	(0.75)	-0.386	(0.47)	-0.737	(0.04)
2006 (Zone 2)	8 (8.4 MW)	3.039	(<0.01)	3.399	(<0.01)	1.044	(0.05)	1.131	(0.04)	4.578	(<0.01)
2007	13 (11.7 MW)	0.579	(0.21)	-0.117	(0.90)	-0.454	(0.32)	-0.531	(0.24)	-0.210	(0.78)
2008	21 (14.6 MW)	-0.210	(0.72)	-0.142	(0.86)	1.295	(0.02)	-0.217	(0.71)	0.163	(0.62)
2009	32 (26.9 MW)	-0.696	(0.03)	-0.331	(0.46)	0.304	(0.43)	-0.702	(0.04)	-0.227	(0.69)

^aPeak demand computed for May 1 - Sept 30.

The Federal Energy Regulatory Commission's (FERC) has called for better understanding of responses to dynamic prices as a function of customer type [14], so we attempted to disaggregate parameter variability results by facility attributes including facility type, HVAC system type, DR strategy, and shed size. Results were inconclusive because of the small number of facility-years in the data set. It was particularly difficult to disaggregate the facilities by DR strategy because many facilities use more than one strategy. Therefore, we were unable to determine what kinds of facilities have more or less variable DR parameters. In an effort to do this, we are in the process of acquiring a larger data set.

3) *Aggregate Populations*: DR parameter variability metrics for each aggregate population are shown in Table IV. For each variability metric, we have computed the two-sided p-value under the null hypothesis that there is no real parameter variability. Therefore, real parameter variability likely exists when p-values are small. Surprisingly, the aggregate populations exhibit a wide range of variability metrics, similar to that seen for the individual facility-years. We would expect more real DR parameter variability in smaller aggregate populations. For example, in 2006 Zone 2 (8 facilities), we find likely real variability in each DR parameter. However, we also find likely real variability in both the Average Demand Shed 1 and the Daily Peak Demand in 2009 (32 facilities). Real variability in the aggregate could result from unmodeled correlation across facilities and/or large variable facilities dominating the aggregate results.

VI. CONCLUSIONS

We have developed a method to determine the error associated with DR parameter estimates. We find that this error is often large and so DR parameter estimates reported without error estimates may be misleading. For example, we may classify a steady shedder as a variable shedder and, therefore, judge the facility to be poorly controlled when, in fact, baseline model error simply prevents us from measuring consistent sheds. Since DR parameter estimates have error, all calculations derived with these estimates, including cost effectiveness estimates, also have error. Future research should explore the degree to which DR parameter error affects cost/benefit analyses on DR programs and technologies.

We also find that observed DR parameter variability is driven, in large part, by baseline model error. For most facilities, observed DR parameter variability can likely be explained by baseline model error alone; however, a number of facilities likely exhibit high variability in control response. In addition, most facilities exhibit a positive correlation between unmodeled load and real shed.

Variability metrics computed for the aggregate populations show that in some cases the aggregate likely exhibits variability in control response, which has implications for the system operator. If aggregate control response is not consistent, the system operator may have to deal with more demand-side variability than exists on non-DR days and, therefore, will need to procure more power systems services. In extreme cases, control response variability could result in ACE and system stability issues. More research is needed to understand control response variability in aggregate populations composed of facilities executing manual DR strategies, as they may exhibit even more variability than populations composed of facilities executing automated strategies.

The DR signal considered here is open loop (often implemented in the individual facilities as closed-loop indoor air temperature control). Our results would be different if a closed-loop DR signal were used. Specifically, we would expect less control response variability, which could mitigate some of the issues we have described. This is an important subject of future research.

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