MARKET SHARE SPECIFICATION, ESTIMATION AND VALIDATION : TOWARDS RECONCILING SEEMINGLY DIVERGENT VIEWS

by

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ABSTRACT

In the May 1984 issue of JMR three papers were published that are replications and extensions of an earlier paper of ours published in JMR in May 1981. They deal with various problems of market share specification, estimation and especially validation. A rough comparison of the research findings seems to indicate that no general conclusions can be arrived at. In fact the picture is not as black because a number of the differences in findings can be explained. Providing these explanations is the main purpose of this paper.

INTRODUCTION

In an earlier paper on the same subject (Naert and Weverbergh 1981 - hereafter NW) we arrived at the following tentative conclusions: danger of overparameterization; better predictions from models estimated by GLS (generalised least squares); no additional gain from iterative GLS; and, superiority of the attraction model over linear and multiplicative specifications. We called for replications by stating:

"Although the empirical results are for two totally different products (gasoline and electric razors) and different markets (dominance of seven and three brands, respectively), we do not exclude the possibility that the similarity in the findings of the two empirical studies is due simply to chance. Further empirical study is needed to confirm our conclusions, to refine them, or to refute them" (NW, p. 152).

It is encouraging to see that at least three teams of authors have responded to our call: Brodie and de Kluyver (1984); Ghosh, Neslin and Shoemaker (1984) and Leeflang and Reuyl (1984) (hereafter BdK, GNS and LR respectively).

Our discussion is organised as follows: on the basis of an analysis of variance we assess the extent of disagreement on the main conclusions in NW. The analysis shows that no generalizations seem possible from these 4 studies. The objective of the remainder of the paper is then to try to explain why the results of these studies are so much at variance. In doing so, we follow the logical sequence of steps taken by the model builder: specification, estimation and validation [1]. The conclusions will in essence be a set of guidelines to assist the model builder in going through this sequence of steps.

EXTENT OF DIVERGENCE OF RESULTS

Can any generalizations be derived from the 4 studies ? To answer this question we submitted the various results to an analysis of variance. As dependent variable we used

Y = the measure of prediction inaccuracy reported in the four studies [2]

As independent variables we used the following

NW2	:	a dummy for the second data set in NW
BdK1	:	a dummy for the first data set in BdK
BdK2	:	a dummy for the second data set in BdK
BdK3	:	a dummy for the third data set in BdK
GNS	:	a dummy for the data set in GNS
LR	:	a dummy for the data set in LR
MULT	:	a dummy for multiplicative specifications
ATT	:	a dummy for attraction models
GLS	:	a dummy for generalized least squares (GLS) estimation
IGLS	:	a dummy for iterative least squares (IGLS) estimation
NORM	:	a dummy for ex-post normalization
PR1	:	a dummy for intermediate levels of parameterization
PR2	:	a dummy for extended levels of parameterization

When we combine all data sets, specifications and estimation methods, a total of 147 observations is obtained and the regression model contains 14 parameters (including a constant term). Separating the results of NW gives 57 observations and 9 parameters, whereas the results of the three subsequent papers leads to 90 observations with 11 parameters. The estimation results of the linear regression of Y on all the independent variables are given in table 1.

insert table 1 about here

The picture looks rather depressing. While the results of all studies combined are to a large extent consistent with those of NW, looking at the three subsequent studies separately, seems to lead to no conclusions at all. We should of course be aware of the limitations of our analysis. First it is not clear that the conditions for applying ordinary least squares are satisfied. In that sense, it is safer to interpret the analysis as a heuristic diagnostic and not to assign too much weight to the statistical interpretation. Secondly, we have not taken interactions into account, although they are definitely present in some of the studies. For example, in NW, GLS by far outperforms OLS in models without parameter constraints (an average reduction of 199.7 in the Theil inequality coefficient) whereas in models with parameter constraints this is not the case. Thirdly, the measures of prediction accuracy are not the same in all studies. To account for such differences, using a multiplicative specification between Y and the independent variables could be more appropriate. As table 2 indicates however, this does not basically change the interpretation of the analysis of variance results. Finally, other dummy variables could have been introduced as well. For example a dummy for specifications with no constant term in the case of the attraction model or equal constant terms across brands in the case of the linear and multiplication models.

insert table 2 about here

In any event, the results of the studies are very much at variance. In the subsequent sections we will try to provide at least a partial explanation. The result will be a set of guidelines for the model builder that may be of some assistance in the analysis of market share relationships.

SPECIFICATION

The specifications used in the four papers are:

$$\text{LIN} \qquad m_{jt} = \alpha_j + \sum_{k=1}^{K} \beta_{jk} X^*_{jkt} + \varepsilon_{jt} \tag{1}$$

$$\text{MULT} \qquad m_{jt} = \alpha_j \prod_{k=1}^{K} (X^*_{jkt})^{\beta_{jk}} \; e^{\varepsilon_{jt}} \tag{2}$$

$$\text{ATTR} \qquad m_{jt} = A_{jt} / (\sum_{i=1}^{N} A_{it}) \tag{3}$$

$$\text{where} \quad A_{jt} = \alpha_j \prod_{k=1}^{K} (X_{jkt})^{\beta_{jk}} \; e^{\varepsilon_{jt}} \tag{4}$$

and m_{jt} is market share (brand j, period t), X_{jkt} is the value of the k^{th} explanatory variable, X^*_{jkt} is the k^{th} variable expressed in share form, ε_{jt} is the random disturbance term, α_j and β_{jk} are the parameters, K, N and T are the number of variables, brands and time periods respectively.

No parameter restrictions will be referred to as "npr", or "extended", $\beta_{jk} = \beta_k$ for all j and k will be called "simple",

$\alpha_j = \alpha$ for all j will be given the symbol "NI" (= no intercept)[3]. When the parameters are only partially constrained we will call the corresponding model "inter" (= intermediate).

In this specification section we will address two points. First, managerial meaningfulness in relation to logical consistency and heterogeneity in the response parameters; and second, restrictions on the constant term.

Managerial meaningfulness, logical consistency and heterogeneity in the response parameters

An important issue in the discussions on market share models has been the incorporation of prior (sum and range) constraints into the models. Thus it has been demonstrated that linear market share models are sum-constrained if the response parameters are homogeneous and if the explanatory variables are themselves sum-constrained (Koehler and Wildt (1981); Weverbergh, Naert and Bultez (1981)). The major disadvantage is that in the more commonly used models the sum constraint is only met at the expense of having to assume equal effectiveness of, say advertising, for all brands. This looks to many as very restrictive. Thus, apparently, there is a trade off between two aspects of managerial meaningfulness in using linear specifications. On the one hand managers, especially if they have never worked with models before, would be surprised to see market share models not summing to one.[4] On the other hand, no marketing manager will a priori accept the assumption that the effectiveness of any given marketing instrument is the same across brands.

It should be recognised that the contradiction is only apparent. As has been shown elsewhere, the linear model can in fact accomodate varying degrees of heterogeneity, while preserving sum (but not range) constraints.[5] Full heterogeneity is achieved when all exogeneous variables appear in all equations.[6] Thus, in principle at least, linear models can be both sum-constrained and allow for differences in effectiveness across brands. The price to pay is having a large number of parameters, with the associated problems of loss of degrees of freedom, multicollinearity etc.

One of the nice characteristics of the attraction model is that logical consistency, both in terms of sum and range constraints, is preserved, whatever the degree of heterogeneity desired, and this in a straightforward way.

Apart from this, a somewhat neglected advantage of the attraction model is its superiority with respect to modelling the characteristics of the disturbance term: because of the generalized logistic transformation, the attraction model is in better accordance with the standard assumptions of least squares estimation techniques. Indeed, linear and multiplicative models conflict with the assumption that the error terms should be identically, and independently distributed within each brand, even when allowing for contemporaneous covariances. In general the theoretical distribution will vary according to the market share, and be more asymmetrical as market shares come closer to zero. This may pose a problem, with respect to testing and levels of significance in the GNS-paper for instance, where average market share is only about 3 per cent.

With regard to managerial meaningfulness, it should be recognised that the problem is not just one of choosing between a general and fully constrained model. The first problem is one of defining the relevant market. Let us take the example of the dry cereals market studied in GNS. They mention that in their study over 140 different brands of cereals were recorded by the panel members. In order to avoid too many zero values for advertising, data were aggregated to 29 brands. Even then we might wonder whether a manager of any one of the brands looks at that many competing products separately, in determining his marketing strategy. Thus from a managerial point of view, further aggregation may be desirable. It is hard to assume that all these brands are in direct competition in all cases: regional differences, distributional characteristics should enter into the picture to obtain a reasoned way of aggregating the brands. If a brand manager considers only three other brands as direct competitors, it may be meaningful to consider a five brand market: our brand, the three major competitors, and the 'competitive fringe' combined. Introducing this combined brand then amounts to imposing homogeneity for the brands concerned, while the possibility for heterogeneous coefficients for the main competitors remains.

Restrictions on the constant term

One of the models considered by NW is ATTR: simple, that is $\beta_{jk} = \beta_k$ for all brands and for each instrument. Brands differ only in response then by the α_j. Both BdK and GNS consider an even more constrained version of the attraction model, namely one in which also the intercepts (α_j) are assumed to be homogeneous across brands. BdK call this ATTR: simple, contrary to our definition. In GNS the meaning is clear since they refer to this case by means of ATTR: simple-NI . It should be obvious that this model is an overly extreme form of constraining the parameters. It indeed requires that brands in no way differ from each other, since under these restrictions, equality of the marketing instruments implies (in equilibrium) equal market shares. It should further be observed that ATTR: simple-NI, is not comparable to any of the multiplicative specifications in BdK and GNS and linear specifications in GNS. Comparability would require analogous restrictions to be imposed on those models as well. Arguing against the attraction model because of the meaningless estimation and/or prediction results of ATTR: simple-NI is therefore unjustified.

With respect to functional forms NI-models, whether simple or extended, are also more sensitive to specification errors. Without intercept, its effect will be partly taken up by the coefficients of the explanatory variables. In that respect, we seem to have set a questionable example in NW.

In any case, we would argue against NI-models that at the same time impose equality of response coefficients across brands. Such models allow for no brand differences at all and therefore lack face validity with respect to specification.

ESTIMATION

In this section we will comment on the data, the zero value problem and the covariance matrix of the error terms.

Data

One of the conditions for obtaining reliable parameter estimates is sufficient variation in explanatory variables. Variability of the data is only reported by BdK. As in many empirical studies of oligopolistic markets, price shows very little variation, and so does distribution, at least when variation is measured within brands. It is not surprising then that one experiences difficulties - such as incorrect signs - in estimation. If the main source of variation in an instrument is across brands, heterogeneity will be difficult to measure, provided constant terms are allowed to vary across brands.

As a result, specifications that based on prior reasoning should contain a number of heterogeneous parameters, may have to be revised, because the limitations of the data are such that separate response parameters by brand cannot be estimated. From an empirical finding that specifications with parameter restrictions perform best one may then not necessarily conclude that response parameters across brands are indeed roughly equal. Instead, the conclusion could also be that heterogeneity cannot be assessed because of limited variation in the explanatory variables. We may add that the latter was certainly not the case in NW and more in particular in the first data set. Market shares and marketing variables varied substantially over the observation period, yet parameter constrained models performed much better than others.

An additional comment on the data is the construction of the distribution index in GNS. Distribution is a dummy variable that takes on the value of one if a purchase is recorded by at least one panel member, the value of zero if no member of the panel made a purchase of the brand in the particular month. A priori this is not a very good way to measure distribution, since it reverses causality and may be source of spurious correlation.[7] The authors may not be very happy with that definition themselves, but they consider it necessary for proper modelling of new brands.[8] They indicate that their regression results were quite insensitive to the inclusion of the distribution variable. We should add, however, that in the extended version of GNS 20 of the 29 distribution coefficients are statistically significant (5 signs are incorrect). Their way of treating distribution also leads to the question - more in particular in extended models - whether for all brands there was a sufficient number of zero values for distribution to differentiate this variable from the brand dummy.

Zero values

When working with monthly or bimonthly data, zero values for some observations is not uncommon. For example, as indicated above, GNS aggregate to 29 brands, among other reasons, to avoid having zero values in the advertising variable. Looking at figure 1 in BdK we notice that advertising share for brand A in market 1 is equal to zero in 6 out of the 28 observations. Such zero values pose problems in the multiplicative and attraction models where logarithmic transformations are used in the linearization procedures.

In applied econometrics it has been common practice to replace all zeros by one, or to add one to the whole observation vector. That this is done without much questioning is exemplified by Snedecor and Cochran, who spend just one line on the issue: "If some 0 values of x occur log(x+1) is often used" (Snedecor and Cochran 1967, p. 329). In some cases it is obvious that replacing zeros by ones is too drastic a manipulation of the data. For example when variables are expressed in shares replacing zero by one would clearly not be permissible, since this would imply replacing an observation with zero per cent market share by one hundred per cent. In such cases the solution has been to take an arbitrary small amount, relative to the order of the other data (e.g. .01).

It is insufficiently realised, however, that this choice could affect the results critically. Naert and Weverbergh (1980) related monthly sales (expressed in thousands of pounds) and monthly advertising expenditures (in thousands of dollars) for a packaged food product. The data base contained 36 observations. In 2 of the 36 months advertising was zero. A linear model with all advertising data resulted in a coefficient of determination of .60 . The multiplicative model using advertising expenses plus one gave an R^2 (on the underlying structural equation, and not on the transformed equation) of .49 . Yet a cross plot of the sales-advertising data clearly showed decreasing returns to scale. They then introduced an additive parameter k to advertising and examined the sensitivity of the estimation results to the value of k . The degree of fit varied from R^2 = .18 for k = .001 (the smallest value tried out) to a maximum of R^2 = .77 for k = 141.

The sensitivity may not always be as dramatic, but the example clearly demonstrates the inherent lack of robustness of multiplicative models to zero or close to zero values for some of the explanatory variables. The comparison of multiplicative (and attraction models with multiplicative attraction functions) and linear models may suffer because of this aspect.

One may of course also argue that a multiplicative or attraction model with, for example, advertising as one of the explanatory variables is inconsistent, because in reality zero advertising does not need to imply zero market share. Nakanishi (1972) and Nakanishi and Cooper (1982) have proposed to replace X_{jkt} by $e^{X_{jkt}}$.[9] Or in cases where the X_{jkt} can take on negative values, Cooper and Nakanishi (1983) propose the zeta-squared transformation.[10]

<u>Covariance structure of the disturbance terms</u>[11]

We have recommended in NW against iterative GLS. This recommendation is followed by BdK, GNS and LR. It is worth emphasizing, however, that this assumes reasonable first round estimates. In logically consistent models with a singular covariance matrix, applying OLS on all brands implies homoscedastic disturbances across equations and equal contemporaneous covariances. In the absence of information to the contrary this is easier to justify than, for example, performing OLS on N-1 brands, which implies a scalar covariance matrix for the brands retained. In the latter case, the brand that is deleted is assumed to be the sole source of contemporaneous correlation. Starting from there might necessitate further iterations.

In the case of the attraction model, there is no need at the level of the structural form to assume contemporaneous correlation. The linearization procedure used will result in a specific structure for the covariance matrix for the linearized form. Depending on the procedure used (logistic transformation or Nakanishi-transformation e.g.), OLS on all equations may be an acceptable starting point or not. In the latter case either more iterations or starting with GLS from the implied covariance matrix may be indicated.

In conclusion, it is our conjecture that the usefulness of IGLS is small when an adequate initial step is taken.

From the above discussion it follows that IGLS cannot in general be discarded. If insufficient attention has been given to assuming an adequate initial step, carrying out one or more additional iterations may pay.

VALIDITY

In this section the following topics are addressed: testing for heterogeneity in the parameters; the U-coefficient in relation to the level of parameterization, and normalization.

Testing for heterogeneity in the parameters

BdK, GNS and LR emphasize the importance of a careful analysis of the degree of heterogeneity in the parameters. In NW the problem has not received the attention it generally deserves indeed. For our data, face validity considerations and clear superiority in terms of forecast accuracy lead to the obvious choice of parameter-restricted models. BdK and LR plead for formal statistical testing of the parameter restrictions. While this is certainly desirable, we would like to emphasize the following points. First, even if on the basis of an F-test one has to reject the hypothesis of homogeneous parameters, all problems are not solved: often a considerable number of heterogeneous parameters will not be statistically significant, let alone be significantly different from each other. Let us illustrate on the basis of the GNS study. In the simple model all four parameters (excluding intercepts) are significant at the .05 level, both in OLS and GLS, for all four models. The only problem is with respect to distribution in the NI-model. Given the problem with the NI-specification referred to above, we should, however, not pay any attention to the NI-results. GNS do not report equality of parameter tests, but their forecasting results show clear superiority of extended models over constrained ones. However, in these extended models, out of a total of 116 parameters, only 32, 23 and 33 in LIN, MULT and ATTR respectively, are both correct in sign and significant. As a result GNS also examined intermediate levels of parameterization. They reduced the number of price, advertising and distribution coefficients to three each, but found a decrease in forecasting accuracy, not just as compared to the extended models but even relative to the simple ones. In the case of NW similar problems with respect to levels of significance were encountered, although imposing restrictions did have a positive impact on prediction power. This shows that the procedure for imposing constraints has not yet been adequately resolved, whereas the results show that the need for such procedures does indeed exist (see also the discussion on U below).

A related point concerns a remark made by Reuyl (1982) with respect to the data used in LR. The original data base contained 15 brands of cigarettes (14 major brands and 1 aggregate of other brands). Applying an F-test the hypothesis of homogeneous parameters had to be rejected. In LR they consider 4 brands (3 plus one aggregate brand). In the latter case the homogeneity hypothesis is accepted. This is an interesting finding which may be explained by the fact that aggregating brands (e.g. those marketed by the same producer, or different varieties of the same brand) averages out some of the heterogeneity. Finding this pattern indicates that, at least in principle, the aggregation is unjustified.

We say "in principle" since the level of aggregation (and therefore of parameterization) is not just a statistical issue. As argued earlier, market share models should in the first place yield meaningful results, that is, they should have face validity. When looked at on an a priori basis, i.e. at the specification stage, less restricted models may be favored. After estimation, however, more constrained specifications will often turn out to be more appropriate from a managerial point of view. Let us illustrate by a result reported by BdK. For brand C, the distribution coefficients obtained in GLS are the following.[12]

LIN	:	.076	not significant
MULT	:	-.230	significant at the .05 level
ATTR-NI	:	1.863	significant at the .01 level

In such a case one might feel more comfortable with the constrained estimates, which are all significant and of the correct sign.

There is a final point to be noted with respect to heterogeneity, which is most striking when dealing with the linear model. Competitive interaction in this case is introduced by expressing the variables either in shares (as in the papers reviewed here) or in relative terms. It can be demonstrated that the assumption of homogeneity is in fact incompatible with this form of accounting for competition.[13]

Concluding, the question of heterogeneity versus homogeneity does pose rather intricate problems. It is not surprising that on this issue the results of BdK, GNS and LR seem most at variance with our previous results. It is indeed with respect to this issue that sample differences will show up most strongly. In addition the comparison of measures of accuracy across different levels of parameterization is not as straightforward as one often thinks as the discussion below will show.

U as a function of the level of parameterization

All studies took as predictive accuracy (or in fact inaccuracy) measures variants of Theil's inequality coefficient (Theil 1966).

BdK use

$$U = \sqrt{MSE} \; / \; \sqrt{MSM} \qquad (5)$$

with MSE the mean square prediction error and MSM the average of squared market shares. In the three other papers the denominator of U also contains a term with predicted shares.

Figures 1a to 1c show the expected shapes of U as a function of the level of parameterization.

insert figure 1 about here

As in GNS we consider three levels: simple (S), intermediate (I) and extended (E). This is of course a highly simplified representation since there is a whole spectrum of intermediate levels between the simple and extended versions. In figure 1a the U-value increases monotonically with the level of parameterization. It is the case where homogeneous parameter models dominate the heterogeneous models. Figure 1b is just the opposite and corresponds to a clear demonstration of an unquestionable heterogeneity in the parameters. Figure 1c indicates that partial relaxation of the parameter constraints leads to the best forecasts. Logically, the possibility of shape 1d has to be dismissed. It indeed implies that partial relaxation of the homogeneity assumption makes forecasts worse, yet further relaxation leads to increased forecasting accuracy. We were therefore quite puzzled by the fact that figure 1d is the shape obtained by GNS. Considering that the naive models (market share constant or equal to lagged market share) perform worse than the extended models, but better than some of the simple models, led us to the hypothesis that the problem resides in the heterogeneity of the lagged share coefficients. That is, for the GNS data we expect models with fixed coefficients for price, advertising and distribution and partly or fully heterogeneous lagged shares coefficients to produce lower U values than either the simple or fully extended models. Whatever the case, it reinforces our conclusion that sound procedures for relaxation are needed.

Normalization

In NW ex post normalization systematically and often substantially improved forecasting accuracy. In GNS it has little effect. They indicate that this is to be expected since $\Sigma\hat{m}_{jt}$ does not deviate by more than .003 from 1 . In LR normalization makes forecast accuracy slightly worse, whereas in BdK normalization has a strong - sometimes positive and sometimes negative - effect on U . These contradictory results may lead us to discard ex post normalization since its effect on U seems unpredictable. Rather than doing this we should question the normalization procedure that was used in BdK, GNS, LR and NW.

Taking

$$\hat{\hat{m}}_{jt} = \hat{m}_{jt} - \delta_t \tag{7}$$

and

$$\delta_t = (\sum_{j=1}^{N} \hat{m}_{jt} - 1)/N \tag{8}$$

normalized predictions can never be worse than non-normalized ones if U is defined as in equation (5).[14]

While this normalization procedure can never make forecasting accuracy worse, it has some problems of its own. The share of each brand is adjusted by the same amount . The adjustment may therefore be large for some of the minor brands. In addition some of the $\hat{\hat{m}}_{jt}$ can be negative even when none of the $\hat{m}_{jt}$ is.

CONCLUSIONS

Given the divergence in the conclusions of the 4 papers the reader will not expect to be given a cook book as to what functional form with what level of parameterization and what estimation procedure is appropriate when.

What we can do is to provide the model builder with a set of guidelines or checkpoints that may assist him in studying market share functions. Many of these may sound trivial to the person more experienced in this area of model building. Others may pick up a few helpful hints.

For the sake of clarity we will summarize the different points under the headings of specification, estimation and validation. The discussion in the paper should of course have made clear that these stages in the model building process are not purely sequential but that they interact.

Specification

On theoretical grounds the attraction model has a number of merits: it is sum and range constrained; heterogeneity can be introduced without destroying the sum and range properties; the disturbance term modelling is superior; and at the level of the structural form there is no need to assume contemporaneous correlation of the disturbance terms.

Attraction models with no intercept and linear or multiplicative models with the same constant term across brands should not at the same time impose equality of response coefficients across brands. The resulting model is indeed one that does not allow for any brand difference, a constraint that lacks face validity.

Aggregation of brands, and thus the appropriate level of parameterization should at least in part be based on managerial considerations. Extreme disaggregation, in terms of maintaining a large number of brands separately, may not be very appropriate from a model user's point of view.

Estimation

In the first place we should recognize the interaction between specification and estimation due to limitations in the data. When data show limited variation in some of the variables, estimating heterogeneous parameters for these variables will become harder. The more limited the number of observations, the harder it becomes to estimate a large number of parameters. As such, finding the best performance for parameter restricted models, may be as much a result of characteristics of the data sample as it may be of underlying properties of the market of product involved.

In multiplicative and attraction models zero values cause problems because estimation involves logarithmic transformations.

Several methods exist to alleviate the problem, such as, replacing the new variable X by X+k, where k is a parameter, or replace X by e^X, or use a zeta-transformation etc. Here again, estimation and specification interact.

Special attention must be given to new brands. For some marketing instruments the response parameter is likely to be time dependent at least in the early phase of the brand's life cycle.

GLS is unlikely to produce improvements if the number of brands is very large. In that case most of contemporaneous covariances are likely to be small, and since there are many, estimation problems are most likely to occur.

While IGLS estimation does not in general lead to improved forecasting accuracy, in cases where first round estimates are based on an (often implicit) covariance structure that is not very plausible, going through one or a few additional rounds of GLS estimation may pay.

Validity

Statistical testing for heterogeneity in the parameters is helpful in deciding whether or not to impose parameter restrictions.

Statistical testing should, however, not be the sole criterion. Face validity considerations, such as, wanting parameters with correct signs should certainly supplement the statistical analysis.

The question of what if any parameter constraints should be imposed should be tackled in a systematic and reasoned way. Discovering a pattern of U as a function of level of parameterization as shown in figure 1d should motivate the model builder to look for other ways of partially constraining the set of parameters.

Ex post multiplicative normalization does not systematically lead to improved overall forecasting accuracy. Ex post additive normalization following the procedure proposed in this paper does, but may be less desirable for some (more in particular smaller) brands. It may be worthwhile to look for even better procedures.

In many cases predictive accuracy is assessed in the following way: a sample is split in an estimation and validation part. One period ahead forecasts are made and forecasting accuracy is measured on the validation sample. At least two directions for improvement can be recommended: one, in order to maintain the largest possible number of observations for estimation, Jackknifing procedures can be used.[15] Secondly, as suggested by GNS it may be more appropriate to look at multiperiod rather than one-period ahead forecasting. Indeed, discriminating models on their capacity to predict market shares one period ahead may be less meaningful since many competing models (including naive extrapolation type ones) are likely to do very well.

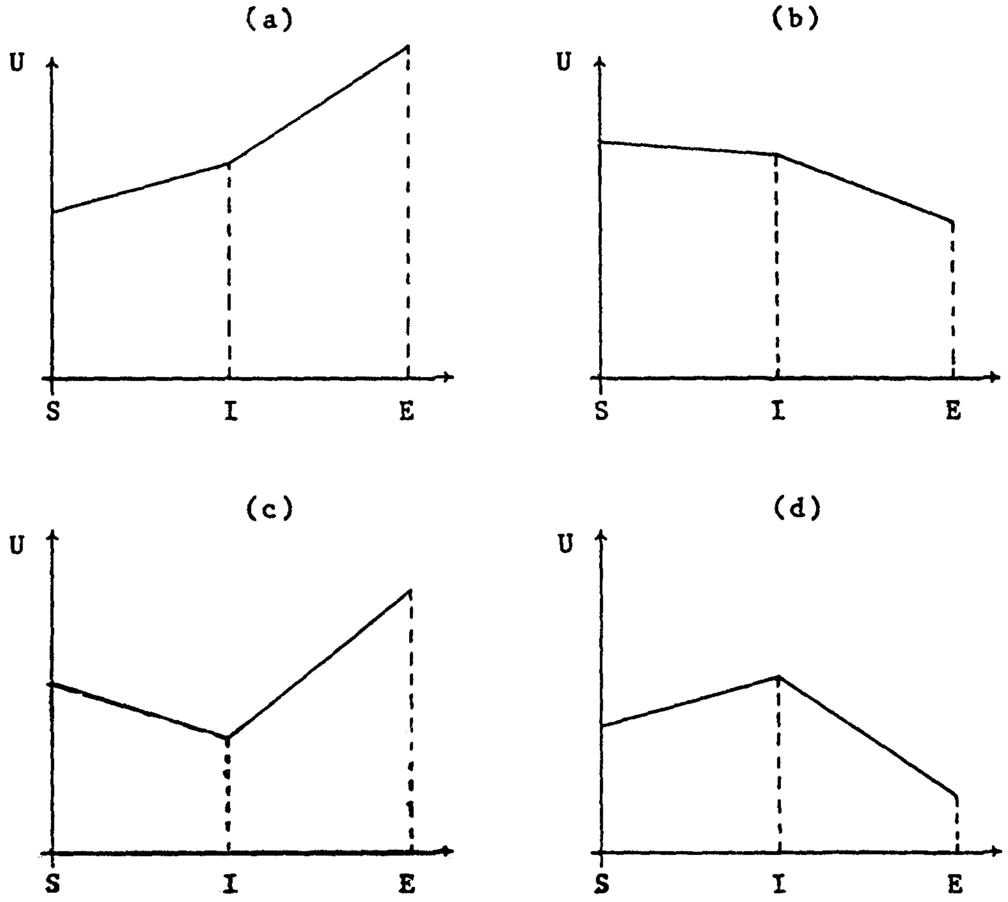
Table 1 : linear regression of forecasting inaccuracy

	ALL OBSERVATIONS (R^2 = .76)		NW (R^2 = .80)		BdK/GNS/LR (R^2 = .84)	
Variables	Estimate	t-statistic	Estimate	t-statistic	Estimate	t-statistic
CONSTANT	175	3.4	203	2.6	630	15.0
NW2	660	13.9	669	12.3	-	-
BdK1	468	8.3	-	-	-	-
BdK2	357	6.3	-	-	-111	-2.7
BdK3	682	12.1	-	-	215	5.2
GNS	449	7.6	-	-	-38	-0.8
LR	-105	-1.9	-	-	-580	-14.4
MULT	5	0.3	-19	-5.3	224	0.7
ATT	-148	-3.0	-372	-4.2	2	0.1
GLS	-3	-0.1	-65	-1.0	18	0.7
IGLS	13	0.2	-19	-0.3	-	-
NORM	-58	-1.7	-13	-1.9	-19	-0.6
PR1	103	1.7	360	3.3	-64	-1.2
PR2	107	3.2	235	3.9	31	1.1

Table 2 : multiplicative regression of forecasting inaccuracy

	ALL OBSERVATIONS (R^2 = .91)		NW (R^2 = .90)		BdK/GNS/LR (R^2 = .97)	
Variables	Estimate	t-statistic	Estimate	t-statistic	Estimate	t-statistic
CONSTANT	5.19	63.4	5.29	45.3	6.46	117.0
NW2	1.53	20.3	1.55	19.0	-	-
BdK1	1.33	14.5	-	-	-	-
BdK2	1.13	12.5	-	-	-0.20	-3.7
BdK3	1.58	17.3	-	-	0.25	4.7
GNS	1.28	13.6	-	-	-0.73	-1.3
LR	-0.86	-9.8	-	-	-2.20	-41.7
MULT	-0.02	-0.3	-0.08	-0.9	0.03	0.7
ATT	-0.28	-3.6	-0.73	-1.3	0.02	0.4
GLS	-0.05	-1.1	-0.24	-2.4	0.02	0.5
IGLS	-0.10	-1.1	-0.19	-1.9	-	-
NORM	-0.07	-1.2	-0.16	-1.6	-1.39	-0.3
PR1	0.14	1.4	0.50	3.0	-0.09	-1.3
PR2	0.15	2.8	0.39	4.4	0.01	0.1

Figure 1 : U as a function of the level of parameterization



FOOTNOTES

1 Due to space limitations our discussion will at times be very parsimonious. The reader interested in a more complete discussion can obtain the working paper version by writing to Philippe Naert at INSEAD, Boulevard de Constance, 77305 Fontainebleau, France, or Marcel Weverbergh at UFSIA, Prinsstraat 13, 2000 Antwerpen, Belgium.

2 For reasons of comparability and consistency, only the one period ahead forecasts of GNS were used.

3 No intercept in the attraction model is logically equivalent to equality of the constant term across brands in the linear and multiplicative models.

4 It has been argued that in many cases the violation of the sum constraint may be so small that it does not warrant special attention. Counterargument would be that in some cases the violation may be significant, e.g. when the relevant range of variation in the explanatory variables is large. In addition, the issue of logical consistency is linked to estimation efficiency.

5 How this can be done is shown in Appendix A of the working paper version of this paper. Space limitations made printing the Appendix as part of the published paper impossible.

6 Applications of this can, for example, be found in studies of systems of demand functions in micro economics. See, for example, Barten (1977).

7 We should add that Brandt (1976) in commenting on a paper by Young and Young (1975) has argued that an observation should in fact be dropped when it shows zero values for both dependent and independent variables. Applied to the GNS data the distribution dummy would of course lose any meaning, since it would then take on a value of one for all the observations. See also the discussion on "zero values" below.

8 We should be aware that new brand introductions pose special problems because of the time varying nature of some of the response parameters. Treating them in the same way as existing brands may lead to poor prediction accuracy. For some evidence to that effect see, for example, Gijsbrechts and Naert (1985).

9 The interpretation of the coefficients may then, however, become more complicated. In the attraction model, for example, the direct elasticity $\eta_{m_{jt},X_{jkt}}$ becomes $\beta_{jk} X_{jkt}(1-m_{jt})$.

10 The transformation may again complicate the economic interpretation of the coefficients.

11 A detailed analysis of this section is given in Appendix B of the working paper version.

12 Values and significance levels taken from Brodie and de Kluyver (1982).

13 This is again related to the logical consistency issue. For a demonstration of this statement, we again refer to Appendix A of the working paper version.

14 It then suffices to show that $\Sigma_j \hat{\varepsilon}^2_{jt} \geqslant \Sigma_j \hat{\hat{\varepsilon}}^2_{jt}$, where $\hat{\varepsilon}_{jt} = m_{jt} - \hat{m}_{jt}$ and $\hat{\hat{\varepsilon}}_{jt} = m_{jt} - \hat{\hat{m}}_{jt}$. From (7) it follows that $\hat{\varepsilon}_{jt} = \hat{\hat{\varepsilon}}_{jt} - \delta_t$. Squaring the latter expression and summing over j gives $\Sigma_j \hat{\varepsilon}^2_{jt} = \Sigma_j \hat{\hat{\varepsilon}}^2_{jt} - 2\,\delta_t(\Sigma_j \hat{\hat{\varepsilon}}_{jt}) + N\delta^2_t$. Since by construction $\Sigma_j \hat{\hat{\varepsilon}}_{jt} = 0$ the inequality above always holds.

15 See, for example, Efron (1979).

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