Estimating the Effect of Exposure to Market Transformation Programs on Demand or Supply of Conservation Technology

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Discrete choice simultaneous equation regressions may be used to estimate conservation technology demand or supply shifts due to market transformation programs. Essentially, the regressions compare the purchases or sales of the technology among exposed vs. unexposed individuals, correcting for the simultaneity of the exposure and technology adoption decisions. To help distinguish program-induced technology adoptions from naturally occurring ones, regression results are used to simulate adoption decision or sales with and without exposure. Variance of the estimates can be approximated using a bootstrap, and stratified or choice-based sampling plans can be handled. Statistics cannot fully describe behavioral changes; therefore a serious evaluation of the market transformation effort requires in-depth interviews of a small sample of prospective buyers or suppliers. Methods are presented to use information from these interviews to improve the market shift estimates.

OVERVIEW

This paper presents a quantitative technique to estimate changes in efficient technology demand or supply due to exposure to an informational market transformation program. Essentially, the purchase or sales of efficient technology among exposed individuals or firms is compared to that among unexposed agents, over a large sample and with corrections for self-selection effects. Small-scale interview results are used to improve accuracy. The method does not apply to programs where people choose to be exposed because they *plan* to adopt the technology, such as a program providing lists of reliable insulation contractors to interested homeowners.

The method does, however, allow people's *predisposition* to adopt or sell the technology affect their likelihood to be exposed. For example, in an effort to encourage efficient lighting supply, the Lighting Research Center has published several articles on lighting technology in building trade journals (Conway and Block 1995). To study the effects on lighting suppliers' sales of efficient lights of being exposed to the articles, one might compare sales between exposed and unexposed suppliers. A regression framework would control for observed differences between lighting suppliers. Yet since suppliers interested in conservation are both more predisposed to promote efficient lights and more likely to read the articles than average, a simple regression comparison would probably attribute too many efficient light sales to the journal articles. One can control for this self-selection by modeling the exposure and sales regressions simultaneously.

A comparable demand-side transformation program might be to publish articles about efficient tumble-action washing machines in popular mechanics magazines. Since readers of the magazines may be more predisposed to trying new technologies than nonreaders, simultaneous equations regressions will be necessary to allow a valid comparison between tumble washer adoption among readers and nonreaders of the magazines.

The quantitative demand-side method we present is based on a simultaneous equations system which Train (1994) originally proposed for free ridership. In Parikh, Kandel & Brown (1995) we applied it to estimate the effects of an electric utility's conservation education efforts. The method requires a large-scale survey sampling both exposed and unexposed individuals or firms. The exposed and unexposed subsamples must each include some people who adopt the technology and some who do not. Our supply shift estimation procedure is a variation on the demand-side method, where the exposed and unexposed subsamples must each have a range of sales amounts.

In evaluating market transformation programs, it is generally good practice to interview a small sample of energy users and try to understand whether and how their behavior has changed. Herein, we propose methods for using such interview results to improve the quantitative demand shift estimates. For example, it is hard for a survey questionnaire to assess whether second-hand information such as word-of-mouth originates from the market transformation program, and is thus a type of exposure. As a result, such indirect exposure will typically be counted as non-exposure, causing underestimation of program effects. Yet if a small-scale set of interviews can assess how much apparent nonexposure is in fact second-hand exposure, the larger-scale statistical model can be adjusted accordingly. This paper describes how.

We also describe how to improve demand shift estimates when the causal effect of program exposure on technology adoption is more accurately described through interviews, or when large-sample mail survey respondents may not be able to answer survey questions as accurately as small-sample interviewees who can ask for guidance. Finally we note that technology use data can be collected from interviewees, to help assess the effects of the changes in adoption.

This paper provides a technical recipe for estimating the demand- and supply-side models, and discusses minor variations. It proposes a simple bootstrap method to estimate sampling error. It then explains how to estimate technology adoption effects and standard errors when samples are stratified or choice-based. Finally it describes how to improve the large-scale statistical estimates based on in-depth study of a smaller sample of interviewees.

The procedure we propose will not give as accurate results as a simple comparison under purely random design. If an electric utility wishes to test the effect of bill inserts on compact fluorescent light bulb purchases, for example, we recommend it include the inserts on a random sample of customers first, and compare their lighting purchases with those of customers who have not received the inserts. If an appliance store wishes to test the effect of a display promoting tumble washers, we recommend it set up and remove the display on random or alternate weeks, and compare tumble washer sales over time. Still, random design is impossible for many market transformation programs. For such cases our procedure will provide consistent market effect estimates, albeit with potentially large variance. (In Parikh, Kandel & Brown (1995) our standard error was one third as large as our demand shift estimate.[1])

Our method gives a one-time estimate and cannot measure future changes in technology purchases due to current programs. If a program is repeated the method will generally not differentiate between the effects of current and past exposure. To follow program effects and technology diffusion over time, one might want to supplement the study with aggregate market indicators proposed by Feldman (1995); market saturation of the technology and price evolution are two examples.

THE QUANTITATIVE MODEL: DEMAND SIDE

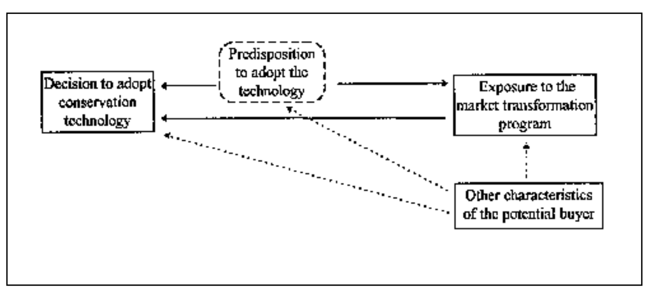
The ‘‘net’’ or program-induced demand shift for efficient technology is the increase in adoptions of that technology due to exposure to the market transformation program. Technology adoptions are driven by exposure, by predisposition to adopt the technology, and by other characteristics of the potential buyer (figure 1). Predisposition to adopt also influences the likelihood of exposure, but is unobserved. One needs to determine the effect of exposure on the probability of adoption, yet if adoption probability is regressed directly on exposure, the omitted predisposition variable will bias results. People whose predisposition to adopt causes them to be exposed to the program will appear to adopt because they were exposed, rather than because of their predisposition. In terms of figure 1, the predisposition box is omitted from a regression, as if some causality flows backward from adoption to exposure, causing simultaneous equations bias.

Train (1994) addressed this problem for free ridership estimation, in assessing how much of a person's decision to install a conservation measure was due to an energy audit program. He proposed a simultaneous equation model which allows exposure to be driven partly by the propensity to adopt, while adoption is driven partly by the outcome of exposure. In a first stage, one estimates each individual's probability of exposure. In the second stage that estimated probability becomes an instrument for the endogenous variable exposure, in a binary choice regression predicting the probability of adoption.

The Technology Demand Regressions

Estimation proceeds in two stages. First, define a binary exposure variable E equal to one if the individual was exposed to the market transformation program, and zero if not. Regress E on pertinent customer characteristics, typically using a probit or logit regression: $\Pr(Exposure) = \Pr(E=1) = F(Z'\gamma)$, where F represents the chosen cumulative distribution function (Normal or logistic) and Z is a set of customer characteristics affecting the likelihood of being exposed (plus the intercept vector of ones). The regression results in a vector g of estimates of the coefficient vector γ. Use g to calculate each customer's predicted probability of exposure, which will be the instrumental variable: $\hat{E} = F(Z'g)$.

Figure 1.



The goal of the second stage is to isolate the effect of exposure on the probability of adopting a technology. Two possible methods are the ''substitution procedure'' and Amemiya's ''nonlinear least squares with instruments,'' also called ''nonlinear two-stage least squares.'' For linear regressions, the substitution procedure would reduce to two-stage least squares, while Amemiya's procedure would reduce to classic instrumental variable estimation. Described briefly below, the methods are laid out in an easy-to-follow logit specification in Train (1994) p. 429.

Substitution procedure. Run a second stage binary choice regression including the instrument $\hat{E}$ as an independent variable: $\Pr(Adoption) = \Pr(A=1) = F(X'\beta + \alpha\hat{E})$. X is a set of variables affecting the adoption decision and may include some variables included in Z. The parameter estimates b and a, for β and α, respectively are not consistent, but the inconsistency is probably small (Train 1994). ''a'' estimates the effect of exposure on adoption of the conservation technology.

Nonlinear least squares with instruments. Nonlinear least squares with instruments yields consistent, but not efficient parameter estimates b and a, and is available as a procedure in several econometric software packages. The exposure dummy E rather than the instrument $\hat{E}$ is used to determine the residual $u_i = \mathrm{A} - F(X_i'\beta + \alpha E_i)$, which depends on the values of parameters β and α. β and α are estimated to minimize the instrument-weighted sum of squared residuals $u'W(W'W)^{-1}W'u$, where W is a matrix containing values of the exogenous variables X followed by a column of values of the instrument $\hat{E}$.

The Demand Shift Estimates

After b and a are estimated, they can be used to simulate buyer decisions and estimate the shift in technology demand due to the market transformation program. This section explains how, assuming all sample data came from a simple random sample. A later section will present adjustments for stratified or choice-based samples.

Define an ''exposed adopter'' as an individual exposed to the program who then adopts the technology. Exposed adopters' technology purchases represent the ''apparent'' effect of the market transformation program, or ''gross demand shift.'' A ''program-induced adopter'' is an exposed adopter who adopts the technology *because* he is exposed. Program-induced adoptions are the ''net demand shift.''

An exposed adopter who would have adopted the technology even without being exposed is an ''exposed natural adopter,'' because his adoption is naturally occurring. Exposed natural adoption is strictly analogous to free ridership in incentive-based conservation programs.

The probability that a given exposed adopter is a natural adopter is $\Pr(A_i = 1|E_i = 0)$, estimated as $F(X_i'b + a.0) = F(X_i b)$. The probability that exposed adopter i is a program-induced adopter is one minus $\Pr(A_i = 1|E_i = 0)$, estimated as $1 - F(X_i'b)$.

Summing over all n_{ea} exposed adopters in a simple random sample, program-induced adoption in the sample may therefore be estimated as

$$n_{ea} - \sum_{i=1}^{n_{ea}} F(X_i'b).$$

That is the net sample demand shift. Total adoption in the sample is n_{ea}, and is the gross sample demand shift (one purchase per adopter).

The net demand shift per exposed adopter, or net-to-gross demand shift ratio, is:

$$\Delta d_{ea} = \frac{n_{ea} - \sum_{i=1}^{n_{ea}} F(X_i'b)}{n_{ea}} \tag{1}$$

This is the probability an average exposed adopter's conservation technology purchase is program-induced.

Train (1994) proposed a different method for estimating the net demand shift per exposed adopter. The gross demand shift for individual i is the likelihood that she is an exposed adopter, estimated as the predicted probability she was exposed times her predicted probability of adoption given exposure, or $\hat{E}_i \cdot F(X_i'b + a)$. The likelihood i is an exposed *natural* adopter (rather than a program-induced adopter) is estimated as $\hat{E}_i \cdot F(X_i'b)$. Her net demand shift is her gross demand shift minus her natural demand shift, estimated as $\hat{E}_i \cdot [F(X_i'b + a) - F(X_i'b)]$. The entire sample is then used to estimate the net-to-gross demand shift per exposed adopter as

$$\Delta d_{ea} = \frac{n_{ea} - \sum_{i=1}^{n} \hat{E}_i \cdot [F(X_i'b + a) - F(X_i'b)]}{\sum_{i=1}^{n} \hat{E}_i \cdot F(X_i'b + a)} \tag{2}$$

Recalling that most discrete choice regressions are biased in finite samples, one might choose this second estimator because the bias factor will be comparable in both numerator terms, as well as the denominator, so that it tends to cancels out. That is, $F(X_i'b)$ may be biased by roughly the same percentage as $F(X_i'b + a)$; call that bias factor ''B.'' B multiplies both terms of the numerator, and the denominator; therefore it cancels. Estimation of $\hat{E}$ also adds finite sample bias which cancels out in the ratio. In equation (1) by contrast, the gross adoption estimate n_{ea} is unbiased while naturally occurring adoption $F(X_i'b)$ remains biased; therefore the entire expression is subject to finite sample bias. The cost of choosing equation (2) is increased variance, as known outcomes E_i are replaced by predicted probabilities $\hat{E}_i$.

In our data set for Parikh, Kandel & Brown, using logistic regressions, we found the two methods (equations 1 and 2) yielded the same ratio to two significant figures, probably because a large sample size allowed the average predicted probabilities of exposure and adoption to converge nearly completely to the sample proportions of the same.

Population net demand shift. If the population of exposed adopters is of size N_{ea} then the total population demand shift attributable to exposure, or population net demand shift, is estimated as $\Delta D = N_{ea}\Delta d_{ea}$. Where the number N_{ea} is not known, it can be estimated as $(n_{ea}/n)N$, where n is the size of the entire sample (including nonadopters and unexposed individuals) and N is the entire population size.

Variance

Variance of the net demand shift estimate ΔD is difficult to determine algebraically, since ΔD comes from a ratio, but one can estimate it easily using a bootstrap. Note that the simple random sampling process involves drawing independent and identically distributed observations from the population of customers, all variables (exogenous and endogenous) drawn jointly. To simulate this in a bootstrap, draw n observations from the sample data set, with replacement, to obtain a simulated data set, $dataset_k$.. From $dataset_k$, run the two-stage regression and estimate ΔD_k. Repeat this process over a large number K of simulations. The sampling distribution of ΔD is consistently estimated by the set of K values of ΔD_k, known as the bootstrap distribution.[2]

The variance of ΔD is then approximated by the sample variance of the bootstrap distribution. Note that the variance will be bigger if the population number of exposed adopters N_{ea} is not known but only estimated based on its sample proportions. To reflect this, the bootstrapping operation must collect a set of values of ΔD_k, rather than of $(\Delta d_{ea})_k$.

Confidence intervals, median, and other percentiles of the distribution of ΔD are estimated consistently as the percentile values of the bootstrap distribution. Thus a 2-tailed 90% confidence interval is the range between its fifth and ninety-fifth percentiles.

For a consistent estimator with small-sample bias, the bias can be approximated (to a first order) as the average deviation between simulated ΔD_k's and the ΔD estimated on the real (not bootstrap) sample. The ΔD estimate can then be revised by subtracting that bias, and variance estimated as average squared deviation in the bootstrap distribution from the revised estimate of ΔD.

If the regressions performed to obtain ΔD are nonlinear least squares with instruments, a consistent procedure, then the bootstrap above will be consistent. As the sample size n and number of simulations K grow, bootstrap-estimated variance will approach true sampling variance. For the substitution procedure the bootstrap estimates will be approximate.

QUANTITATIVE MODEL: SUPPLY SIDE

Our method can be applied to technology supply rather than demand if the sample of suppliers is large enough for the consistency property of discrete choice regressions to overcome their small-sample bias. ''Adoptions'' are replaced by ''sales increases,'' abbreviated in the following math as ''sales.'' Since ''sales'' is a continuous variable, the main regression will be linear, but the exposure regression typically remains binary.

Exposure, E, is regressed on independent variables to obtain the instrument $\hat{E}$, predicted probability of exposure. Next, $\hat{E}$ is used as a regressor in a second stage linear sales regression: $Sales = XB + \alpha\hat{E} + \varepsilon$, or as an instrument in classic linear instrumental variable estimation.

Alternatively, since sales is continuous, a Heckman-style self-selection correction can be added to the sales regression: $Sales = XB + \alpha E + \delta M + \varepsilon$, where M is a selectivity correction term, for example the inverse Mills ratio if $\hat{E}$ is estimated in a probit regression.

The gross supply shift is the total increase in sales of the technology among exposed suppliers. The net supply shift is the gross supply shift minus amount sales would have increased had the suppliers not been exposed. The net-to-gross supply shift ratio among exposed suppliers, then, is:

$$\Delta s_e = \frac{\sum_{i=1}^{n}(sales_i - X_i'b)}{\sum_{i=1}^{n} sales_i} \qquad (1')$$

where n_e is the number of exposed suppliers. Since $X_i'b$ is an unbiased estimator, there is no need to add variance by using expected exposure in the calculations, as was done for buyers in equation (2).

If the sample was drawn randomly over all suppliers, from a population including N_e exposed suppliers, the total net supply shift estimate would be $\Delta S = N_e \Delta s_e$. Typically, however, supplier samples will be stratified, requiring a weighted sum of stratum-specific supply shifts. In some cases the sample may equal the supplier population, in which case the numerator of Δs_e is the estimated net supply shift.

If past history is unavailable, ‘‘sales’’ may be defined as actual sales of the technology rather than increase in sales. $X_i'b$ then predicts levels of rather than changes in sales, so the only control for each supplier's exposure-generated sales is the naturally occurring sales of *other* suppliers that were not exposed. Clearly this is inferior to controlling for suppliers' naturally occurring sales based on their own past sales as well.

If supplier sample sizes are too small to justify the nonlinear exposure regression, a skilled interviewer might be able to estimate how much of each supplier's sales change is due to exposure. If so:

$$\Delta s_e = \frac{\sum_{i=1}^{ne} (\text{sales attributed to Exposure})_i}{\sum_{i=1}^{ne} sales_i}$$

Finally, note that some market transformation programs might work on the supply and demand sides, for example by facilitating supplier-purchaser interactions. In that case, supply and demand shifts can be estimated separately, and compared. Both are measures of the increase in transactions at the new demand/supply equilibrium, and should equal each other apart from measurement error.

STRATIFIED OR CHOICE-BASED SAMPLES

Sampling efficiency will often dictate a stratified or choice-based sample, particularly if the number of exposed or technology-adopting customers is small compared to the population. (Choice-based samples in this case would be samples stratified by variables including the adoption-or-not choice and/or the exposure-or-not event.) This section explains how to apply the above methods to such samples.

The Regressions

Ordinary unadjusted regression estimation on stratified samples is inefficient. It is also biased and inconsistent if the stratification is correlated with the dependent variable, as in choice-based sampling.

The simplest method for attaining consistency is to weight each observation by the inverse of its probability of being drawn, or a multiple thereof. A good-sized multiple is the population fraction N_h/N divided by the sampling fraction n_h/n, where N_h and n_h are the population and sample sizes of stratum h, the stratum in which a given observation falls.

For probit regressions such weighting is not efficient, but is often used because it is consistent and practicable. For the efficient maximum likelihood estimator, see an advanced text such as Amemiya (1985).

For linear least squares regressions, this method may conflict directly with weighting for heteroskedasticity and is not generally efficient but may be chosen where stratification inaccuracies outweigh heteroskedasticity inaccuracies. If stratification is based on a measurable correlate of the dependent variable, stratification biases can instead be controlled for somewhat by including that correlate as an independent variable; a set of stratum dummies is thus one solution. This assumes stratification only affects the intercept; to control for other coefficient effects, stratum interaction variables may be necessary. In the limiting case, the regression could be estimated separately in each stratum, so long as one does not try to average the stratum-specific coefficients into a (false) general population coefficient estimate.

Logistic regression coefficient estimates are efficient and maximum likelihood when one runs the regression without any weights, but includes stratum-specific intercepts (one dummy for each stratum except perhaps the ‘‘base case’’ stratum). To get consistent and efficient intercept estimates, subtract $ln[(\text{sampling fraction})_h/(\text{population fraction})_h]$ separately from each stratum's intercept estimate, $(b_0)_h$. For the base case stratum, subtract that same log ratio from zero to create an intercept.

See Skinner, Holt & Smith (1989) for details on linear or logistic regressions under diverse sampling plans, or Ben-Akiva and Lerman (1985) for treatment of stratified discrete choice regressions.

The Demand or Supply Shift Estimates

Define w_i as the weight attributed to observation i, equal to [the population fraction in i's stratum] over [the sampling fraction in i's stratum]. Then w_i is multiplied by each element of each sum in the demand and supply shift estimates. For example, equation 1 is replaced by:

$$\Delta d_{ea} = \frac{\sum_{i=1}^{nea} w_i[1 - F(X_i'b)]}{\sum_{i=1}^{nea} w_i}$$

Equivalently ΔD or ΔS can be estimated separately for each stratum and combined in a weighted average based on population stratum size.

Bootstrap

Since observations are presumably independently and identically distributed within a stratum, the resampling part of the bootstrap should be done separately for each stratum. That is, from each stratum h of size n_h, draw n_h observations with replacement from the n_h observations in the stratum. The simulation sample will then have n_h observations from each stratum h, for a total of n observations. In simulation run k, calculate ΔD_k as explained above in this section. The bootstrap distribution is then the set of all K ΔD_k's, as is the case for a simple random sample.

SMALL SAMPLE INTERVIEWS

Before any market transformation program is designed, a small sample of customers should be studied in-depth. Well-conducted interviews may suggest customer decision mechanisms, which in turn suggest how the market might be transformed and what survey questions would help assess program success. The questionnaire design process may go through several iterations as modifications based on one small group of customers are tested on a separate group.

After the market transformation program has been carried out, serious researchers will interview a small sample of energy users in depth to assess the program's behavioral effects. This small sample interview can also be used to improve the demand-shift estimates presented above, so long as it is conducted on a subsample of the larger mail/phone survey, and includes the four categories of buyers (exposed and unexposed, adopters and non-adopters). The methods we present rely on a skilled interviewer obtaining more accurate responses to questions than a written questionnaire could elicit, and are largely adapted from Kandel, Lang & McNally (1993).[3]

The exposure variable

Where second-hand effects of the program are common, seemingly unexposed individuals may in fact be exposed indirectly. For example, unexposed person A may buy efficient lighting because exposed person B told him about it. Analogous to spillover from incentive-based programs, the existence of unreported second-hand effects can bias program effectiveness estimates downward. The comparison group of reportedly unexposed individuals has higher technology adoption rates than they would without the program, making natural adoption rates appear too high and the net supply shift appear too low.

Interviewers may measure second-hand exposure, by calling up the sources of word-of-mouth information to see whether they were exposed to the program. If so, the improved ''exposure'' responses may be used in one of the following ways:

Bias adjustment. Perform the demand shift estimation twice on the small sample: once using the mail/phone response on exposure to obtain ΔD_{survey}, and once using the corrected exposure variable to obtain $\Delta D_{interview}$. The bias factor from underreporting exposure is estimated as $\Delta D_{interview}/\Delta D_{survey}$ and may be multiplied by the large-sample demand shift estimate to correct its bias. Where small sample size or other factors lower your confidence in the interview regression results, correct for less than the full bias estimated.

Imputation. On the interview sample, regress ''true'' exposure (based on the interview and follow-up) on self-reported (mail/phone) exposure, and independent variables predicting exposure (the Z in the large sample regression): $\Pr(E_{\text{``true''}} = 1) = F(Z_i'h + cE_{survey})$. Since binary choice regressions are biased in small samples, the linear probability model ($E_{\text{``true''}} = Z_i'h + cE_{survey}$) might be appropriate. Either regression yields an equation that predicts true exposure rather than the misreported exposure predicted by a large mail/phone survey first stage regression. Apply this equation to each member of the large sample to obtain predicted $\hat{E}_{\text{``true''}}$, which should then replace $\hat{E}$ in the second stage demand-shift regression. Use the substitution method, since the instrumental variable method involves the underreported outcome $E$. For the net demand shift estimate use equation (2), with $\hat{E}_{\text{``true''}}$ replacing $\hat{E}$, because equation (1) would invoke a downward-biased n_{ea} since exposed adopters underreport themselves in the large sample.

The effect of exposure on adoption

Skilled interviewers may be able to assess and to what extent interviewees' technology adoption was caused by exposure. Beware of interviewee self-report bias, however, if technology adopters are unable to correctly imagine how they would have behaved without exposure, or if they choose answers to fulfill strategic objectives or researchers' expectations. The quantitative model described above avoids all self-report bias, but is subject to sampling error (which diminishes with sample size) and misinterpretations of ambiguous survey questions. If you have conducted informative interviews where you expect little self-report bias, but find the large-sample questions ambiguous or insufficiently encompassing, you may wish to use interview results to improve your demand shift estimate.

First assess the accuracy of the large sample statistical results using survey responses of the subsample of interviewees. Apply the parameters a, b and g estimated on the large sample to predict a net-to-gross demand shift ratio on the smaller interviewed subsample, as in equations 1 or 2 above. A competing interview-based net-to-gross ratio is calculated as the apparent proportion of total adoptions which are not naturally occurring but required exposure. If the two net-to-gross ratios diverge significantly, consider applying one of the following procedures, depending on your analysis of the relative accuracy of the large and small sample questions and answers:

Bias adjustment. Use the divergence to estimate a direction of bias in the large sample statistical results. If interview net-to-gross is 80% of statistical net-to-gross on the small subsample and the interview is clearly more reliable, multiply your final large sample statistical demand shift estimate by 80%. Choose a less-than-full bias adjustment if the interview results are not unquestionably more accurate, per individual tested, than the statistical results. The bias adjustment procedure assumes that the small sample size is large enough to be representative in the relationship between interview and mail/phone information.

Bayesian weighted combination. Estimate demand shift as a weighted combination of the statistical and interview results. For example, demand shift = (.7)(statistical results) + (.3)(interview results). Assign weights based on your assessment of the relative accuracy of the competing estimates, based on relative sample sizes and the clarity of spoken vs. written questions and responses.

Adoption and other variables

For variables relating to technology, building characteristics, or decision-making processes, interview responses may be more accurate than mail/phone responses because interviewers can help respondents answer questions and perhaps inspect their buildings or technology. Hence for the interview sample there may be two sets of values for regression variables: those obtained from the mail/phone sample, and those obtained later from the interview.

The mail/phone values will have already been used as part of the large data set regression. On the corresponding interview variables, one may run the same regression to get a competing set of coefficient estimates. Where coefficients differ significantly, one may choose a Bayesian weighted combination of the two conflicting estimates. In assigning relative weights to the competing estimates, remember that small samples are not likely to represent the entire population as well as the larger sample, and that discrete choice regressions are biased in small samples. So any weighing in of small sample results are based on confidence that interview answers are more precise than mail/phone survey variables.

Use of the technology

Eventually demand shift estimates may be translated into rough energy savings estimates, which depend on use of the technology. Savings estimates can be based on billing analyses, with each technology adopter's net-to-gross demand shift ratio multiplying their observed savings, but that involves costly follow-up of all large sample members. Therefore a researcher may need to apply a savings per unit or engineering formula to the technology adoption figure. This can be improved with information on the use of the technology, most accurately obtained via the in-depth small sample interviews.

The interviewer tries to obtain accurate estimates of technology use T, perhaps as hours running per week, or an intensity of usage. Then using the interview sample, the researcher regresses T on a set of predictor variables V available in the large sample; for example if T is washing machine usage V might include an intercept, family size, and an indicator variable for laundry-intensive employment. V may also include mail/phone reported technology usage, which will probably be less accurate than the T elicited by a skilled interviewer, but correlated with it.[4]

Using regression results, one can predict a value of T for each large sample member, and use that predicted T value in the engineering-type equation estimating savings.

CONCLUSION

The effects of informational market transformation programs on sales and adoption of energy efficient technology can be estimated numerically, although the variance may turn out to be high. A demand shift is estimated by comparing adoption rates of exposed vs. unexposed individuals, and correcting for the simultaneous nature of exposure and adoption decisions. Supply shifts can be estimated based on changes in exposed and unexposed suppliers' sales of the technology.

These methods work for programs where exposure is easily definable, and is not required for adoption of the technology. Indirect effects of the program on not-directly-exposed populations may bias market shift estimates downward, however. Further, delayed responses to the program will not be captured.

A good analysis will include a set of in-depth interviews to observe behavioral effects of the program. These interviews may also collect more accurate responses to survey questions, and be used to improve the accuracy of the quantitative market shift estimates, as proposed in this paper.

Finally, note that our method is presented for discrete adoption and exposure choices, but the algebra can be extended to continuous, ordinal, countable, or multiple choice variables, using for example linear, ordered probit, Poisson, or multinomial probit or logit regressions.

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ENDNOTES

*Opinions expressed in this report are those of the authors only, and do not represent positions adopted by the Energy Commission Staff or the full Commission.

1. Given a sample size of nearly 700, this study estimated the demand shift net-to-gross ratio from Southern California Edison's commercial energy informational services at .59, with a standard error of 19.

2. Bootstrap resampling is simple using many software packages, but tricky in SAS, the software we used in Parikh, Kandel & Brown. For a copy of our SAS bootstrap program for simple or stratified samples, contact A. Vayssiéres Kandel at (916)654–4910 or kandel@gordy.ucdavis.edu.

3. For an edited and improved excerpt of the portion of Kandel, McNally & Lang which presents these and other tools, contact A. Vayssières Kandel.

4. Hungerford (1995) found that a small sample of housing co-op residents underestimated their typical washing machine usage by nearly half on a written questionnaire, but were more accurate in a later interview (he compared responses with a subsequent laundry room tally sheet).

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